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VOLUME THE THIRD

SESSION 1909-10

PART I

GENERAL REPORTS

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SECTION FOR THE STUDY OF DISEASE IN CHILDREN

CLINICAL SECTION DERMATOLOGICAL SECTION

ELECTRO-THERAPEUTICAL SECTION



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VOLUME THE THIRD

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GENERAL REPORTS

DISCUSSION ON "VACCINE THERAPY: ITS ADMINISTRATION, VALUE, AND LIMITATIONS"



LONDON
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1910

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Royal Society of Medicine

May 23, 1910.

Sir William S. Church, K.C.B., President, in the Chair.

Vaccine Therapy: its Administration, Value, and Limitations.

An Address Introductory to a Discussion on the Subject.

By Sir Almroth E. Wright, M.D., F.R.S.

I do not conceal from myself that in opening this debate I am undertaking a task of quite exceptional difficulty. I have to set out to you such new points in connexion with vaccine therapy as may seem to me to deserve attention. I have to attempt to take the measure of the achievements of this new therapeutic method. I have to discuss its limitations. And, above all, I desire to suggest to you in connexion with all these subject-matters certain canons of criticism.

Before I embark upon these tasks I may perhaps with advantage consider for a moment the rationale of vaccine therapy. The fundamental principle of vaccine therapy, as I conceive of it, is to exploit in the interest of the infected tissues the unexercised immunizing capacities of the uninfected tissues. Let me illustrate my meaning. I will take the case of a localized infection. We have here a condition in which the invading microbes are holding their own or getting the upper hand, and our object must be to turn the tables upon them. For this purpose we inoculate into some other part of the body microbes which are similar to those which the patient has to combat. Such inoculation is not, however, as the unthoughtful immediately assumed, a mere replica of the original infection. There are two points of difference. *First*, the

microbes in the vaccine have been devitalized, so that their multiplication is impossible; *secondly*, the dose of vaccine is so regulated that the organism of the patient must inevitably win at the site of inoculation. Now a victory such as this is won by the elaboration of antibacterial substances, and these are generated in such a case on a scale which is more than adequate to bring about a destruction of the bacterial elements which have been incorporated. The surplus of specific antibacterial substances which have thus been elaborated will now find its way by the channel of the blood to the focus of infection. There it will bring aid to the defensive forces of the organism which before were ineffectually combating the invading microbes. The victory which the uninfected tissues have won over the microbes of the vaccine will in this way lead up to a victory of the infected tissues over the microbes they have to combat.

A therapeutic method which proceeds on this new-found principle must, of course, make new demands. It will be well to realize what they are.

The medical man who has recourse to vaccine therapy ought to have a familiar acquaintance with the microbes which affect the human body. He ought to appreciate the fact that vaccines owe their efficacy to the reaction they set up in the tissues, and not to any action exerted directly by the vaccine upon the invading microbe. He ought to have mastered the general principles of immunization. He ought to know in connexion with each vaccine the *minimum effective dose*—i.e., the dose which gives the minimum immunizing reaction without any intervening negative phase—and the *medium*, or average, dose—i.e., the dose that gives, after a negative phase, a more powerful immunizing reaction. He ought to know the general conditions which affect the sensibility of the organism. He ought to understand how to adjust the dose to the requirements of the individual patient. And he ought to have a knowledge of the conditions which obtain in the focus of infection, and of the methods of circumventing the difficulties which are introduced by these conditions.

If vaccine therapy demands all this of the medical man, it is clear that the discussion of vaccine therapy must inevitably lead on to the opening up of the question as to whether the medical practitioner must of necessity be a bacteriologist.

If, in the course of this discussion, it becomes clear that vaccine therapy is more effective than any other method of treating bacterial disease, and if it is conceded that it holds out in very many cases the

only real prospect of advantage to the patient, such concession will in reality be tantamount to a declaration that applied bacteriology is the essential and indispensable part of medicine, and that the practice of medicine must be reorganized upon that basis.

If, on the contrary, the conclusion is arrived at that vaccine therapy has, as compared with other methods, only a limited utility and a limited application, this will be tantamount to a declaration that the reorganization of medicine on the basis of bacteriology may still a little longer be postponed. Such reorganization, of course, becomes inevitable as soon as an effective application of bacteriology has been made.

Inasmuch as these are, if I understand the matter aright, the real issues which lie behind this debate, I think it will not be unprofitable if, before dealing specifically with vaccine therapy, I pass in rapid review the recent history of medicine and of its relations to bacteriology.

As one looks back on the history of medicine, and recognizes that the successive applications of bacteriology stand out as great landmarks, one often wonders that medicine was not long ago swept along irresistibly into the channel of bacteriology. One wonders, for instance, in connexion with the advent of antiseptic surgery why this triumphantly successful application of bacteriology was not immediately followed up by a serious attempt on the part of our profession to study and turn to account this new science. Perhaps it was that the science of bacteriology was then quite in its infancy, and perhaps that the thoughts of our profession were here too engrossed in experimentation with different antiseptics. But the dominant factor in the situation was, I feel sure, the fact that antiseptic procedures could be successfully carried out without any knowledge of bacteriology. Seeing that the action of antiseptics is exerted upon all microbes without distinction of kind, the study of bacteriology appeared to the surgeon to be merely a work of supererogation.

When, following in his wake, the physician embarked upon the treatment of bacterial diseases by antiseptics, his thought followed upon the same road. He saw in antiseptics therapeutic agents which would be effective indiscriminately against all microbes, and seeing this, he, like the surgeon, held himself absolved from any study of bacteriology.

In the meanwhile the science was advancing with giant strides. One by one the pathogenetic microbes with which we are now acquainted were discovered and their rôle in connexion with the production of disease was elucidated. At the same time methods for identifying and making cultivations of each of these species of microbes were gradually

elaborated. Then followed the discovery of agglutination, and with this the recognition of the possibility of diagnosing the nature of a bacterial infection by testing the properties of the blood fluids. After this came the discovery of the "deflection of complement test" and the discovery of the opsonic power of the blood, and with this another and much greater extension of the method of serum diagnosis. Finally came the recognition of the possibility of inducing auto-inoculation for diagnostic uses, and with this the serum tests which previously gave assistance only when spontaneous auto-inoculations were taking place became applicable also in connexion with strictly localized infections.

But the clinician has not yet been prevailed upon to reconsider his position. He still—in face of these discoveries—claims to be absolved from a study of bacteriology; and he is still bemused with the idea that the final appeal must always be to himself and to his methods of physical diagnosis.

Let me, in connexion with this fixed idea, invite you to consider the following points:—

(1) There was undoubtedly a time when the verdict of the pure clinician on a question of a diagnosis was incontestable. That was when the disease could be recognized and identified only by its clinical symptoms. Diphtheria then meant a condition where a particular kind of false membrane appeared on the throat; typhoid fever, a fever where the temperature ran a particular course; and phthisis, a disease in which certain noises could be heard down a stethoscope. With the discovery of the specific causes of these diseases came a quietus to all this. Diphtheria now means an invasion of the throat by diphtheria bacilli; typhoid fever an invasion of the blood by the typhoid bacillus; and phthisis an invasion of the lungs by the tubercle bacillus. It is not, at this hour of the day, arguable that the verdict of a bacteriologist stands in need of confirmation from a clinician. In blunt language, "the boot is on the other foot."

Let me pass to a further point:—

(2) While it must be conceded that clinical methods do furnish in many cases a bacteriological diagnosis, this is reached only by a process of inference, and the chain of inference is in this case made up of a series of open links. I should not have thought it necessary to emphasize this so obvious fact were it not that I was recently assured by a reputable clinician that it had never crossed his thought that dullness over the apex of the lung did not supply proof of tubercular infection.

And there is yet one further important point:—

(3) A diagnosis of the nature of a bacterial infection which is based upon the physical signs comes short not only in the respect that it can never be wholly trustworthy, but also in the respect that it must almost always be belated. For physical signs such as can be appreciated by clinical methods can, in the nature of things, be elicited only after comparatively gross physical damage has been done. I need not insist that bacteriological methods have from this point of view a great superiority. Pathogenetic microbes may, as you know, often be found in the secretions or the blood long before a diagnosis can be arrived at by clinical methods; and when methods of direct bacterial diagnosis give negative results, we can nearly always by inferential methods—by the opsonic index, or the agglutination test, or the deflection-of-complement test—arrive at the nature of the infection long before its clinical features have become distinctive.

While all this was, and is, uncongenial to the pure clinician, he did not fail to appreciate that there could be no question of ignoring bacteriological discoveries which were of fundamental importance for medicine. He was therefore face to face with the problem as to how to make provision for the carrying out of the necessary bacteriological tests and examinations. The proper way of confronting the situation would, of course, have been to look the future fairly and squarely in the face, and to enact that in the future no one should qualify as a medical man without some elementary knowledge of bacteriology, and that no one should proceed to any of the higher qualifications without a thorough training in this science.

If that course had been taken even a few years ago—and it has not yet been taken—the younger rank and file of the profession and the younger generation of clinical teachers would now be abreast of any work which might fall to them in connexion with vaccine therapy.

There was, however, one way of escape out of the situation. This was to delegate all bacteriological work in hospitals to a special bacteriological department, and in private practice to such public or private laboratories as might be disposed to undertake it. For a time—as in the case where the Merovingian Kings delegated all their functions to the Mayors of the Palace—everything smiled upon this arrangement. In particular the next great advance in bacterial therapeutics—I refer, of course, to the serum treatment of diphtheria—seemed to fit very harmoniously into this scheme. The medical man who had divested himself of all bacteriological work found here, to his joy, that it would be practicable for him to get his diagnosis from one bacteriological laboratory and his remedy from

another, and that there would still remain for himself a dignified rôle as a middleman between the patient and the bacteriologist. Moreover, this rôle would, it was anticipated, continually grow in importance as serum therapy found new application in connexion with one acute disease after another. The medical man even dreamed dreams of polyvalent sera that would make everything in the nature of minute bacteriological diagnosis superfluous.

Upon this basis the march of bacteriological discovery seemed to promise advantage to the medical profession—advantage in the form of greater therapeutic success—and there would not be as counter-weight any added burden of unaccustomed labour.

The advent of vaccine therapy has disturbed this carefully thought-out scheme, for it has brought home to us that bacteriological work is called for in connexion with nearly every case. And that is a fact which gives, as you will see, quite a new complexion to the proposal that the medical man should depute to others everything that relates to bacteriology. In point of fact we are here brought up against a very fundamental question—the question as to when, and under what circumstances and to what extent, the skilled work of a profession may legitimately be delegated. We may here confine ourselves to the case of the delegation of skilled work in connexion with our own profession.

It will prevent a confusion of issues if we at the outset discriminate between the case where a consultation is held over a patient and the case where a specimen is sent to a laboratory for report. In the former case the consultant is brought face to face with the patient and with the problem which is to be resolved. He is asked to undertake any examination which may seem to him required for the elucidation of the case, and to discuss the whole problem with the practitioner; and the consultant and practitioner can then, as fellow experts, consider exactly where and to what extent "*functional errors*"—concerning which I shall presently have something to say—may be affecting their several findings, and exactly how far these findings may be accepted as assured. And, finally, when, by such collaboration, a decision has been arrived at, the responsibility for any action taken rests upon the shoulders of both.

In the case where a specimen is sent to a bacteriological laboratory for report, we have utterly different conditions. The bacteriologist is not brought face to face with the problem. Instead of being afforded an opportunity of obtaining his own specimen in the way that he judges best, or even an opportunity of suggesting the proper way of approach-

ing the bacteriological investigation, he is tied down to the examination of such specimens as may be submitted to him. Again, the bacteriologist is generally very imperfectly informed with respect to the nature of the problem which is to be resolved. Moreover, when the written bacteriological report comes to hand the practitioner, who has not himself been a laboratory worker, will be quite unable to exercise any expert criticism upon it or to see where fallacies may come in. He will, in the ordinary case, assume that laboratory methods are not subject to fallacy and "functional error," and that the "Yes" of the laboratory means definitely "Yes," that the "No" of the laboratory means definitely "No," and that in the case of a laboratory result which is expressed in terms of figures, those figures must be an absolute mirror of reality. And even if it should so happen that the practitioner to whom the bacteriological report is sent has expert knowledge of laboratory work, he will, in default of personal knowledge of, and opportunity for questioning, the bacteriological worker, be quite unable to evaluate the "functional error," and to assign in accordance with this its proper value to the report.

Having discriminated the case of the consultation which is, as we have seen, really the case of collaboration in skilled work from the case where a specimen is referred to a laboratory for report, we have now to try to arrive at a considered decision upon the morality of that kind of delegation. Three different cases of delegation must be distinguished.

The *first* is the case of a man who, being himself skilled in the whole of the work which he undertakes, has part of it carried out by an assistant, satisfying himself that it has been done in the proper manner and accepting the responsibility for this. It is the case of the delegation of duties by the honorary physician or surgeon to the resident staff of the hospital, by the director of a laboratory to his staff, and by by the practitioner to the nurse.

The *second* is the case of a man who, when invited to undertake work which lies outside the range of his competence, hands over that work and the attaching responsibility to another who has the special kind of skill which is required. This is the ordinary case of a practitioner sending on a case to a specialist.

The *third* is the case of the man whose studies have not covered the whole of his professional work and who arranges to delegate to others that portion of the work which he has omitted to learn, while retaining for himself the responsibility for the whole, along with the higher scale of rewards which goes with that responsibility. You will, I think,

recognize that this is the case of the medical man who, finding that bacteriological work is required for diagnosis and treatment, makes shift, without learning any bacteriology, to depute that work to others while retaining for himself the control of the case.

I have no doubt that you will adjudge the *first* of these forms of delegation to be both legitimate and necessary to the carrying out of work on any large scale; that you will commend the *second* while recognizing that it can only exceptionally be resorted to; and I believe that you will see that the *third* case is not very clearly distinguishable from the case of a layman who should undertake to treat a case on condition of his being permitted to consult by correspondence competent medical authority whenever he might see occasion for doing so.

When I suggest that these cases are comparable, I do so because I want to put this issue before you. If we, as a profession, deprecate treatment by the unqualified on the ground of the dangers which may attach to the treatment of grave cases by the ignorant, can we then refrain from condemning, as perilous to the patient, the treatment of grave bacterial infections by those who are ignorant of bacteriology? And if we as a profession condemn consultation by correspondence on the ground that a trustworthy opinion cannot be based upon medical data which are furnished by an ignorant patient, how then shall we refrain from condemning the system by which a medical man who is ignorant of bacteriology selects the bacteriological data upon which a diagnosis is to be based?

I confess that I myself can find only one answer to that question. But let me assume that the question as to whether this kind of delegation is legitimate is still doubtful. There is then still another way by which we may arrive at a decision. Every tree is known by its fruits. We may therefore inquire into the practical working of the system. I would point out here—for we must judge everywhere by one and the same standard—that if we deprecate the treatment of cases by the unqualified on the ground that such treatment may sometimes be perilous to the patient, and if we condemn consultation by correspondence on the ground that this system would often work badly, we must, on the same basis, condemn the system by which the bacteriologically uninstructed delegate bacteriological work to the laboratories if this system is occasionally perilous to the patient and if it often works to his disadvantage.

I suppose that no one who has any experience of the practical working of this system of delegation has any doubt on this point. Every worker in such a laboratory could tell a tale of opportunities

of diagnosis missed, of misdirected searches, and of quite incredible solecisms committed by distinguished clinicians whose qualifications date back to pre-bacteriological days. It is probable that every worker in a clinical laboratory has frequently been sent blood in a capillary tube with a request that it should be examined microscopically with a view to the detection of pathogenetic microbes. Or he has been sent a specimen of desiccated blood in a capsule with a request to examine for the malaria parasite. Or he has been asked to report on the opsonic index of a charred specimen of blood—the opsonic index being, in the view of the sender, a function which does not stand in need of any further qualification, and a function which is indestructible by heat. Or he has been asked to find the malaria bacillus in a specimen of urine, or has been sent a twenty-four-hour specimen of urine from a case of septicæmia to make a vaccine for the treatment of the case. Or he has exhibited a preparation showing phagocytosis and has been asked by a high clinical authority to say which is the microbe and which is the phagocyte. In fact every bacteriological worker has been asked to place his services at the disposal of medical men who are in everything that appertains to bacteriology more uninstructed than the educated layman.

One ought perhaps here, you think, to accept as a counterpoise to the bad results which accrue from this system of delegation the good results which accrue from it in connexion with the diagnosis of diphtheria from swabs sent to the laboratory, and in connexion with the treatment of this disease by diphtheria antitoxin. I do not for one moment wish to overlook either these or the many other advantages which may have accrued from the establishment of laboratories for clinical bacteriology: but if I take these into consideration, I am, by the same compulsion, obliged to take into consideration also that treatment by the unqualified and consultation by correspondence may also do good. And I put it to you that if we, as a profession, are under an obligation to call attention to the shortcomings of unqualified practice and consultation by correspondence, are we not under the same obligation to exercise censure upon ourselves when we have recourse to a system of delegation which involves us in similar shortcomings?

But let me deal with the suggestion that our present system of delegating bacteriological work, as exemplified in the case of the diagnosis and treatment of diphtheria, is an ideal system. I am very far, indeed, from taking this view of the matter. I regard our present system of dealing with the diagnosis and treatment of diphtheria as essentially defective. It is defective, first, in the respect that it takes

into account only one bacteriological factor—the diphtheria bacillus—and one of its pathological effects—the intoxication by diphtheria toxin. It is defective, secondly, in the respect—and this defect attaches to nearly all our therapeutic efforts—that, instead of adapting itself to the particular requirements of the individual case, our present treatment of diphtheria aims only at securing a high average of successes. I hold it to be a great defect in our methods of diagnosis of diphtheria, that attention is concentrated on the presence or absence of the diphtheria bacillus. We simply close our eyes to the fact that there may be associated with the diphtheria bacillus other pathogenetic microbes, such as streptococcus, whose presence may perhaps involve almost as much danger to life as the diphtheria bacillus itself. This neglect of the associated infections is only a natural outcome of the delegation of bacteriological work to bacteriologists who are out of touch with the case, and of the faulty appreciation of bacteriological data by medical men who are uninstructed in bacteriology.

Nor even if we leave entirely out of sight the question of therapeutics of the associated infections, and confine ourselves to the question of the therapeutics of the diphtheritic infection, would it be possible to applaud the policy of delegating the manufacture of the serum to a bacteriologist who is not in personal charge of diphtheria cases. For the laboratory bacteriologist almost by necessity sets himself, in the manufacture of serum, an ideal which is merely a laboratory ideal. He aims at the production of a serum which will conform to accepted laboratory tests, and, in seeking to achieve the greatest possible antitoxic potency, he leaves out of sight the fact that a diphtheritic infection is something more than an intoxication by diphtheritic poison. In other words, he makes no provision to secure a serum which would promote the rapid casting off of the diphtheritic membrane and extermination of the diphtheria bacilli. So even in the case of diphtheria a policy of delegation, which absolves the clinician from all concern with bacteriology, and commits his work to the purely laboratory bacteriologist, would seem to be merely a policy of the second-best.

(I) Limitations of Vaccine Therapy.

And now I pass to deal with the subject which is set down for me. That subject is vaccine therapy: its administration, value, and limitations. Let me begin with the discussion of the limitations of the method. To the man who is not in sympathy with vaccine therapy

the discussion of its limitations must be pre-eminently welcome; and to those of us who desire to see vaccine therapy employed whenever it can do good, such discussion must be equally welcome. With a view to facilitating the consideration of this question, I have here set out, in the form of a tabular statement, certain salient points with reference to the limitations of vaccine therapy. I have, as you see, divided my table into two columns. In the first column I have set down a list of the objections which I have heard urged by pure clinicians. In the second column I have set forth the limitations which I myself, in common I hope with every bacteriological worker, would wish to insist upon (*see* p. 12).

When you have scanned the suggestions with regard to the limitations of vaccine therapy which are set out in Column I of the table on next page, you will, I think, recognize that we have in No. 1 the objection of the practitioner who shuts his eyes and ears to the facts until it becomes absolutely impossible any longer to do so. In Nos. 2, 3, 4, and 5, we have the usual objections of the general practitioner, consulting physician, operating surgeon and specialist who desire to stand upon the old paths, and to be let alone. Lastly we have in No. 6 the objection of the man who has no conception of the rôle which bacteria play in connexion with disease.

The contention that therapeutic inoculation has a useful application only in connexion with staphylococcus infections finds its exact parallel in the contention that prophylactic inoculation finds a useful application only in connexion with small-pox. We all now know that prophylactic inoculation has achieved brilliant results in man in connexion with typhoid and plague, and in animals in connexion with anthrax and many other diseases, and we infer from this that the principle of prophylactic inoculation is a principle of general application. In the same way we all know — all of us who care to know — that therapeutic inoculation is every day applied successfully in connexion with every kind of pathogenetic microbe. And we know also—and those of us who are actually at work on the question have realized this from the outset—that we have here a principle of general application.

We pass now to the contentions 2, 3, 4 and 5, to the contentions of those who urge that their particular spheres of practice cannot usefully be intruded upon by vaccine therapy: to the objection of the consulting physician that vaccine therapy has no useful application to any of the graver bacterial infections which he is called in to see; to the objection of the operating surgeon that vaccine therapy may never take the place

LIMITATIONS OF VACCINE THERAPY.

(I) Limitations as contended for by the clinician who regards vaccine therapy as an uncomfortable innovation.

(1) Vaccine therapy finds no useful application except in connexion with furunculosis.

(2) Vaccine therapy is of limited utility, because it can be applied only by those who have devoted study to bacteriology and immunization.

(3) Vaccine therapy finds no useful application in connexion with the graver infections, such as pneumonia, rheumatic fever, typhoid fever, phthisis, meningitis, and streptococcal endocarditis.

(4) The proper sphere of vaccine therapy is not to take the place of any surgical operation, but to supplement it.

(5) Vaccine therapy finds no useful application in connexion with the ordinary infections of those regions of the body which fall within the sphere of the particular speciality that may happen to come under discussion.

(6) Vaccine therapy is of limited utility because it is applicable only to disorders which are referable to bacterial infection.

(II) Limitations as contended for by the bacteriological worker who looks forward to vaccine therapy being applied in conformity with scientific principle.

(1) Vaccine therapy can be applied only where an exact and complete bacteriological diagnosis has been made, and where the diagnosis is kept up to date.

(2) Vaccine therapy can be applied only by those who have some acquaintance with bacteriology, some understanding of the rationale of vaccine therapy, and a knowledge of the dose of the particular vaccine which it is proposed to employ.

(3) A limit is placed to the efficacy of inoculations by the fact that there are definite limits to the responsive power of the patient.

(4) Successful results can be obtained only where an efficient lymph stream can be conducted through the foci of infection.

(5) In long-standing infections vaccine therapy can give definite results only after a long succession of inoculations, and there is no security against a relapse until the infection has been completely extinguished.

(6) In a not inconsiderable percentage of cases it is essential to success that the dose of vaccine shall be controlled by measurements of the opsonic index.

of operative procedures; and to the objection of each several specialist that useful application cannot be found for vaccine therapy within the particular speciality which he himself practises.

In reality there is no kind of necessity for combating these contentions. In so far as they are the outcome of hasty generalizations, and in so far as they are irrational and interested, they will crumble away of themselves; while in so far as they are based on reason and upon a real insight into facts of nature they will, of course, hold good.

For the present I will content myself with pointing out that already vaccine therapy promises to be brilliantly successful in pneumonia; that it holds out promise in typhoid fever and in many forms of rheumatism; that it supplies the only ray of hope we have in endocarditis; that it or a system of regulated auto-inoculation—such as Dr. Patterson has suggested and carried out at Frimley Sanatorium—is our only stand-by in phthisis; that it has already—pre-eminently in the case of tuberculous glands and many other forms of tuberculous infection—proved its superiority over operative procedures; and that it has given very favourable results in treatment of diseases of mouth, nose, ear, and genito-urinary system.

There remains to be considered objection No. 6—the objection that vaccine therapy is of limited utility because it is applicable only in disorders which are produced by microbial infection. While the world in general has progressed up to the point of accepting from the bacteriologist the fact that epidemic disease is due to microbial infection, the medical profession in general has not yet accepted it from the bacteriologist that we have in practically every disease a bacterial infection, or the result of a bacterial infection. It is still comparatively unfamiliar matter that jaundice, bronchitis, common colds, many cases of asthma, infantile paralysis, and almost all cases of cardiac disease are one and all referable to microbial infection.

And though it is made a reproach to the bacteriologist that he finds everywhere a bacterial infection, it is in reality the besetting sin of the bacteriologist to underrate, in common with every other man, the part played by microbes in disease; and he constantly has the mortification of finding that he has failed to appreciate the microbial factor in a disease, and has therefore misapprehended its nature. I would invite you to reflect in this connexion how few of us are prepared even to give a hearing to Metchnikoff when he urges that atheroma of the arteries may quite well be the result of an intoxication by bacterial products absorbed from the intestine, or to Mr. Arbuthnot Lane when

he urges—as I understand him to do—that where the breasts and ovaries of the middle-aged woman are not regularly flushed through with blood in the hyperæmia of the sexual orgasm, they tend to undergo cystic degeneration under the influence of toxins absorbed from the intestinal canal.

But perhaps I shall press home my point better if I tell you some of the awakenings that have only recently come to me, and if I show you how prone one is to be misled by tradition and nomenclature and to overlook everything that does not directly obtrude itself upon our senses. I may take these things in any order, just as they come to mind.

Formation of Scar Tissue.—It has been handed down to us as a dogma that where there has been very considerable loss of substance as the result of deep and extensive ulceration, or deep and extensive burns, the best result which one can look for is healing by scar tissue. I remember being wakened out of this belief by finding that very deep and extensive tuberculous ulcers may heal up under vaccine therapy with a complete *restitutio ad integrum.* In the first case, I remember, the ulcer was situated on the back of the hand. It was more than 2 in. in diameter and it went down to the bone, and the *restitutio ad integrum* was here so complete that, a year afterwards, it was practically impossible to tell the hand which had been ulcerated from its fellow ; and the patient, who, as a furrier, had to undertake very delicate manipulations, found his hand as flexible as before. The second case was that of a patient who has now for two years been one of our laboratory men. In his case a tuberculous ulcer which measured not less than 10 in. by 4 in., and which extended from the ear to the point of the shoulder, and which was at one point so deep as to seem as if it must burst into the pharynx, has now for years been covered in with a soft white elastic skin which shows no sign of contraction and is not very different from normal skin. It would seem in view of these and other similar cases that it might perhaps be practicable to avoid the formation of scar tissue after burns by combating the supervening bacterial infections. We are trying to see what can be done on these lines.

X-ray Dermatitis.—It will perhaps seem to you that only a man who was riding a hobby to death could suggest that a bacterial factor entered into the pathology of X-ray dermatitis. I will confess that it had never occurred to me that this might be the case until I was asked to see an X-ray operator whose hands were in a terrible condition with cracks and ulcers. Cultures here disclosed the fact that we had to deal

with an extensive streptococcus infection, and the patient received great benefit from vaccine therapy, the intractable ulcers rapidly healing up as soon as the proper dose of his vaccine had been arrived at. I ask myself, in view of the burning quality of the pain in X-ray dermatitis, and of the course that the disease runs, whether a streptococcus may not often be an important factor in this complaint.

Inflammatory Trouble at the Roots of the Teeth and Toothache.—It probably has not occurred to the ordinary man to connect trouble at the root of the teeth and toothache with a bacterial infection. But no sooner does one make an effort to shake off the disease of not thinking, and to sit down to reflect upon the subject, than it becomes clear that trouble at the roots of the teeth must be due to microbes finding their way down. And a very simple bacteriological observation then suffices to show that we have in those cases where there is trouble at the roots of the teeth generally to deal with the ordinary streptococci of the mouth. We have, therefore, here a source of trouble which may quite well fall within the range of vaccine therapy; and as a matter of fact I have often seen such trouble quiet down under the influence of a vaccine. I would suggest here, as a point for investigation, whether the burning pain of actual toothache is not analogous to the burning pain of the streptococcus ulcer, and whether it may not be due to a quite similar infection.

Pruritus ani.—Pruritus ani is, again, one of those disorders which the ordinary man would not think of referring to a bacterial infection. At any rate, it had not occurred to me that it might be due to such an infection until a patient who was suffering from this condition was referred to me for the treatment of an associated furunculosis. I now find it difficult to understand how it is possible to look at pruritus ani from any other point of view than that of a bacterial infection. I have had under observation and treatment, in addition to the case just referred to, three very desperate cases of this affection. In each case I have found that a platinum loop applied to the seat of irritation brought away quite astonishing numbers of microbes, invariably staphylococcus and pseudo-diphtheria, and occasionally tetragenus; and in each of these cases life has been rendered comfortable, or, if not quite comfortable, at any rate quite endurable, by the use of appropriate vaccines.

Hay Fever.—Although the brilliant researches of Dunbar have put it beyond doubt that hay fever is to be traced to the toxic action of pollen, it would none the less seem possible that bacteria play a rôle

in connexion with it. On the one hand the consideration that the coryza which begins as hay fever may culminate in an asthma which may last for months after the season of pollen is over, and on the other hand the fact that cultures made from the mucous membrane of the nose in hay fever often furnish voluminous growths of staphylococcus and *Bacillus septus* and other organisms, make it probable that the action of the pollen may pave the way for a microbial infection, and that this may seriously aggravate the disorder. If this is so, and if the exaggerated susceptibility to pollen which is the prime cause of the disease cannot be successfully overcome, bacterial vaccines may quite well prove useful in these cases.

Urinary Calculus.—It is now perfectly well understood that the formation of biliary calculi stands in connexion with a coli infection of the biliary ducts and gall-bladder, and it has been known for very many years that phosphatic calculi form in the urine as the result of changes produced by bacteria. It is only going one single step further to search for a bacterial cause in connexion with every case of urinary calculus, and to try to identify the bacterial cause if such should exist. I have not undertaken any systematic observations along these lines, but, again, in the course of our daily routine of work a certain number of facts which all point in one direction have thrust themselves on my attention.

Case I.—A medical man who had suffered for years from a bacteriuria, which furnished in every case a pure culture of staphylococcus, developed a renal calculus and was operated upon.

Case II.—The daughter of a medical man, who had undergone an operation for renal calculus which resulted in the removal of twenty-one oxalic-acid calculi from the pelves of the kidneys, was brought to me with a view to something being done to prevent a recurrence of her troubles. A series of cultures made from her urine furnished in each case a copious growth of staphylococcus.

Case III.—A patient whose urine had for years furnished on each occasion a pure culture of a coliform bacillus, and who had undergone vaccine treatment for this, developed symptoms of renal calculus. The operation revealed the presence of a stone, and cultures made from the pelvis of the kidney furnished copious cultures of staphylococcus. A similar operation for calculus, undertaken twelve months later on the other kidney, again furnished a culture of staphylococcus.

Case IV.—A patient who had been admitted to the Inoculation Wards at St. Mary's Hospital for the treatment of a deep and extensive ulceration caused by a combined syphilitic, streptococcal, and staphylo-

coccal infection, developed symptoms of renal colic. Radiographic examination showed the presence of a stone in the pelvis of the right kidney, and a similar calculus in the right ureter. An examination of his urine revealed the presence of staphylococci in fair numbers.

Indigestion, Vomiting, Flatulent Distension of the Stomach.—The fact that these are often prominent features in early phthisis is one of those facts which have been known so long that no one any longer asks for an explanation of them. I believe the answer to the problem may perhaps be found in the fact that pyorrhœa alveolaris is a frequent accompaniment of phthisis. I can hardly doubt, after what I have seen result from vaccine treatment in these cases, that the gastric disturbance must often be due to streptococci, swallowed with the food. I have, for instance, seen vomiting that had occurred regularly every day for six months disappear after the inoculation of a vaccine made from streptococci derived from the mouth. We must remember, in connexion with gastric disturbance which is associated with pyorrhœa, (1) that cooked food is for all practical purposes sterile food; (2) that the admixture of microbes which gives rise to fermentation can only come from the mouth or stomach; (3) that inasmuch as a more or less effectual process of sterilization takes place in the stomach, while the development of microbes goes on without such check in the mouth, the cause of the gastric fermentation is probably to be found in the microbes of the mouth which are swallowed with the food.

Epilepsy.—It would seem as if we had in epilepsy a condition which could not by any possibility stand in any ætiological connexion with any bacterial infection. But even here the judicious will find ground for hesitancy. He will reflect that in infancy almost any infection which is associated with the setting free of bacterial toxins in the organism will produce convulsions. It may therefore not unreasonably be surmised that a bacterial focus which stood in connexion with the nervous system might even in the adult produce a like result. And I believe that I have seen epilepsy in tuberculous patients improve under inoculation treatment.

Cancer.—In spite of the fact that a moment's consideration would bring it home to anyone who had come into intimate contact with cancer that the microbic infections make a large part of the misery of the disease, both for the patient and for those that come in contact with him, the rôle which microbic infections play in connexion with cancer is as yet almost unrecognized. In reality that rôle is far more important than appears at first sight. Owing, no doubt, to that defective resistance

which seems to be a feature of all neoplastic tissues, cancer, very early in its history, long before it has burrowed its way to the surface, becomes the seat of a staphylococcus infection—an infection by the so-called *Micrococcus neoformans* of Doyen. And there is reason to believe that much of the pain and swelling and inflammation in connexion with the tumour, and much, if not all, of the so-called cancerous cachexia, is due to this micro-organism. In view of this it is clear that, even though we cannot hope to strike at the root of the evil by anything in the nature of a vaccine treatment, cancer—and in particular inoperable cancer—comes within the range of vaccine therapy.

Diabetes.—It must over and over again have suggested itself that diseases which are due to the faulty functioning of some organ—diseases, for instance—such as Graves' disease and pancreatic diabetes—may ultimately be traced to bacterial infection. Countenance is given to this suggestion by such work as has recently been done in connexion with the vaccine therapy of diabetes. I may refer to a case of pancreatic diabetes which was treated by my colleague, Capt. S. R. Douglas, where the secretion of sugar and the symptoms of the patient were found to vary with the patient's resistance to a coliform bacillus which had been isolated from her fæces. I may refer also to the interesting facts which Dr. McWatters is bringing forward in this discussion in connexion with the treatment of glycosuria by staphylococcus vaccine. I think you will see that his facts suggest that glycosuria and carbuncle, which we have always supposed to be related as cause and effect, may perhaps in some cases be merely two different manifestations of a staphylococcus infection.

Enuria.—Finally, let me invite you to consider whether a bacterial infection may not in some cases turn out to be the cause of enuria. I can call to mind a case where enuria was apparently attributable to an unsuspected coli infection of the urine, and another case in which it was also associated with the same infection. Both cases yielded to treatment by vaccines.

I have perhaps said enough to rebut the contention that the range of vaccine therapy is restricted. In point of fact it not only covers almost the whole range of our present medicine, but also much that lies altogether outside its borders. For it beseems every doctor to remember with humility that there are an infinitude of chronic or recurrent ills affecting mankind which are never seriously brought to his notice, because no one believes that there is any understanding of them in the medical profession, or any possibility of a cure being found for them.

In this connexion I may just glance at another point which, perhaps more than any other, will bring home to you how extensive is the range which opens out before vaccine therapy. It is certain that we come into this world each with his individual susceptibility to microbic invasion. I am here thinking not of our susceptibility to those microbes which can pass from man to man in the form of epidemics—those microbes which alone come into the field of view of the hygienist. I am thinking rather of the fact that one man is by nature deficient in his resisting power to the staphylococcus, another to the pneumococcus, another to the bacillus of Friedländer, another to the influenza bacillus or to the acne bacillus, another again to some one of the different forms of streptococcus, or of the pseudo-diphtheria bacillus or of the coli bacillus, and another to the tubercle bacillus. And I would have you reflect, in connexion with these microbes, that, while their ravages may not be dramatic, they are collectively responsible for nine-tenths of human disease. And then I would have you reflect that while man makes efforts to guard himself against epidemic disease, and while he enlists the help of the State in this task, he accepts it as if it were an unalterable law of nature that he should be buffeted throughout his life by the particular microbe to which he individually is liable. Thus one man puts up with recurrent influenzal attacks, another man with a succession of sore throats, another man with continual recurrences of boils, another man with chronic bronchitis, another with perpetual trouble in the roots of his teeth, another with a continuous discharge from the ear, another with sycosis or acne, another with the coli infection of the urine, another with continual pruritus, another with tuberculous glands, another with phthisis, another with recurrent intestinal attacks, and so on through the whole gamut. Vaccine therapy will, I believe, help every man to keep under the particular microbe which besets him.

I now pass to consider Column II of my table; in other words, to consider the limitations of vaccine therapy, which must—if I see the matter aright—be contended for by every bacteriological worker who desires to see vaccine therapy applied in conformity with scientific principles.

I think that only the first and the last two of the contentions in Column II stand in need of anything in the way of amplification and defence. There is no one I conceive who would think of questioning that a complete and exact diagnosis is a necessary preliminary to a successful application of vaccine therapy. Nor will any one who has done work

on a case of mixed infection have any doubt as to the absolute necessity of keeping the diagnosis up to date. It must none the less be emphasized that as soon as ever a definite label has been placed upon a case, that label generally dismisses from view all further bacteriological possibilities.

The risk of accepting an incomplete for a complete diagnosis, and so imperilling the success of our treatment, is perhaps most conspicuous in connexion with syphilitic and tubercular infections. And yet in many cases the very clinical characters which serve as stigmata of tuberculous or syphilitic infection are characters which ought properly to go down to the account of other microbes; and when we go into the matter it becomes clear that it is the presence of these microbes in the lesion which puts the clinician on the trail and furnishes him with the logical justification for the diagnosis of syphilis or tubercle.

A typical instance is furnished by rupia. The characteristic rupial scab testifies to the presence of a bacterial agent which induces a copious transudation of lymph. The bacterial agent which has a pre-eminent power of doing this is the streptococcus. But in the case of a typical pure streptococcal infection the exudate is wont to be a perfectly clear lymph which sets into an amber-yellow scab which crumbles down into a powder like gum arabic. In rupia, however, the discharge is semi-purulent, and the heaped-up scab which suggests the idea of a streptococcus infection is very tough and opaque. It is, in fact, a scab that contains a large admixture of leucocytes. This suggests a superadded infection, and the commonest infective agent which leads to an emigration of leucocytes is the staphylococcus. The characters of the rupial scab thus furnish presumptive proof of the presence of streptococcus and staphylococcus—in reality both these are regularly to be found in rupia—and, little as the pure clinician appreciates the fact, it is this twofold infection which leads him to the diagnosis of syphilis. For we have apparently in the spirochæte a microbe which brings about a general lowering of the resistance of the body to microbic infection. I may say, in passing, that the inference that a multiple infection such as we have in rupia is *always* due to syphilis is not always correct.

It is, however, certain that there is no tuberculous or syphilitic ulceration of a mucous membrane, and no extensive tuberculous or syphilitic ulceration of the skin, which is not complicated by secondary infections. It follows that it is improper in any case of tuberculous ulceration of the larynx or tongue to depend entirely upon tuberculin and to overlook the associated infections. Similarly, it is important, in any connexion with

syphilitic ulceration, to keep in mind the possibility that chemotherapy may be ineffective if we do not turn our attention to the associated microbes. And in such cases it often does not suffice to combat only one variety of associated microbes. One comes across case after case where, owing to the fact that one of a batch of infecting microbes has been overlooked, a wound obstinately refuses to get well. I have, for instance, in mind a case of tuberculous ulceration of the chin which failed to make progress when treated with a combination of tubercle and staphylococcus vaccine, and which healed up rapidly when, after the discovery of a streptococcus in the wound, a combination of tubercle, staphylococcus, and streptococcus vaccine was employed. The subsequent history of this case is interesting also, as showing the necessity of keeping the diagnosis up to date. When some months after the patient had gone back to the country the ulceration broke out again, the same triple combination proved quite ineffective. The ulceration spread in an alarming manner, and the patient returned to hospital for treatment. It was then found that the *Bacillus proteus* had established itself in the wound, and that his blood gave a positive reaction with Wassermann's test. A proteus vaccine was now administered, and under the influence of this, in combination with a few doses of iodide of potassium, the wound healed up rapidly.

The fact that long-standing infections cannot be got under save by a long succession of inoculations is obvious to anyone who understands anything of the rationale of vaccine therapy. I, however, specially emphasize it here because I find that the most unreasonable expectations are afloat as to what may legitimately be expected from vaccine therapy. In view of these, I must try to make it clear what we can and what we cannot hope from the method, in a case of long-standing infection.

Perhaps the easiest way of bringing home some appreciation of the conditions is to compare the human body to a garden, the vaccine-therapist to the gardener, and the pathogenetic microbes to weeds which can be thinned out, but which, so long as any of them remain over, retain their power of multiplying and regaining lost ground.

This analogy makes it plain that the most that can be achieved in a long-standing infection by one dose of a vaccine is a temporary reduction in the number of the infecting microbes, and that it is not worth while, in such a case, to embark upon anything less than a systematic campaign. And it also makes it plain why it should be necessary to inoculate again and again for an unlimited time when a vaccine is employed for therapeutic purposes, and once or twice only where it is

employed for prophylaxis. Clearly where inoculation is resorted to for therapeutic purposes, the surplus of anti-bacterial substances, which is elaborated, is immediately expended in the destruction of microbes; where inoculation is resorted to for prophylaxis, such surplus is not used up, but is kept in reserve against future eventualities.

When a patient is informed of these facts, and when he is told that it will be necessary to inoculate again and again for an unlimited time, he will inevitably ask how long it will take to accomplish a *cure*.

It will be profitable, therefore, to clarify our ideas about this question of the achievement of a cure.

Bacterial infections fall in reality into two classes:—

(*a*) Into the first class would fall "surface infections" by microbes which are normally saprophytic on the affected surfaces. The pneumococcal or streptococcal infections, which are the commonest causes of bronchitis, are instances in point.

In this class of infection it is unreasonable to aim at a complete extinction of the infecting microbe. What we can here hope for in the way of a cure is to keep the number of microbes within bounds, and to minimize the chance of a recrudescence by keeping the patient's resisting power up to the mark.

(*b*) Into the second class would fall infections by microbes which are extraneous to the normal organism, which are always pathogenetic, and which can only be acquired by infection from the sick. Tuberculous infections are instances in point.

In this class of cases the extinction of the infection is quite a reasonable ideal. But if now, in connexion with this class of infections, the question is pressed as to how soon this can be achieved, the vaccine therapist is bound to reply that he can never say beforehand, and can never guarantee the attainment of this result. But, though he cannot prophesy, he can from time to time take stock of the patient's condition, and tell him whether or no he has made progress, and whether or no the infection has been extinguished.

In reality, we have in addition to the clinical condition—which, of course, cannot tell us whether an infection has been extinguished—not less than four different methods by which we may see how we have progressed. Let me explain what these are:—

(1) In the case where the patient's index is being regularly taken, let us say at the expiration of ten days after inoculation, we have an automatic check upon the progress of the case. For if we find that, instead of sinking rapidly away to below normal, as it did at the outset, the opsonic index now maintains itself at the level of the normal, or a

somewhat higher level, we may—if we have excluded the fallacy of auto-inoculation—confidently conclude that the patient is making good progress. For clearly as the pathogenetic microbes in the body diminish, there must be a proportionately slower expenditure of anti-bacterial substances. When we have satisfied ourselves that this is the case, and if also all the overt signs of infection have disappeared, we may proceed cautiously to apply the next test.

(2) Where we have reason to believe that the infection may have been extinguished, and want to make certain that this is really the case, we may tentatively give up the inoculations but continue to make measurements of the opsonic index. And if, under these circumstances, we still find the opsonic index remaining normal month after month, we may conclude that the drain upon the patient's anti-bacterial resources —it is probably that drain which accounts for the subnormal indices found in the early stages of tuberculous infection—has been arrested. This will signify the complete, or all but complete, extinction of the infection. A table showing the results obtained by this method of testing on a series of patients is subjoined (*see* p. 24). If we desire an even more searching test, we may take either the next or the one following.

(3) This test is based upon the consideration that if we send a lymph stream through a limb or any other region of the body which has been the seat of a bacterial invasion, that lymph stream will, as it returns to the blood, carry bacterial poisons back into the blood, with the result that a characteristic fluctuation will occur in the opsonic power of the blood. This test furnishes information which is specially valuable where the decision of the question as to whether the patient may use his limb depends upon the question of the extinction of the infection in a joint. For the purpose of this test, active hyperæmia is induced in the suspected focus of infection, a bandage is then applied to the vein in such a way as to obstruct the blood current, and when a transudation of lymph has in this manner been obtained, the lymph is driven back into the circulation by massage or active movements. In order that the test may yield conclusive results, it is advisable, in cases where there can be only very little remaining infection, to keep the limb at rest for a day or two before undertaking the test, in order to allow of an accumulation of bacterial toxins in the focus (Chart I, *see* p. 25).

(4) While the test which has just been described resolves the question as to whether there is still lurking infection in a suspected focus, consideration will show that it will not resolve the question as to whether there may not be elsewhere in the patient's body an unextinguished focus. If this question should not be held to be sufficiently resolved by

TABLE OF CASES IN WHICH THE INFECTION IS INFERRED TO BE EXTINCT FROM THE MAINTENANCE OF A NORMAL OPSONIC LEVEL, AND IN WHICH CONFIRMATION OF THIS INFERENCE HAS BEEN AFFORDED BY THE FACT THAT THE PATIENTS HAVE REMAINED QUITE FREE FROM ALL SIGNS OF INFECTION.

Case initials	Nature of tuberculous infection	Length of treatment up to complete disappearance of all evidence of infection	Measurements of opsonic index undertaken after cessation of treatment. Months									
			1	2	3	4	5	6	7-8	9-12	13-16	17-20	21-24
A. H.	Extensive ulceration of skin and subcutaneous tissues	3 months	—	·86	—	—	—	1·06	—	·93	—	—	—
F. M.	Peritonitis	39 ,,	1·13	—	·95	·91	—	—	1·05	1·05	—	—	—
M. B.	Adenitis	6 ,,	1·11	—	·98	·94	·94	—	* ·84 1·07	—	—	—	—
W. C. P.	Cystitis	5 ,,	1·17	—	—	·88	† ·69 ·96 ·88	—	—	—	1·13	—	—
G. E. G.	Epididymitis	2 ,,	·96	—	—	—	—	—	1·02	—	—	—	—
W. M. P.	Tubercular mass in vitreous	23 ,,	—	—	—	—	—	1·01	—	·96	—	—	—
E. P.	Adenitis	11 ,,	1·13	—	1·30	1·06	—	1·15	—	—	·96	1·12	—
L.	Ulceration of hand ...	7 ,,	1·18	—	·94	—	1·00	—	—	·86	1·04	—	1·20
G.	Adenitis	14 ,,	—	·89	—	·94	—	1·12	—	—	—	—	—
R. M.	Arthritis of knee	25 ,,	1·17	—	·98	—	1·06	—	1·11	—	1·14	1·19	—
R. W.	Adenitis	9 ,,	·80	—	·97	—	·96	—	·87	—	1·05	—	—
W. S.	Arthritis of knee	34 ,,	1·00	·93	·92	—	—	—	1·04	—	—	—	—
G. L.	Adenitis	22 ,,	1·12	—	—	1·18	·93	—	·84	1·07	—	—	—
G. M.	Arthritis of knee	13 ,,	1·21 1·08	1·07	1·02	—	—	—	—	—	—	—	—
G. S.	Adenitis	21 ,,	·92	—	—	1·28	—	—	·90 1·24	1·37	1·11	1·04	—

* Patient suffering from a temporary indisposition. † There is a doubt as to the accuracy of this estimation, as the patient was in perfect health, and the blood, when tested three days later, gave an index of ·96.

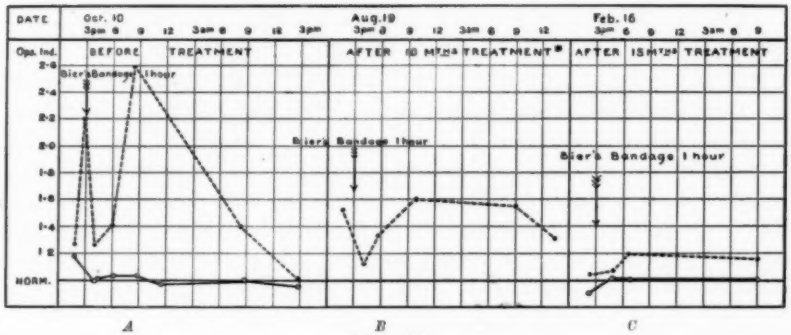


CHART I.

Chart showing results of auto-inoculation tests carried out in a case of gonococcal arthritis of wrist. ---- = Gono-opsonic index. —— = Tuberculo-opsonic index. *A*, before treatment; *B*, after 10 months' treatment with gonococcic vaccine; *C*, after 16 months' vaccine treatment.

such a series of tests as is described under (2), it can be further put to the proof by testing the patient's blood before and after severe exercise. The routine procedure which we employ in such a case is to test four samples of the patient's blood—the first drawn off immediately before exercise, the second immediately after, the third six hours after, and the fourth twenty-four hours after. A pair of companion charts showing the results obtained by this method of testing is subjoined:—

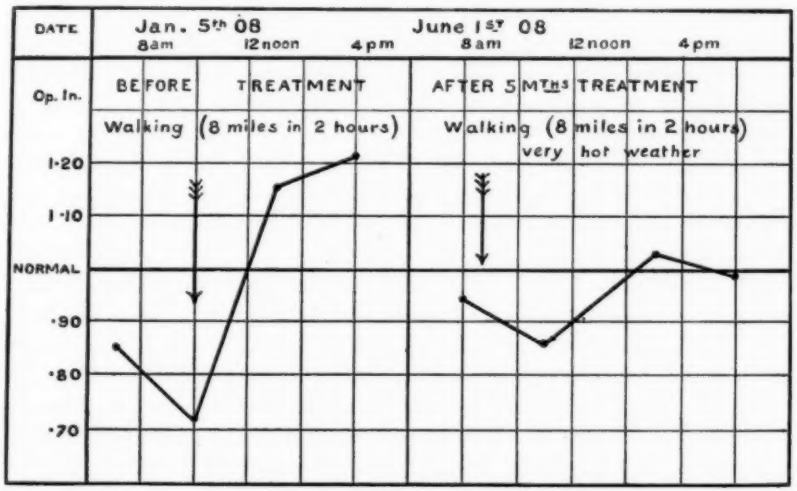


CHART II.

G. D. O. Phthisis. Auto-inoculation tests carried out: *A*, before treatment; *B*, after 5 months' treatment.

The only one of the contentions in Column II which is in any sense of the word controversial is the contention that it is not infrequently essential to success that the doses of vaccine shall be controlled by measurements of the opsonic index. We have in this connexion to consider, *first*, the question as to whether the opsonic power of the blood can be accurately measured; *secondly*, the question as to whether there is a correlation between the rise and fall of the opsonic index and improvement and aggravation in the condition of the patient; and, *thirdly*, the question whether the measurement of the opsonic index can be dispensed with, and whether any other guide can take its place.

(i) *Question as to whether the Opsonic Power of the Blood can be accurately Measured.*

I may point out, *in limine*, that the controversy which has taken place on the subject of the accuracy of the opsonic index is only what might have been expected, seeing that it is in connexion with the measurement of the opsonic index that the capacity of bacteriological workers for accurate quantitative work has for the first time been seriously put to the test.

I may very briefly refer here to three schools of criticism.

The *first*, represented by Pigg-Strangeways and his collaborators, asserted that differences such as emerge in connexion with the counting of phagocytic preparations of different bloods might quite well be due to the operations of chance, which brought into the field of view of the observer in one specimen of blood a group of leucocytes which happened by chance to contain more microbes, and in the other specimen of blood a group of leucocytes which happened to contain less microbes. These critics further suggested that if a sufficient number of leucocytes—1,000 leucocytes was the proposed number—were counted, it might be found that there was never any difference between tuberculous and non-tuberculous bloods.

A *second* school of criticism declared that, while there could be no question but that there were often marked differences between the blood of infected patients and normal blood, the margin of error in the method is so considerable as to deprive the method of any practical value. In support of this thesis some observers brought forward discordant results obtained by themselves on duplicate samples of blood. Other critics again brought forward discordant results obtained by a series of observers in different laboratories who were all supplied with the same bloods. In

connexion with the last-mentioned results, it is to be observed that in tests thus organized the results of inaccurate workers throw doubt upon the work of accurate workers. Moreover, as I have already elsewhere[1] pointed out, results obtained in different laboratories with different strains of microbes are not, and cannot be expected to be, comparable *inter se*.

A *third* school of criticism proclaimed that, inasmuch as the average ingest of a certain number of leucocytes is taken to represent the opsonic power of a blood, there here enters into the method a certain factor of chance—a factor whose magnitude can be calculated only by a statistician. Again, this school of criticism contends that, inasmuch as the phagocytic power of the individual leucocyte varies within certain limits, there here enters again into the method another factor of chance which also is a proper subject-matter for mathematical evaluation. On this basis the statistician puts forward a claim to determine the limit of error of the opsonic method.

In reality, however, in addition to the two factors which have come within his purview, there are many others which have an important influence on the result.

In the first place, the range of variation in the phagocytic power of the leucocytes, which is by the statistician assumed to be a constant, is a factor which is in reality profoundly modified by the treatment to which the leucocytes are subjected. While we have in some cases a comparatively small range of variation in cases where the leucocytes have been maltreated we have a much larger range of variation.

Again, though the statistician assumes that a record of bacteria counted in a series of leucocytes is as unambiguous and as little open to error as, let us say, a record of pips counted on a series of cards, this is very far from being so.

Microbes cannot be satisfactorily counted unless the leucocyte has first been spread out flat as a card. Again, the microbes must have been differentially stained so as to stand out perfectly clear against the background of the leucocyte. Again, the bacteria must not be fragmented or gathered together into groups. They must not be so numerous as to make accurate counting difficult. And there is also another requirement; the observer must bring to the task of counting exceptional concentration, and he must display no little judgment in the avoidance of pitfalls and fallacies.

[1] "Studies on Immunisation," p. 450, footnote 2. (Constable, London, 1909.)

It follows that the figures which represent the number of bacteria counted in the individual leucocyte are affected by the treatment to which these have been subjected *in vitro*, and these figures are also affected by the degree of skill and attention which has been brought to bear upon the preparation of the bacterial suspension, the spreading of the blood film, the fixing and staining of the preparation, and the counting.

Figures which are the resultant of all these factors, which are personal to the individual worker, cannot, I submit, possibly furnish a "*mathematical limit of error*" for a method. What they may, perhaps, furnish is a value for the "mathematical limit of error" in combination with what I may perhaps call the "*functional error*" of the particular observer, or groups of observers, whose work is under review.

Let me try to make clear to you—in so doing I shall only be elaborating what is familiar matter to us all—the importance of the "*functional error*" in connexion with our work.

The "*limit of error of a method*" is, if I understand the expression aright, a function which can attach only to quantitative methods in which we have an inherent factor of chance. It is a value which is unalterably fixed by mathematical laws, which can be arrived at only by a mathematician. It is a value which is exactly the same for every worker. No one can emancipate himself from it, or do anything to diminish it; and it is indissolubly attached to the *method*.

The "*functional error*" is an error which attaches only to methods which involve a certain amount of skilful functioning. It attaches to the *operator*. It has a different value for every operator. It may in the case of one and the same operator vary from hour to hour with his physiological efficiency. Its value can be diminished by practice and attention. It cannot be evaluated by a mathematician. It can be pretty accurately gauged by the operator himself.

The "*working error*" corresponds sometimes to "the limit of error of the method," sometimes to the "functional error of the operator," and sometimes to the sum of these values.

In the case of such an exercise as counting of the number of pips on a series of cards and taking the average of these, or in going through the same operations with a series of throws of the dice, nothing in the nature of skilful functioning is required. The "functional error" is therefore here negligible; and the "working error" may be identified with the "limit of error of the method."

In such a case as the auscultation and percussion of a chest the whole "working error" is the "functional error of the operator."

In the case of the measurement of the opsonic index "the working error" is the sum of "the limit of error of the method" and "the functional error of the operator." Of these, by far the most important factor is the "functional error."

I have satisfied myself, and all my fellow-workers have satisfied themselves, and I am glad to say a very large and increasing number of bacteriological workers all over the world have satisfied themselves, that when the "functional error" has been reduced, as it can be by practice and patience, to small dimensions, and when, in connexion with tubercle, the customary counts of 100 or more leucocytes are made, the "mathematical limit of error" of the opsonic index is such as need not seriously be taken into account.

(ii) *Questions as to whether the Rise and Fall of the Opsonic Power of the Blood is correlated with Improvement and Aggravation in the Condition of the Patient.*

I have elsewhere[1] discussed at some length the questions as to whether it is a practical ideal to make a *complete evaluation* of all the factors which make up the resisting power of the organism to bacterial invasion. I have pointed out there that inasmuch as it would be necessary for a complete evaluation of the antibacterial power of the organism to enumerate all the leucocytes, to measure their individual phagocytic activity—and, I may add, their power of digesting the ingested microbes—and to consider the question as to how far the leucocytes which are available could be brought into application; and, further, inasmuch as it would be necessary for a complete evaluation to evaluate all the known antibacterial elements which may affect the microbe from which the patient is suffering, and to make allowance for all the antibacterial elements which have not yet been discovered; and, lastly, inasmuch as there is no common denominator to which we can refer these different classes of defensive elements so as to add the one to the other or to set off the one against the other, it must for ever be impracticable to make a complete evaluation of the antibacterial forces of the body. And I have pointed out[2] that it is for these reasons

[1] "Studies on Immunisation," pp. 331-5.

[2] *Ibid.*, p. 334.

absolutely necessary—if we are to have any direct guide in our immunization proceedings—to content ourselves with a confessedly *partial evaluation* of the antibacterial powers of the blood. Again, I have pointed out that we can quite well turn to account a partial evaluation if the antibacterial element which has been selected for measurement is an element which can be accurately measured, which decreases and diminishes in the blood in response to inoculations of vaccine, and which increases and diminishes in correlation with the clinical condition of the patient.

The measurement of the opsonic power of the blood is *confessedly* and *in intention* a partial evaluation of the antibacterial defences of the body; it can be accurately measured, it is an element which decreases and diminishes in the blood in response to inoculations of vaccine; and the question for discussion is whether it increases and diminishes in correlation with the patient's clinical condition.

I have affirmed that there is such correlation. If anyone desires proof of this, he has only to watch the effect which is produced on the clinical condition and the opsonic power of a tuberculous patient by an excessive dose of tuberculin or an excessive auto-inoculation. Or if he wants to see that a rise in the opsonic index is correlated with an improvement in the patient's condition, he has only to take a tuberculous patient who has a chronically low index and watch the improvement that goes hand in hand with an improvement in the index. Or if he wants to watch the way in which the clinical condition varies with the opsonic index, he has only, in a tuberculous person, to inoculate somewhat smaller doses than are required and he will see the patient's condition improve and his opsonic index increase for a few days after inoculation, and then regularly fall away again before the next inoculation is undertaken.

I have hitherto always emphasized this generalization and said little about the exceptions, *first*, because in every new scientific departure our business is with the rule and not with the exception; and, *secondly*, because when it has once been stated that a measurement of the opsonic power of the blood is confessedly and in intention a partial evaluation of the antibacterial defences of the body, that statement inevitably brings it home to the thoughtful that there cannot then by any possibility be a perfect correlation between the opsonic power and the clinical condition.

I leave the matter there for your consideration. I would, however, point out to you that the difficult thing in this intricate web of things in

which we are entangled is to possess ourselves of the broad generalizations. It is no difficult task to alight upon an exception to a generalization which confessedly gives only the general rule. The man who, meeting an exception which might have been foreseen, straightway throws up the sponge is like the man "who encountered a corpse and retreated to bed, announcing that all life was contradicted." I suppose that not even the correlation that exists between the readings of the clinical thermometer and the condition of a patient is to be accounted a perfect correlation.

(iii) *Question as to whether the Measurement of the Opsonic Index can be dispensed with, and whether there is any other Guide which can take its place.*

I have said elsewhere[1]—weighing my words very carefully as I did so—practically everything I have to say on the question of the importance of controlling the dosage by measurements of the opsonic index. I pointed out there that in a large class of cases it is impossible within the short interval which normally elapses between one inoculation and another to tell by any clinical observation whether the preceding dose of vaccine has elicited a satisfactory immunizing response. We have in chronic tuberculous infections typical examples of this class of case, and there are also many obstinate cases of infection by other micro-organisms—cases of empyema, sinusitis, middle-ear disease, urinary infections, streptococcal infections of bone, &c.—where a definite clinical improvement can be hoped for only after a consecutive series of effective inoculations. In all these cases we have either to work entirely without a guide or to rely upon the opsonic index as our guide. I have also emphasized that even where we have more or less definite clinical symptoms to guide us at the outset there will, if the patient improves, inevitably come a time where the clinical symptoms will fail us as a guide, but where the inoculations ought to be continued for the purpose of extinguishing the infection. Here, again, the opsonic index is our only possible guide. And we cannot dispense with it if we are to diagnose our obscure cases, to take accurate stock of the progress of the cases we have under treatment, and to satisfy ourselves that we have extinguished the infection and that we may suspend our inoculations.

[1] "Studies on Immunisation," pp. 434-9.

If any of these statements are controvertible, it would be well that they should now be controverted. For, up to the present, what I have urged in favour of the importance of controlling dosage by the opsonic index has been met only by insistence that the correlation between the opsonic index and the clinical condition is not a perfect one—if this were a reason for dispensing with the opsonic index, it would also be a reason to dispense with the thermometer—and by the iteration of the formula that the clinical signs give to the clinician sufficient guide in the regulation of his dose. When I find a speaker obsessed with this formula; when I find him ignoring the fact that there are cases where no guidance can be obtained from the clinical symptoms; when I find that he refuses to face the problem as to how in such cases the dose of vaccine is to be regulated; when I find him citing cases which he has inoculated with success under the guidance of definite clinical symptoms, and adducing these as proof that the opsonic index can be dispensed with in cases where guidance cannot be obtained from the symptoms, I ask myself whether I am not perhaps listening to "one of those sages whom a man should understand less as he heard him longer."

While I have insisted, and continue to insist, that there are many cases where we are not doing the best for the patient if we are not controlling the effect of our inoculations by the opsonic index, I have from the very outset recognized that vaccine therapy can in many cases be carried out with success without its aid. I recognize that more fully every day, and I rejoice, with all those who understand what vaccine therapy may mean to the world, that it should be so.

Let me briefly describe to you what our practice is in connexion with the control of inoculation by the opsonic index in the case of the out-patients and in-patients in the Inoculation Department of St. Mary's Hospital. In an ordinary case of localized streptococcus or staphylococcus infection we practically never have recourse to the opsonic index. In connexion with these infections we know the appropriate doses of vaccine, and the clinical symptoms furnish any further guide that may be required. The same holds true of acne. It holds true again of croupous pneumonia.

Where we have to deal with a case of staphylococcus infection, such as sycosis, which has obtained a firm hold upon the patient and which offers considerable resistance to the treatment, and which we can only hope to overcome by a succession of effective inoculations, it is often necessary to regulate the dose by means of estimations of the opsonic index.

The same holds true of the very chronic streptococcus infections which are associated with tuberculous disease of bone. It holds true again of the chronic coliform infections.

In the cases of tuberculous infection we make a distinction. We make it a practice in every case of phthisis to control the effects of the inoculations by the opsonic index, but employing, as we do in the case of phthisical out-patients, only doses which give no negative phases, we find it sufficient to determine by blood examination, undertaken on the day before the patient returns for inoculation, whether the dose has been adequate to keep the opsonic index up to the normal. In the case of phthisical patients who are treated in the wards, more frequent examinations are undertaken. In the case of tubercular adenitis and other localized forms of tubercular infection we limit our opsonic examinations if satisfactory progress is being made. As a rule, we undertake these only where the question of increasing the dose presents itself. In cases which do not make such satisfactory progress the opsonic index is estimated much more frequently.

In cases of septicæmia and in cases of advanced phthisis, and, in short, all cases where the condition of the patient is undergoing constant and rapid changes under the influence of continuous auto-inoculations, we find that the measurement of the opsonic index does not render any very valuable services.

In conclusion, I may mention, in connexion with the question as to whether the temperature in a pyrexic case can be taken as a guide to the opsonic index, that we have over and over again verified that, except in those unfortunately more or less rare cases where a pyrexic infection is being definitely got under by inoculation, we do not find any of that inverse correlation of temperature to the opsonic index which is illustrated in some of our published charts[1] and which Dr. Latham, generalizing apparently from very few cases, has alleged to constitute the general rule. The temperature cannot therefore be depended upon as a guide in immunization.

I pass on from the consideration of the limitations of vaccine therapy to its results.

(II) Value of Vaccine Therapy.

It would clearly be an impossible task to attempt here even a summary of the results of vaccine therapy. All that I would propose

[1] "Studies on Immunisation," pp. 404 and 407.

to do is to suggest for your consideration certain general canons of criticism which ought, I think, to be kept in view when we set ourselves to appraise the results of any therapeutic procedure.

The question as to how we are to give our verdict upon the success or failure of vaccine therapy in the individual case ought obviously to take precedence over the question as to how, when we have passed our verdict upon the success or failure of vaccine therapy in a series of cases, we can bring these separate verdicts together into the form of a general verdict.

Let me then begin with the former of these questions. Now I would put it to you that we have in connexion with vaccine therapy to consider two entirely different classes of cases.

We have, first, the simple and unambiguous case. This is the case where the treatment consists of a single inoculation of vaccine, and where the verdict takes into account only the results of that one inoculation. We may take as an example the case in which the vaccine therapist sets himself the task of aborting a stye on the eye, or of arresting a streptococcus infection in a poisoned wound. To abort the stye and to stop the streptococcus infection promptly is to succeed—for these things do not happen of themselves—to do less than this is to fail. The game consists here in the winning of one trick, and there can be no two opinions as to whether that trick has been won or lost.

We come now to the more intricate case. This is the case where the treatment comprises a whole series of inoculations, and where what has to be adjudicated upon is the success or failure of the whole series of operations. Consideration shows that the conditions which here present themselves may be compared with a game which consists in winning, not, as in the case last considered, a single trick, but a whole series of tricks. Among the problems which here present themselves there is again a simpler and a more difficult one.

Let us begin with the former. It is clear that if the patient gets steadily worse under inoculation, the case must be counted as a failure to vaccine therapy. And if the case gets steadily better and has ended in the extinction of the infection, it must be classed as a success to vaccine therapy, *if* it is conceded that the case offered no prospect of a spontaneous cure. If that is not conceded, the case becomes, for those who have either a bias in favour of or a bias against vaccine therapy, a case for wrangling over. For the judicious and unbiased person it becomes a case which is to be set down as a more or less probable success for vaccine therapy. In other words, if the game is

won it may be a matter of dispute whether it is the vaccine therapist or another who has won the tricks, and the dispassionate man will decide on the probabilities.

I pass to the really difficult cases. We have such a case where the vaccine therapist begins by winning a series of tricks and then begins to lose, and the game is broken off. We have again such a case where the vaccine therapist wins a series of tricks and it is assumed that this wins the game, and then the game is broken off, and then, after abandonment of treatment, there comes a relapse. And we have again such a case where the game is never definitely won, but where the vaccine therapist reinoculates whenever the condition of the patient requires it, and each time temporarily ameliorates matters.

In these cases the judgments that are passed are often absolutely reckless. It is here that the man who is blinded by a bias in favour of inoculation claims credit for winning the whole game when he has won only a very few tricks. It is here that the man who is blinded by a bias against inoculation contends that the game has been lost when only one trick or a very few tricks have been lost.

I will leave it to others to illustrate the effects of the bias in favour of inoculation. I think the following verdicts will illustrate the effects of the bias against inoculation. They relate to cases which were treated by me, which then passed out of my hands into the hands of others, furnishing to them material for controversial uses:—

The first was a case of tuberculous epididymitis. An eminent surgeon had arranged to operate in this case, and the day had been fixed. On the advent of that day he, however, found that the disease had progressed so rapidly that the operation had to be abandoned. The patient was then referred to me for treatment by vaccine therapy. For a long time the patient made very satisfactory improvement under vaccine therapy. He then lost ground. If I remember aright, the epididymis began to break down. He then went back to the surgeon, and the operation for amputation of the testicle, which had before been abandoned, was undertaken. The case was then controversially cited as an example of the failure of vaccine therapy.

I do not demur. I merely point out that this was a case where, in the language of my metaphor, I had won a series of tricks; I then began to lose, and the fact that I had failed to bring about a cure was the only fact which was taken into account, although, if I appreciate the case aright, the success which attended vaccine therapy had made it possible to undertake the operation with a prospect of success. But I ask

myself what would be the verdict of the judicious upon such a case. Would *they* pronounce it a failure or a success?

The second case was that of a patient who had been suffering for several months from a chronic œdematous inflammation of the pharynx and soft palate, which had been treated unsuccessfully by an eminent laryngologist. I was asked to see the patient, and found that the cause of the trouble was a mixed pneumococcus and catarrhalis infection. A striking improvement manifested itself immediately after the inoculation of the first dose. The patient was easier and the œdema and cyanosis of the affected parts had markedly diminished. A few days later, however, a small perforating ulcer made its appearance in the central line of the soft palate, and inoculation-treatment was discontinued. The case was published by the laryngologist who had been in charge of the case as a failure for vaccine therapy.

Again I ask myself what the verdict of the judicious would be. It would perhaps have been that one trick had been won by vaccine therapy, or perhaps—in view of the development of the ulcer—that one trick had been lost. But I think the decision would not have been that the game had been played to a finish and that vaccine therapy had proved a failure.

The third case was that of a patient with fairly advanced phthisis who was treated as an out-patient in the Inoculation Department of St. Mary's Hospital. Considering the fact that she was a frail little woman and had a very considerable distance to come for treatment, this patient had, we thought, made very fair progress under vaccine therapy; and she, of course, assured us that she had. Some time in the late autumn of last year she fell seriously ill, discontinued her attendance, and saw in consultation with her medical attendant an eminent physician. By his advice she was placed upon continuous inhalations of an antiseptic. A letter published by her medical attendant in one of the medical journals early in this year recounts the brilliant success of this method and the failure of vaccine therapy.

Only two or three days ago that patient was admitted to St. Mary's suffering from pneumothorax and in a very serious condition.

I do not know what the commentary of the judicious would be upon this. He might, of course, decide that the case was a success for vaccine therapy, and a success also for the method of the continual inhalation of antiseptics. Or he might decide that it was a failure for both methods. But I think he would not decide that it was a failure for vaccine therapy and a success for the method of the continuous

inhalation of antiseptics. I think he would remind the letter-writer that "with what measure you mete it shall be meted to you again."

I have suggested all these difficulties to you. I confess I do not see my way out of them, unless perhaps we could content ourselves, in cases like those which have been cited above, to abstain from pronouncing a verdict on the summarized result of the whole. In such cases we might, it seems to me, content ourselves with giving a verdict on each successive movement in the game.

If I have succeeded in making you see eye to eye with me in this matter, if I have succeeded in making clear to you how in many cases almost insurmountable difficulties stand in the way of a final decision on the results obtained in the individual case, it will not be necessary for me to emphasize the fact that it must be hopeless to attempt to summarize our verdicts on vaccine therapy in any statistical form.

I have, in conclusion, only one or two passing remarks to make in connexion with the question of the mode of administration of the vaccine.

(III) Mode of Administration of the Vaccine.

Probably to those who are familiar with Dr. Arthur Latham's work the issue as to whether the administration of vaccine by the mouth is an improvement over the method of hypodermic inoculation will be the first issue to present itself for consideration. For all of us, however, who approach it from the standpoint of the laboratory, this issue was already *res judicata* before it was brought into prominence by Dr. Latham. I had satisfied myself long ago — and the results of my experiments are embodied in my "Short Treatise on Anti-typhoid Inoculation"—that, though typhoid vaccine may be absorbed by way of the intestinal canal, it is only badly and incompletely absorbed, and above all that its action is uncertain. My friend and successor at the Royal Army Medical College, Sir W. B. Leishman, has carried out far more extensive and more careful experiments in connexion with the same question, and has arrived at the same result. He finds that inoculation by way of the alimentary canal is an extremely uncertain process. I do not think it is necessary to go any further than that, for science never sanctions a more complicated and more uncertain method being employed where a simple and certain one, such as subcutaneous inoculation, is available. And I would submit that the idea that a *via media* can be found between the antique system of

prescriptions, with doses taken t.i.d., before or after meals, and scientific applications of bacteriology, and the idea that that *via media* can be found in the administration of vaccines by the mouth, ought steadily to be put away from us. The new wine of bacterial vaccines cannot with impunity be poured into the old bottles of ancient medicine.

There is, in conclusion, just one other issue which I should like to put before you for consideration. The problem has been before my mind for a long time, and I understand that it has also suggested itself to others.

Let me introduce it to you by recalling to your mind the mental picture which I dwelt upon at the outset of this discourse. You will remember that I suggested to you that the rationale of vaccine therapy was to be found in the exploitation of the unexercised immunizing capacities of the uninfected tissues. In other words, I suggested that the antibacterial substances which are elaborated in the organism in response to inoculation are elaborated by the tissues into which the vaccine is introduced. Now inasmuch as we may make our inoculation into any part of the subcutaneous tissue, it would, upon the theory that antibacterial substances are produced at the site of inoculation, seem to follow that we ought, by inoculating in a series of different places, to get a summation effect. The idea that such a thing might be opens up whole vistas of therapeutic possibilities.

But at present the achievement of a summation effect from a series of simultaneous inoculations undertaken in different parts of the body belongs, like the achievement of a summation effect from a series of consecutive inoculations undertaken each after the previous one has evoked its immunizing response, to the realm of unrealized possibilities.

There lies before us here a wide and very difficult and unexplored field of work.

"Something lost behind the Ranges. Over yonder.
Go you there."

Dr. CHARLES SLATER: It is with considerable reluctance that I take part in this discussion, and I shall not detain the Society more than a few minutes, as I wish to confine myself to emphasizing the necessity which exists at this stage of the inquiry for great care in selecting the evidence which is to be used in support of vaccine therapy. There is no need to do more than refer to the solid basis for this method of treatment which was laid by the work of the older bacteriologists, and we appreciate the more recent additions due to the labours of Sir Almroth Wright, Sir William Leishman, and their co-workers.

No doubt in many inoculation departments a large body of evidence based on well-observed and well-authenticated cases is being accumulated, but as yet not much of this has been published. In the meantime judgment as to the value of the method of treatment is being formed on the basis of cases which are not of well-defined clinical characters, and whose bacteriology is even more indeterminate. It must be remembered that this system of treatment claims not only to alleviate or to cure, but that it is a scientific treatment, depending for its effect on the use of specific materials counteracting specific infections or intoxications. Consequently something more in the way of evidence is needed than improvement after the administration of a vaccine; every trade circular advocating a new drug contains that kind of evidence. How many of the cases on which judgment is being formed are cases of disease of the nose, throat, air-passages, intestinal and genital canal—diseases of ill-defined character, often very chronic, and liable to very considerable variations in severity? Of the clinical side I do not wish to speak, but on the bacteriological side I would point out that there is often very slight *evidence* that the vaccines employed have any real connexion with the disease; it is frequently a case of presumptive evidence. The relation between the pathological condition and the vaccine rests very often solely on the numerical preponderance of a particular germ either in preparations or in cultures made from materials in which a very large number and variety of organisms are present. Now every bacteriologist knows that there is often a very great discrepancy between organisms as seen in the preparations made from such material and in the organisms as found in the cultures made from the same material, and that it would be very unsafe to assume pathogenic importance on the basis of numerical preponderance in preparations, and still less safe on numerical preponderance in culture where so much depends on the culture medium and other conditions.

Not infrequently selective culture media are employed, which makes the argument still less cogent. The connexion is strengthened when preparations and cultures agree, but this is very frequently not the case—indeed, I should say is generally not the case. Practically, since the vaccine must be grown, the evidence of the culture is usually allowed to overweigh the evidence of the preparation. That this difficulty is felt is shown by the fact that further evidence is adduced by observations on the opsonic index, and that organism, from a number, is picked out as the most important which shows an abnormal opsonic index when tested against the patient's serum. This, again, I hold to be a very frail thread. I do not deny that its usefulness may be great sometimes, but not when we are considering evidence in support of vaccines. Even the much more definite agglutination tests, if used for the purpose of fixing the pathological importance of an organism in a case of doubtful bacteriology, would lead us to some strange conclusions.

In many cases reported the characters of the organism used are indifferently defined. Organisms such as *Micrococcus catarrhalis*, *Bacillus segmentosus coryzæ*, and *Micrococcus paratetragenus* are freely mentioned, but their characters rarely stated to have been tested. The former is by no means easy to secure in pure culture, and is especially liable to be mixed with organism having fermentative powers, and I have noticed that varieties are spoken of and admitted in some investigations. It is not an organism which lends itself to the opsonic technique; at any rate, some varieties are difficult to work with, and occasionally a strain which lends itself to the formation of a workable emulsion is used for testing the patient's index instead of the one actually isolated. The criticisms I have made above apply with equal, if not greater, force to many of the cases of intestinal infections which are recorded, and the *Bacillus coli* is so easy to find that it must always be suspect to a bacteriologist unless there is very clear evidence of its connexion with the disease which it is supposed to cause.

These criticisms are made solely in the hope that evidence of doubtful value, which does not become more valuable because it is multiplied, should be rigidly set aside, and that it will not be necessary for some future investigator to go through the records, as has recently been so brilliantly done by Dr. Bulloch in his work on hæmophilia, and reject a large proportion of the work. At the present stage of the matter, cases with well-marked clinical characters whose bacteriology

is definite, or in which the proof of the connexion between the disease and the vaccinating organism is complete, should alone be admitted as evidence for or against the value of vaccine therapy as a scientific treatment. These views may seem ultra-conservative, but it must be remembered that vaccine therapy has a commercial as well as a scientific side, and it by no means follows that all the cases treated are necessarily such as contain even the elements of scientific accuracy. I am speaking as the friend of this method of treatment, and not as an opponent, when I urge that the cases quoted in proof of its efficacy should be rigidly selected.

Dr. L. COLEBROOK: Sir Almroth Wright has anticipated that there will be some barriers raised across the path which he has taken, and he has asked me to bring forward some evidence on one or two issues which are certain to come up.

The first of these is the relation of the temperature to the immunizing process. The contention that one could control inoculations just as well by watching the temperature as by taking the opsonic index was raised by Dr. Latham before this Society a year or two ago, and Dr. Latham brought forward several charts which showed clearly an inverse correlation—i.e., a high temperature coinciding in time with a low index, and vice versa. Since that time there has accumulated much evidence along this line, and in Sir Almroth Wright's wards at St. Mary's Hospital we have taken a good deal of care to establish whether, or not, we can rely as well on the temperature as the index.

Taking, first of all, some cases where the temperature is keeping steadily along, or near, the normal line, we find that following a slightly excessive dose of vaccine there occurs, *almost* invariably, a rise in temperature in association with, or slightly preceding, the "negative phase" in the blood. Turning, however, to the more serious and also the more difficult class of cases, where the temperature is not keeping steady, it has been a much less easy task to unveil a general law. To this end we have studied some cases where we thought there was only one microbe concerned. Firstly, two cases of advanced tuberculosis of serous membranes—pericardium, pleuræ, peritoneum, where there was usually a temperature variation of 3° F. or 4° F. in the twenty-four hours. Sometimes, in these cases, the index followed the excursions of the temperature; sometimes it went in the reverse direction.

Again, we studied a case of puerperal septicæmia of long standing, doing three or four indices a day, but that yielded us nothing in the way of a general law. On some days our charts showed curves like Dr. Latham's—i.e., high indices concurring with low temperature, and vice versa; on other days the reverse relation. Again, I would refer you to three charts bearing on the same point in the case of glanders. They relate to some periods in the illness of our late colleague Dr. Wells. This chart (I) needs a word of explanation: the temperature was keeping fairly normal, but Dr. Wells had a quiescent focus in the calf of his leg, and we did not believe the infection was finished with. For a month before the period recorded on this chart his blood had not been below 1·4; then, for some reason, it dropped below the normal.

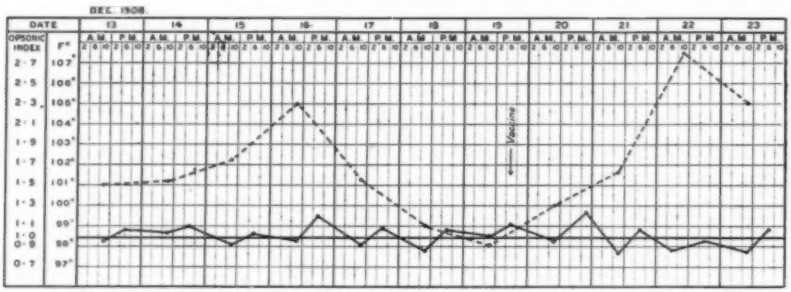


CHART I.

Glanders. ---- Opsonic index to *Bacillus mallei*.

There was no indication from the temperature that one should inoculate, but from the index there manifestly was. Similarly there was no considerable temperature phenomenon corresponding with the marked rise in opsonic power after the dose. Turning now to Chart II, you see both types of correlation more or less distinctly. There had been no inoculations at about this time, but at this point ($\overset{M}{\downarrow}$) the record tells us that he walked to his bath and massaged the calf of his leg. The temperature rose to 102° F., but at the same time the index went steadily up also, being examined every day. If one had been using the temperature above as an indicator one would have inoculated at this point, but from the point of view of the investigator of the immunity

it would have been the wrong moment. It would seem from this piece of chart, as from a considerable amount of other evidence that will occur to you, that the temperature is to be regarded as an intoxication effect rather than an immunity effect. The third chart from the case of Dr. Wells (III) will show you again both types of correlation. Our ultimate failure to save Dr. Wells, in spite of constant observations of

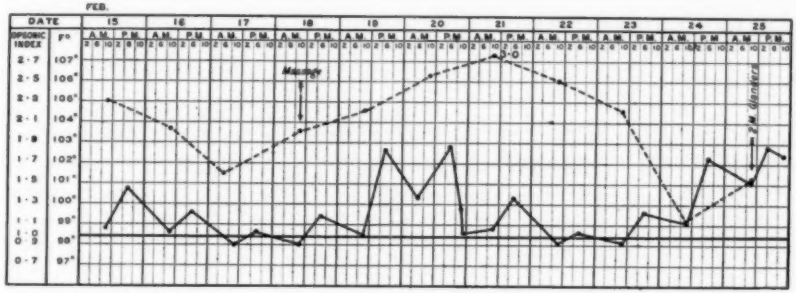


CHART II.

Glanders. ---- Opsonic index to *Bacillus mallei*.

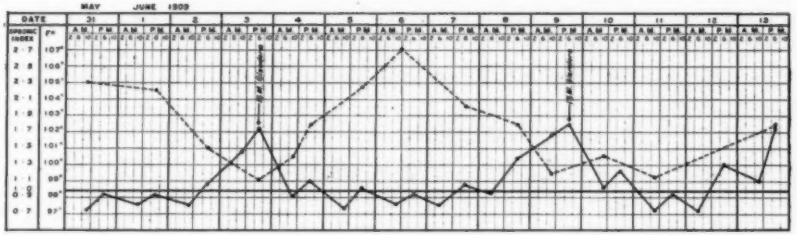


CHART III.

Glanders. ---- Opsonic index to *Bacillus mallei*.

his immunizing efforts (opsonins and agglutinins) and every kind of dose of vaccine, brings home to us the most important of the limitations of which Sir Almroth Wright has spoken—viz., our deficient knowledge of the immunity processes.

What conclusions emerge from this study of the temperature question?

First: It seems that where the temperature is steady, along or near the normal line we may get *some* valuable indications as to dosage —that is to say, we may detect even a slight *excess* in dose, of vaccine— but that is all; we shall get no evidence to guide us as to whether a given dose is the optimum or something less, and that guide we want, to get the best result of our treatment. Where, on the other hand, the temperature is irregular it will not serve as a reliable guide to us and must be regarded as an intoxication effect rather than an index of the immunizing effort.

I would say something now as to the value of the opsonic index in vaccine therapy. Sir Almroth Wright has not touched to-day upon its accuracy, and I must leave it to someone else to bring forward the more subtle mathematical considerations with reference to it. But with many of us, I fancy, these more refined data and arguments will weigh less than such evidence as the following, which has evolved from our routine blood examinations of cases in the wards and out-patient department, quite apart from any definite investigations upon this point; treatment being the only question at stake when the "bloods" were tested. The contention has been urged by some that all sera will be found equal in opsonic power (i.e., every index will come out normal) if enough trouble is taken to perfect technique, and especially to count a large number of cells. This contention does not demand serious thought on the part of any one accustomed to dealing with microbes other than tubercle and staphylococcus, for it is by no means uncommon to find a patient's serum with opsonic power anything from two to forty times as great as normal serum—for instance, in cases of *Bacillus coli* infection. I have just shown you glanders charts showing opsonic indices of three times normal. But when we consider tubercle cases the contention is possibly more in order, inasmuch as here we are not accustomed to nearly such large variations in the blood, and one can understand the worker, perhaps, with limited experience, saying that, if one's technique is good, all sera come out within normal limits.

In this connexion I would cite to you the following case: A woman with afebrile phthisis was in our wards six months, and during that time her index was recorded two or three times per week, and showed only the small range of variation usual in such cases when at rest— i.e., between 0·7 and 1·3, the majority being closer to normal, either a little above or a little below. Suddenly she developed pleurisy, which proved to be a mixed infection of pneumococcus and tubercle, both

microbes being found in the effusion. That process served as an auto-inoculation, and while her temperature was still elevated, 100° F. to 101° F. We recorded twelve consecutive indices during a fortnight, ranging from 1·10 to 1·60 (1·14, 1·25, 1·10, 1·19, 1·34, 1·38, 1·22, 1·43, 1·53, 1·59, 1·20, 1·10). Of seventy-two indices previous to this attack of pleurisy there was not one above 1·30. Synchronous with this maintained high level of immunity was an equally striking clinical improvement, associated with an almost complete cessation of the output of sputum, which had been a troublesome and obstinate feature for two years.

Nor is this an isolated example; our records at St. Mary's could furnish many similar series of maintained high or low level, some extending for much longer periods than the above, and these, if there were really a tendency for all to be normals, could not be explained except on a very violent assumption of coincidence.

One more case may be cited here: A child came to "casualty" with a tender mass of glands in the neck, and an index of 1·47 to tubercle. All through the following two weeks she continued to have high indices, three times above 1·40, and the glands were decreasing rapidly in size. Apart from its bearing on the point at issue here, this series of high indices taught us two things of value in treatment—viz., that the infection was tuberculous, and therefore to be regarded seriously, and that, for the present, nature was doing well enough without any artificial aid. After three weeks the child's index came down to normal and we inoculated, the glands having almost entirely disappeared. She has had no recurrence at all during eight months.

Here I would leave the consideration of the accuracy of the index, and say something of its current value in diagnosis and treatment. Sir Almroth Wright has referred you to a chart which may serve well as an example of our method of using the index in diagnosis, when it is a question of deciding which of two or three known pathogenic microbes is responsible for a given lesion, say an arthritis. In another class of cases we have used the index for diagnosis where the problem is somewhat different—viz., to pick out the one or more microbes which are acting as pathogenic agents in an individual case, when many varieties are present in the infected area. Such a problem faces us in dealing with colitis, and this method of attacking it (viz., by isolating a few of the possibly pathogenic microbes from a stool and testing the blood to find out to which of these there is an abnormal resistance) has

been largely used by Dr. Matthews in his work on the application of vaccine therapy to this disease.

Chart IV illustrates an extension of this process for diagnosis—viz., an attempt to bring to our aid an auto-inoculation experiment analogous to that used for the arthritis case, as shown in Sir Almroth Wright's chart. To this end calomel was administered, and, some four hours later, massage of the thickened colon was employed. Samples of blood, taken before the primary administration of calomel, and at intervals after the massage, were tested to five coliform microbes

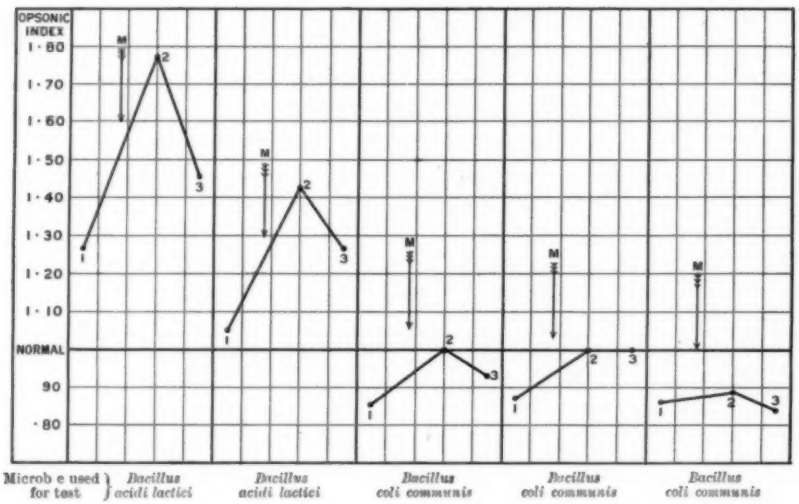


CHART IV.

Mucous colitis. Experiment to determine the causal organism. Auto-inoculation by calomel and massage, M ↓ Blood specimen 1, before calomel; blood specimen 2, half-hour after massage; blood specimen 3, four hours after massage.

isolated from the stool. With three of these microbes only slight variations in the opsonic indices were recorded, while with the other two the range of movement was greater, and this was adduced as evidence that these microbes were playing a pathogenic rôle. On more complete bacteriological examination of these two microbes it

was found that they were in reality identical, giving the reactions of *Bacillus acidi lactici.* This diagnosis of the causal organism was sufficiently confirmed later, by the marked improvement of the clinical condition which followed the use of the vaccine, having previously resisted other measures for three years.

Before leaving the subject of the value of the opsonic index for diagnosis, I would remind you, as Sir Almroth Wright has done, of the importance of making a complete diagnosis, which should include, not only the detection of the offending microbe(s), but also an assessment of the auto-inoculation processes obtaining under conditions of rest. Particularly is this of importance in cases of phthisis, where it is notoriously difficult to form an accurate conception of the extent and intensity of the infection from physical signs alone, and where, too, the ultimate result is more closely connected with this matter of auto-inoculation than with any other factor in the disease.

I may, then, pass over at this point into the domain of treatment, and consider briefly the current value of the opsonic index there. I would claim for it that it enables us, with the least delay, to arrive at the dose best suited to any given patient, either at the outset or at any point in the subsequent treatment of the infection, if we have reason to reconsider the propriety of our dose. This statement, balanced against what we know of the labour and expense involved in the determination of the index, is practically equivalent to saying that it demands and justifies its application in treatment to the more difficult and serious cases, in the conduct of which we can ill afford to lose (in Sir Almroth Wright's phrase) a single "trick."

I will now submit to you some concrete examples illustrating such advantages as I have claimed for the use of the index:—

Case I.—A woman was admitted to Sir Almroth Wright's wards with a very acute tuberculosis of hip, intensely painful, with considerable swelling, and an irregular, raised temperature. A succession of high indices to tubercle determined a verdict against a pyogenic infection of the joint, but the index soon fell and the question of dosage of tuberculin came up for consideration. The index showed us that only a very small dose, $\frac{1}{50000}$ mg., was, at that time, appropriate. Gradually pushing the dose, during two months of extremely satisfactory progress, we were surprised to find that, following $\frac{1}{20000}$ mg., the indices remained low (0·80), although there had been no temperature reaction and malaise following the dose. A further increase, to

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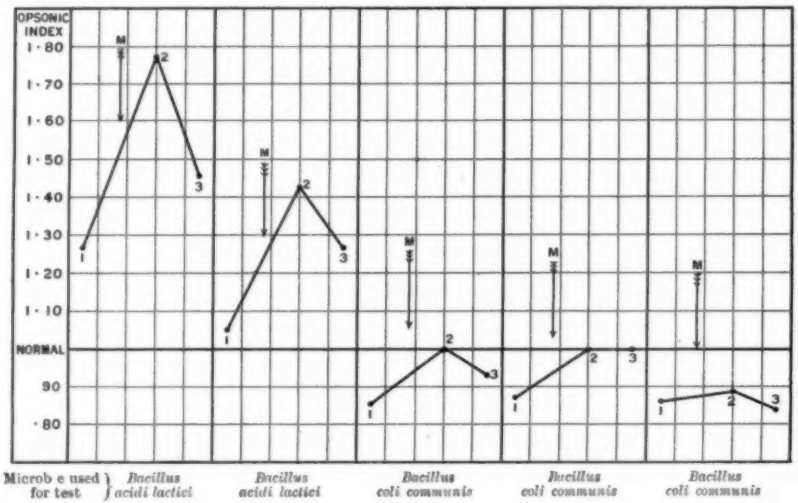


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$\frac{1}{15000}$ mg., sufficed to provide slight temperature reaction, with headache and continued low indices. Going back in dose, to $\frac{1}{30000}$ mg., we obtained satisfactory immunity and continued improvement. Here was a difficult case because of unusual susceptibility to tuberculin, even at the time when the focus was quiescent. I think it very probable that, if such a case had been treated without the help of the opsonic indices, larger doses would have been employed all along, and the case regarded, ultimately, as a failure of vaccine therapy.

There was in the ward at the same time a small boy, aged 7, with a chronic tuberculous infection of bone (lumbar vertebræ) and sinuses which were infected with staphylococcus and streptococcus. An irregular temperature chart did not help us in establishing dosage, nor did subjective phenomena, on account of the boy's small mental stature, so we had to fall back on the index, and, with this guide, pushed the dose of tuberculin to $\frac{1}{2000}$ mg. before we reached the optimum—a larger dose than any adult in the wards was having; and not until we had arrived at this dose could we control the excursions of his temperature.

To cite one more case: A country practitioner told me that he had been inoculating several cases of urinary tuberculosis with satisfactory results, but, in contrast to these, he had one case which was not doing well on a similar plan of dosage. I suggested that there was perhaps some other organism in addition to the tubercle bacillus playing an important part. This, however, proved not to be the case, and so some four or five determinations of the index were made. The blood was found to be keeping low, but on reducing the dose considerably, it rose, and a few weeks later I heard that the clinical improvement had been very marked.

The last two cases have been adduced in illustration of the value of the index in *difficult* rather than urgently serious cases, and before leaving the subject I ought to say a word upon these latter cases. The question has often come up for discussion, as to whether or no we may place any reliance upon one index per day when the temperature chart is showing irregular and large variations. Although we have found the excursions of the index to be, on the whole, less distinct than those of the temperature within twenty-four hours, yet we must, I think, answer the above question in the negative. We may, at the same time, wisely reserve as a method of choice in difficult cases of this kind, the practice of examining two or three specimens of blood in the twenty-four hours

preceding, and following, an inoculation. Such a method of choice must, however, remain, for the most part, impracticable.

I would now bring before you a few cases of real, or apparent, failure of vaccine therapy which have come under my notice. Into Sir Almroth's Wright's wards during the past year we have admitted several cases which have been segregated from our out-patient department, by the fact that they were not doing so well as the majority of the cases, and we have instituted special investigations to determine, in each case, the reason for such non-success. I think a short series of such gland cases will be of interest to you.

The first case is that of an adult woman (N. S.), who had received inoculations during two years, for a large mass of glands on one side of the neck, of which the clinical characteristics were held to be more those of tuberculous than of a lymphadenomatous infection. In differentiation between these two infections the index has not served us, since in both it is abnormal to tubercle bacillus, and although in some lymphadenomatous cases improvement has followed tuberculin treatment, the larger number have refused to yield to it. In this case the surgeon cleared up our difficulty by removing the mass of glands, which proved to be typically lymphadenomatous. Here, then, was only an apparent failure of tuberculin.

The second case is very similar, an adult woman (B. H.), with a large mass in the same situation, who had been under treatment for eighteen months without diminution of the chief tumour mass, although some outlying glands had decreased and disappeared. Skilful clinical fingers were unable to agree as to the presence or not of fluctuation, but a sac full of sterile pus was all that was found by the surgeon, who was considerably surprised at the complete absence of any other infected glands. Here, then, was a real failure to remove by vaccine a collection of pus—a failure which will surprise none of us.

The third case is that of a small boy (J. J.), with large masses of glands on both sides of the neck, in the anterior and posterior triangles, which had diminished satisfactorily in size during the first eighteen months of his tuberculin treatment, but in the last six months had made no headway, increasing, in fact, in size, on one side of the neck. On admission his temperature chart showed an abnormal range, up to 100° F. or 101° F. at night. To account for this, we searched in vain for any other focus of disease, until a slight discharge from the ear, which we had previously missed, was detected by Mr. Warren Low.

After some preliminary treatment of the ear his temperature fell, almost to normal limits, and Mr. Low, to whom we are indebted for all the surgical work in this group of cases, removed the most superficial of the glands. These glands yielded, on culture, pyogenic microbes similar to those obtained from the ear discharge. Suspecting such secondary infection, Mr. Low decided not to do an extensive operation, as would have been necessary for complete removal of the glands, but to return to this later, if necessary. Since that time, however, the boy has made very satisfactory progress during six months on inoculation treatment, and we anticipate that no further extirpation will be required.

The fourth case is that of a girl, aged 15 (L. B.), who was sent to us by a surgeon for a large mass of glands in both sides of the neck. A higher index confirmed his diagnosis of tuberculosis, and treatment was continued for six months without any improvement, but rather the reverse. On admission, a portion of gland was removed for histological examination, and Dr. Spilsbury reported it to be an endothelial type of sarcoma. In this case I have no explanation to offer of the anomalous tuberculo-opsonic indices, for although a series of bloods taken before and after massage of the glands gave but a small range of variation, and did not therefore confirm our previous opinion that the glands were tuberculous, we have as yet had no reason to suspect any other focus of disease in the body, of a tuberculous character, to account for the anomaly. Failing such a finding later, the case must remain on record as affording a rare exception to the rule of the value of the index in diagnosis. Here, again, there was only an apparent failure of tuberculin treatment.

My fifth and last case is of a different nature again. An adult woman (M. G.) was under inoculation treatment twelve months; during the first nine months, of which progress was of a negative character, the glands increasing slowly in size, and only during the last three months beginning to improve, also slowly, although her general health has kept remarkably good. At no time was there any tendency to softening of the glands. Here index and clinical features had, again, pointed to tuberculosis. Mr. Low recently advised operation, in view of slow progress, and on section of the glands we found that 80 to 90 per cent. of their substance consisted of firm caseous material, without evidence anywhere of pus formation. As regards the gland tissue remaining, Mr. Low expressed the opinion that it was of an unusually

tough fibrous character, which condition, he thought, was certainly traceable to the tuberculin treatment. This case presents us with a problem to which I can give at present no adequate answer—namely, why some such cases readily form pus and others do not. Until we get to some understanding of this and other problems connected with tuberculous glands, we shall probably continue to treat some of these cases without success.

Dr. StClair Thomson: The very brief contribution I have to make to this discussion will be founded on my experience of vaccine therapy in affections of the upper air-passages. I can further circumscribe my remarks by dealing chiefly with two disorders—lupus of the nose and throat, and suppuration in the accessory sinuses of the nose. Yet these two infections—one chronic specific and the other chronic pyogenic—would appear to offer a good field both for beneficial results and for fairly obvious observations. Lupus is readily recognized on the mucous membrane, and pus in the accessory sinuses of the nose generally makes its presence only too disagreeably evident. Still, "right here," as our American cousins would say, I would offer a protest against accepting the evidence of cure of either disorder, except from a trained expert after a complete examination. Such loose statements as that "lupus nodules ceased to appear" or "the nasal discharge ceased" are valueless. Lupus of the mucous membrane will not only improve under various treatments, but it will heal up and remain cicatrized for years in some cases under no treatment at all. A mucous membrane may, to the uncritical eye, appear free from lupus, and yet the trained rhinologist, aided possibly by the use of adrenalin, as I have suggested, may detect minute pin-head deposits beneath an intact epithelium. Again, in regard to nasal suppuration, we must first rule out of court recent infections. The large majority of cases of acute sinusitis get perfectly well without anything more than symptomatic treatment, and I would lay stress on the point that by a recent case I mean any case within some months of its origin. I have published the records of carefully observed instances in which pus has persisted in a maxillary sinus for three months, and then disappeared spontaneously and permanently. And in the case of undoubted chronic sinusitis, we must not forget that it is not uncommon for the suppuration to become so latent that the patient

may think he is cured, and the pus may escape detection except by those specially trained and interested in this work, or that it may only be intermittently present in one sinus, or may cease to collect, and yet go on smouldering in another cavity, from which more generalized multi-sinusitis may take place from time to time.

I have to apologize for the scantiness of the actual *pièces justificatives* which I have to submit to the Society. Most of my work has been carried out in the stress and strain of the out-patient department, where scientific observations are so frequently lost from the inability to take careful notes. I cannot give the exact number of lupus cases which were treated by tuberculin; there were probably five. I have fairly complete notes of one (L. C.), who, between October 29, 1906, and February 19, 1907, had ten injections of $\frac{1}{800}$ mg. under opsonic control. Fresh crops of lupus deposits occurred after each injection, so that the patient begged to be allowed to abandon them and to return to the intermittent treatment with the galvano-cautery, which has kept her malady in check for ten years. Other lupus cases gave very similar results. No case was even improved, and some of them even appeared to be aggravated.

Of cases of suppuration in the accessory sinuses I have notes of six in which it was tried. In one case, and one only, was it associated with improvement, and this was a post-operative case. Ten days after operation on ethmoidal and maxillary sinusitis there was thrombosis of the internal saphenous vein, with fœtid crusting in the operated nostril, and double suppurative otitis media. The nasal pus showed the pneumococcus in large numbers, and the patient received the stock pneumococcus vaccine (25 to 150 millions). But it is right to point out that this patient on her return to bed after operation had a profuse hæmorrhage from the operated nostril, requiring plugging with gauze and adrenalin, saline injections, and strychnine. The risks of septic absorption after nasal plugs are so well known that they are never used if they can possibly be avoided; but, still, such complications as those described, have occurred before nasal operations, and have quite disappeared without vaccine treatment. But in the other five cases, of which I have fairly complete notes, there was not even a trace of any objective improvement. Patients were vaccinated with staphylococcic, streptococcic, and pneumococcic vaccines. Some received the treatment before operation, and other cases were given it after drainage of the sinus had been established but had failed to check suppuration. In both

classes the results were equally disappointing. In one case of multi-sinusitis in which the frontal and maxillary sinuses were operated on successfully, but pus continued to be secreted by the ethmoidal and sphenoidal cavities, cultures carefully collected from intranasal pus sometimes showed a growth of streptococci in pure culture, on other days a pure culture of pneumococci, and on repeated occasions the inoculated media remained quite sterile! Now surely such an observation as this should give us pause. It refutes the excuse often made for vaccine therapy in nose and throat work that it is the mixture of infection that accounts for failure. It indicates that suppuration in a sinus may one day be entirely associated with one organism, on another day with an entirely different coccus, while the same cavity may on a third secrete a pus which is germ-free. It would look as if the organism, the original enemy, had in these chronic suppurations become a mere casual camp-follower. If the bacteriologist is to be the diagnostician, as Sir Almroth Wright suggests, this patient is in a parlous state. He might be told one week that he had a streptococcic sinusitis; the next week a pneumococcic sinusitis; and in the third week, although his nose was full of pus, he might be told he was quite well!

It is a difficult and a thankless task to prove a negative; but, as we have been invited to-day to discuss the limitations of vaccine therapy, I think it right to record that in rhino-laryngology my own experience is so disappointing that we cannot yet preach its value as a truth to those that eddy round and round. This is important; for, unfortunately, many of both the profession and the public are so pleased with the attractive theory of the method, and so delighted with the short cut it promises to both diagnosis and treatment, that sound and reliable clinical procedures are being neglected. I come across old patients of mine with purely neurotic vasomotor rhinitis who have passed through the inoculation phase simply because their medical attendant sent a swab of nasal mucus to a laboratory which reported the presence of staphylococci—an organism present in the vestibules of every nose in this room. Cases of suppuration in a maxillary sinus disappear from my ken to pass under the needle of the vaccinator; they return uncured, and in the time lost in neglecting surgical relief the pyogenic process may have spread to other cavities. Infections of the lower air passages, solely due to the stagnation and putrefaction of nasal secretion from merely mechanical causes, are submitted to a vaccine treatment which

may be longer and more tedious, and is certainly more uncertain, than the methods we already possess. If cases do not return because they are cured by vaccine therapy in other hands, no one would more gladly hear of them than myself; for the disappointing recurrences in lupus, and the anxieties associated with sinus surgery, would make simpler and surer methods most gladly welcomed. Surgical methods are at present not only more effective but generally more speedy than vaccine therapy. Patients cannot afford to keep a medical gardener permanently to look after their nasal flora; what they want is a plumber, who will put things in order with a greater chance of permanency. It is a delight to hear of its success in other departments of medicine; in that of laryngology, my only regret is that if it is to be, it is not yet.

Royal Society of Medicine

May 25, 1910.

Sir WILLIAM S. CHURCH, K.C.B., President, in the Chair.

Discussion on Vaccine Therapy: its Treatment, Value, and Limitations.[1]

Dr. W. HALE WHITE: My experience of this subject is entirely clinical, and it is limited to some sixty patients, excluding sufferers from tubercle. I propose this afternoon to say nothing about those suffering from tubercle, not because I have not used vaccine methods for that disease, but because it is a malady about which it is particularly difficult to arrive at conclusions, for usually tuberculosis is a slow disease, and, further, the subject is so large that it is well to limit oneself to one part of it. I may say, before I go any further, that in none of the sixty cases have I been aware that the use of a vaccine has done the patient the slightest harm. Whenever possible the vaccine treatment has been combined with other treatments which previous experience has taught us is likely to do the patient good: for instance, abscesses have been opened.

Out of the sixty cases in which vaccine treatment has been used, in eight or ten it has certainly done no good, although it has done no harm. In some others, although it did no harm, I admit there is no proof of good; but in many of these there is a considerable probability of benefit from it. Taking those in which benefit was undoubted, I will first tell you of some in which we obtained the most strikingly good results—namely, those in which the infecting micro-organism is the gonococcus. As I said, I propose that what I say this afternoon should be entirely clinical. Here is the case of a woman, aged 49, who had a multiple

[1] Second meeting (adjourned from May 23).

gonorrhœal arthritis for nine months. No treatment that she had had during those nine months had done her good. She was treated with gonococcal vaccine, and after only three injections the arthritis entirely subsided, and she was discharged at the end of a month quite cured, and has remained cured. Here is another case: A man, aged 36, had had gonorrhœal arthritis for two months. He was vaccinated with a vaccine prepared from the gonococcus derived from himself. He was given four doses, after which he was discharged quite cured, and has remained cured ever since. A very striking case is the next, and it is one to which I particularly wish to call your attention, because I think every one in this room would have admitted when he saw this woman that she would in all probability have died; and I can only say that my own opinion, standing at the bedside, is that the vaccine treatment saved her life. It is that of a woman, aged 30, who came in for very severe septicæmia, with a temperature of between 104° F. and 105° F., and had rigors. On trying to discover the cause of the septicæmia we found she had a profuse purulent discharge from the vagina. Streptococci were cultivated from that, but no gonococci were found. As the index to the gonococcus was three, it was thought probable, and I think there could be no doubt, that the original infection was gonorrhœal. So she was vaccinated with gonococci and streptococci, and the improvement was most striking. The temperature fell, and in a few days, from being a woman whom anyone would think must die, recovery set in and she was discharged perfectly cured. Another very striking case was that of a woman, aged 25, a patient of Dr. F. K. Holman, who was eight months' pregnant. He asked me to see her for multiple arthritis affecting both knees, both ankles, and several fingers. The right ankle was discharging pus, and she had fever. Considering the suddenness of the onset, and that one of the joints had broken down, I thought it was probably a pneumococcal infection; but examination of the blood made it more likely from the opsonic index that it was gonorrhœal. Ultimately gonococci were found in the discharge, she was vaccinated with gonococci, and was rapidly and completely cured. The gonococci in that case were cultivated from the pus which came from the ankle-joint. There are among my notes many other cases, but these I have given are good examples of the benefit done by vaccines in gonorrhœal cases. I am sure everyone here will admit that gonorrhœal rheumatism is frequently so intractable that when we can bring forward such cases as these it shows that the treatment has been of use.

Other genito-urinary cases which I should like to mention are some

in which the infecting organism was a coliform organism. For example, here is a good example, which again is one to which I want particularly to draw your attention. I saw the patient with two doctors well known to all of us, and I can only say the opinion of all three of us was that, when seen, the patient was a dying woman. The case was that of a lady, aged 45, who had pains and swelling in many joints, obviously severe septicæmia. The ears and the bridge of the nose were swollen, and she had double conjunctivitis, severe cystitis, blood and pus in the urine, a high temperature, and pain over both the bladder and kidneys. She was very ill and was vomiting, probably because of the double pyelitis. She was almost pulseless and had a high temperature. The urine was examined at once, and the *Bacillus coli* was found. A vaccine was prepared, and again the improvement was immediate and permanent. Thirty-six hours from the first injection the temperature fell, and she completely recovered. All the joints subsided, and when heard of a year afterwards she was perfectly well. The next example I shall select was also that of a coliform organism causing very severe cystitis and pain in the right kidney region. A vaccine was given, and, after only three injections, the patient recovered; the pain subsided after the first injection. Another case which I may trouble you with, without going into details, was one in which a young man had severe pain in his kidney, and pyuria. It was proposed to explore his kidney; I did not agree with this suggestion, for the urine contained a coliform organism. He was cured by a vaccine, and there was no operation.

Leaving the genito-urinary group, I now come to alimentary cases. One of the most interesting was a man who was invalided home for an abscess in the liver. He coughed up enormous quantities of pus, many gallons of it. He improved up to a certain point, even doubling his weight—he was practically a skeleton when he arrived—but the expectoration of pus did not cease until the micro-organism was discovered. In his case it was the *Streptococcus pyogenes longus.* A vaccine was given, and then at once, although he had been under treatment months before, the amount of expectoration diminished, and then ceased, and he is now perfectly well and earning his living. The next alimentary case is very strange, and if any one has had a similar experience I hope it will be published. The patient was a woman who had seen many doctors, and it was proposed to explore the abdomen. She had repeated attacks of vomiting, associated with high temperature. The stomach contents were examined, and were full of coliform organisms. A vaccine was prepared, and after several injections she got well. She never has

an attack now, she has gained considerably in weight, and is perfectly well and healthy. It is very unusual to find such cases due to the *Bacillus coli* or any allied organisms in the gastric contents. Under the vaccine treatment the coliform organisms disappeared from the gastric contents.

This case impressed me very much. A young man, aged 17, lay in bed, thin and wasted, having suffered from ulcerative colitis for many months. He had been seen by many physicians in London, and I think he had had every form of treatment but opening the appendix and vaccine treatment, so that we could contrast the effect of vaccine with other treatments. We did not open his appendix, but from the commencement of the vaccine treatment the temperature fell, the motions became more normal, the passage of blood ceased, and he is now well, of normal weight, and earning his living. I have vaccinated several patients who suffered from ulcerative colitis. The results are not so striking as in this case. The disease is very tedious at best, and very often several means of treatment have to be carried out simultaneously. Often it is difficult to be sure which is the offending micro-organism; so, although I believe that often the vaccine treatment has helped these patients very much, I cannot bring forward such precise proof as I have tried to do in the cases which I have already mentioned.

I have already described a patient who coughed up a liver abscess and was very much improved by vaccine treatment, and other cases of discharge of pus from the lung have also been much benefited by this treatment. One of the most convincing, on account of the unfavourable conditions, was that of an old man in an asylum. He had been an inhabitant there for many years, and he had pneumonia. When I saw him it was four months from the pneumonia, and he had signs of an empyema at the left base and was coughing up some pus. The offending micro-organism was *Streptococcus lanceolatus*. Under vaccine treatment the expectoration soon ceased; that is two years ago, and he is now well and still in the asylum. I mention this case because everyone will admit that an old man, an inmate of an asylum, is not a very promising subject when he has an empyema. Another case in the same group, of empyema being coughed up, was that of a woman, aged 34, who came into the hospital on account of expectoration of pus. Physical signs and the X-rays showed that the empyema was just over the upper part of the right lung. Although she had been coughing up quantities of pus, it diminished very rapidly under treatment. A coliform organism was cultivated from the pus, vaccines were given, and the

striking thing was that after the first injection with the vaccine the temperature began to fall and she never looked back. She was discharged perfectly well, no surgical interference having taken place. When she went out it was easy to see with the X-rays that the empyema had shrunk up and was virtually cured.

I have tried this method of treatment in some cases of bronchiectasis. Considering how difficult they are to cure, even under the best circumstances, it would be very welcome to us to be able to help them by any method. Unfortunately, I have not had any successful case. The experience of others may be different. This failure no doubt is largely due to the fact that the flora of the bronchial expectoration is very varied indeed, and possibly we have not succeeded in isolating the offending micro-organism.

Two instances in which patients seem to have been snatched from the grave are two puerperal cases which I should like to mention to you. One was that of a woman aged 27. I saw her for general puerperal septicæmia and septic pneumonia. She was so near death that it seemed probable that she would not live until the offending organism could be isolated and the vaccine prepared. But we did get some sputum, which was sent to the laboratory, and a coliform organism and a pneumococcus were cultivated. She was vaccinated with both, and the improvement was immediate and remarkable, because before we began she certainly appeared to be dying. After the first injection the temperature soon became normal, and, as far as I know, she is perfectly well to-day.

Another case was that of a woman, aged 30, who had puerperal septicæmia with acute nephritis and blood in the urine. From the catheter specimen the *Streptococcus aureus* was cultivated and a vaccine given. But she did not get better, and then coli organisms were found in the vaginal discharge; she was vaccinated with that also, and again the response was immediate, for the temperature in a few days became normal, she made an uninterrupted recovery, and she is now well. Those cases illustrate the benefit which may occur in puerperal cases.

The last case I shall mention was very dramatic. The patient was aged 30, and had been seen by many physicians. She was taken ill on February 24, with arthritis in the right shoulder, and a temperature of 105·6° F. It soon became clear that she had general septicæmia, following upon a small scratch on the hand. She rapidly developed severe septicæmia; there was arthritis in many joints, the

temperature was raised, and remained at 103° F. to 104° F. for a month; she had various swellings about the body, and at the end of a month a swelling appeared in the middle of the right arm. It was incised, and much serum oozed from it. This contained a pure culture of *Streptococcus pyogenes*. A vaccine was prepared, and, although her temperature had previously been raised for a month, within forty-eight hours of the first vaccination the temperature fell, it never went up again, and she is now well. Her husband, a doctor, writes: "The immediate good result after using a special vaccine ought to convert the most hardened sceptic."

I am not myself a bacteriologist, and I am a sceptic, so I think I have approached this subject with an unbiased mind. I can only say I have come to the conclusion, after using vaccines pretty largely, that, given in the doses which are now employed, I have never seen harm follow. I have sometimes seen cures when other modes of treatment have failed, and in some of the cases I have mentioned the patients seem to have been literally saved from death by vaccination. I am sure it sometimes helps other modes of cure; and sometimes, unfortunately, in some of the cases in which we should like most to use it, it is inapplicable, because the offending micro-organism cannot be found. But that, I think, should not deter us from trying to find the offending micro-organism by every means which the bacteriologist can devise; and when he has found it, we should use a vaccine where previous experience has taught us that some good is likely to follow.

I have selected these seventeen cases to relate to you, for I wished to bring forward examples in which the benefit following vaccination appeared so soon after vaccination, and was so great that all who were watching the cases were convinced that the improvement was due to the vaccine.

Dr. J. W. Eyre: I take it that Sir Almroth Wright, in his opening remarks, gave such a comprehensive survey of the principles of vaccine therapy that those who follow him in support of the views he put forward are relieved from the necessity of dealing with the general principles, and are free to devote themselves to contributing to the discussion observations which have occurred in their own experience, and which have struck them as being of importance. There is one thing I would refer to before mentioning my own impressions, and that is with regard to the relations existing between the clinician on the one side and the bacteriologist on the other. Sir Almroth Wright

explained that the bacteriologist was now "coming into his own"; in other words, that the bacteriologist was taking a definite place in the treatment of disease as well as in diagnosis. But I do not think the opener wished to imply that the bacteriologist was competent to take entire charge of the patients, at this early stage since his emergence from the seclusion of the laboratory. It seems to me that for the present, at all events, and until the new generation of students who are now being trained have grown up and come out into the world, that the clinician is as essential to the patient as the bacteriologist, and vice versa. That is to say, the presence of the clinician is essential to the patient because of his experience in the treatment of symptoms, for the bacteriologist has not yet had time to accumulate clinical experience of the human subject; and although it is from animals that we have derived most of our knowledge of how to apply vaccine therapy to the human subject—for the work which has culminated in the treatment of man by vaccines has been based upon exact experiments on animals in the laboratory—there are many respects in which the laboratory animal differs from man besides size and weight.

I do not propose to say anything about the value of vaccine therapy, because that seems to me to be a self-evident proposition; and, although there are still some conservative members of our profession who adopt a sceptical attitude in the matter, their number are decreasing rapidly, and one or two are being converted every day.

The point, however, which particularly impresses me is the way in which vaccine therapy is being applied in the special branches of medicine. Dr. Hale White has given you his experiences in a wide selection of cases in general medicine; but, in addition to the cases which are met with in general medicine where vaccine therapy has proved of such value, if one turns to some special department of practice, such as ophthalmology, we have a notable example of the superiority of imitating Nature's method of cure over the methods that have been in use in former times, when the removal of diseased tissue was attempted by drugs and operations. Take, for example, tuberculosis of the conjunctiva—a condition in which I have been particularly interested for many years, and which, in spite of operations repeated time after time and all sorts of local medicament, one could never hope to cure permanently, at any rate without considerable distortion. In the past two or three years seven or eight cases of tuberculosis of the conjunctiva have come under my notice, and have been treated with tuberculin, and with tuberculin alone. In every instance the

result has been splendid; and the whole of the diseased tissue has been removed naturally and without distortion. After six to eight months' treatment in each instance a normal conjunctiva, with no trace of disease, has been obtained. In every case it may be mentioned that the diagnosis has been made absolute by bacteriological methods, including inoculation. Again, in the old days one used lotions of various sorts for simple conjunctivitis. Some of the patients went on for months and even years, but now in chronic cases, due to the *Diplobacillus lacunatus*, very rapid and complete recovery is insured by the help of vaccines. In acute streptococcic conjunctivitis vaccine therapy is by far the most useful agent we can employ; whilst gonorrhœal ophthalmia, which formerly had such terrible results, can be efficiently controlled by vaccines, and practically by those alone. But here a word of warning is necessary: unless care is used and the injections controlled by the estimation of the opsonic index in gonorrhœal ophthalmia, definite harm may ensue. I have a very vivid recollection of a case of gonorrhœal ophthalmia which I treated with gonococcus vaccine, but one injection of the vaccine was given at the wrong moment, and practically all the cornea was lost, so that in the end the result was as bad as if the case had been treated in the ordinary way by lotions and drops. Another class of case in ophthalmological practice, to which my attention has been recently drawn, is orbital cellulitis associated with proptosis. In the old days this condition was usually said to be due to specific disease. Mercury and iodide of potassium were given, but often with no obvious results. Upon the investigation of these cases by bacteriological methods, and the recognition of the fact that there may be cellulitis or periostitis of the orbit, due to bacterial infection, the aid of vaccines is invoked, and it has been found that one can influence a large number of these cases in a favourable way, and the improvement in some of them is very striking indeed.

Another special branch of medicine in which vaccine therapy has scored heavily is dentistry. Goadby and other workers have investigated such conditions as pyorrhœa alveolaris, and have shown that even vaccine alone is capable of doing a large amount of good of a lasting character—much more than local surgical measures, long continued, are capable of effecting—while the combination of vaccine therapy and local treatment achieves results little short of marvellous. This is the more striking because pyorrhœa alveolaris is a condition which is exceedingly chronic, and after local treatment is liable to bad relapses.

In genito-urinary diseases vaccine therapy has come to stay: some instances of its application have already been detailed by Dr. Hale White. Puerperal septicæmia, as you have already heard, is a condition which has, on several occasions, yielded to vaccine treatment after the failure of antitoxic sera. In such a severe infection, where the case is not usually seen until late in the disease, it would not be surprising if vaccine therapy failed, because one of the chief limitations to vaccine therapy is the one which Sir Almroth Wright had on the board as the fourth—namely, that imposed by the resisting power of the patient. That is to say, if the resisting power fails, and especially if it falls to such a point that it no longer responds to the stimulus afforded by a vaccine, obviously vaccine therapy is of no use in that case. Often we do not see cases of puerperal septicæmia until the resistance of the body is at its lowest. But the fact that we are able to obtain a measure of success in this fatal form of disease is another instance of the success of vaccine therapy in a special department.

Again, in throat diseases, such as tuberculosis of the larynx, there is a notable yielding to treatment with tuberculin when intelligently carried out. But here is a point which has been forced upon me in many cases, which may be common experience, or may be noted only in isolated examples; but I have found that in tuberculosis of the larynx this local focus will clear up and the patient get a respectable voice again, and yet when, as so often happens, the lungs are infected as well, the tuberculous process in these organs will go on more quickly than before. In other words, the local resistance is improved, but the general resistance is lowered. One notices the same thing in joint diseases associated with phthisis. When the patient is suffering from a tuberculous joint and tuberculous infection of the lungs, very frequently the joint clears up completely; but the lung condition, so far from being favourably influenced by the tuberculin, actually progresses.

Finally, a word with regard to vaccine therapy in colitis. I have paid considerable attention to the disease, and I was rather surprised by a remark of Sir Almroth Wright's with regard to diagnosis—and by diagnosis I mean, of course, the recognition of the offending organism from amongst those isolated from the case. His suggestion was the administration of a cathartic and the estimation of the opsonic index to the various bacteria before and after an action of the bowels; for as his diagrams showed, very often one gets only small movements of the index with many organisms, and it may be necessary to carry out

an extended series of estimations before sufficiently widely divergent indices are recorded to enable any particular bacterium to be picked out as the offender. It seems to me that, in any but very mild cases, such a method is quite out of the question, because of the danger from hæmorrhage which may follow such a proceeding. The diagnosis of the infecting organism can, I believe, best be carried out by searching for specific agglutinins. In many of these cases the organism at the bottom of the trouble is either the colon bacillus or a member of the same group, and in all the infections I have met with so far, where such has been the case, there has been developed a sufficient amount of agglutinin in the patient's serum to enable one to pick out the required organism with certainty. But in other cases other organisms have been responsible, such as streptococci and pneumococci. These, however, usually present no difficulties. Again, with these colitis cases the necessity for an exact and absolute diagnosis, from the clinical as well as the bacteriological standpoint, is one of the very greatest importance. Given a case of simple bacterial infection of the colon causing colitis, the results to be obtained by vaccine therapy are simply wonderful. But many cases of colitis are really due to some condition which may escape observation if not thoroughly investigated. One such case occurs to my mind. The colitis, although secondarily bacterial, was undoubtedly primarily due to chronic constipation resulting from a band across the colon, which was only demonstrated during an X-ray examination. Vaccine therapy might have been carried on in that case for years with no real benefit, while the operation for the removal of the band resulted in the disappearance of the constipation, and the colon quickly recovered its normal condition.

Dr. Harold A. des Vœux: It is very difficult for an ordinary practitioner to make up his mind, as he often has to, in a hurry as to the value of a new method of treatment. Our patients, who worry us on so many subjects, are always most urgent and insistent that we should give an opinion within five minutes on every subject. They want to know whether this treatment or that is the right thing, and they add, "Surely you can tell." And we are in the position that, if we want to keep any practice at all, we must pretend to know much more than we do. I do not think that is maligning the profession, and I know it is true of some of the men whom I come across. I once said to a lady to whom I was almost a stranger, "I do not know," and her answer was, "Well, I had better get somebody who does know." I

became rather angry and left the house. She sent for another doctor —perhaps wisely. He of course knew. We have to make up our minds on these questions, and even with time and thought it is difficult to do so; yet we are often driven into a corner and are bound to give hurried opinions. We are very apt to judge of the value of a treatment by the first cases in which we see it tried. We may see a sudden and remarkable result, or we may come across cases in which the treatment has been tried in the last stages of a disease and the patient has gone from bad to worse. For instance, we may have seen a vaccine employed in a hopeless case of tuberculosis, which would incline us to say that the vaccine treatment was no good. We may have tried unsuccessfully a vaccine in Malta fever or acute septicæmia, and we would have formed an opinion accordingly; and once a man in our profession has committed himself to a statement, whether favourable or unfavourable, it is difficult to retract. It does not do to change an opinion. I heard one man say he never changed his opinion; and perhaps he was wise, for he was very successful.

There are two groups of cases in which the vaccine treatment has been used—in one very successfully, and in the other more or less unsuccessfully.

In the first group—of which staphylococcic infections of the skin (boils) are a type—inoculation is most successful. The results obtained by so many practitioners in that kind of case have had much to do with the more extended use of the treatment in other diseases. I have treated over one hundred cases of boils with vaccines, and since doing so my method of treating them has altered completely. I shall not go into statistics, but before I took up this treatment my ordinary practice, on seeing a patient with a hard, brawny, painful boil, was to advise early and free incision under an anæsthetic. On the whole it was a successful treatment, but disagreeable and sometimes prolonged. Since I have taken up the vaccine treatment I have never had to give an anæsthetic. But even now, when I find a boil in a softened condition, I make a small incision into it to evacuate the liquid pus, and in this detail disagree with Sir Almroth Wright. I will only mention one of the cases I have seen. A man came to me from Chili in June, 1908. He had been suffering from very bad boils for four years, they having followed an attack of typhoid fever. He came to me immediately after the voyage and said he was much better, but he wanted vaccine treatment. I said, "You had better wait to see whether the voyage has cured you." He said, "I have only six months in England, and I want to be cured."

However, he waited, and after three weeks he developed a boil—an enormous one, 4½ in. by 3 in. Over a certain part of his body he had quite a Clapham Junction of scars from old incisions. He had three doses of vaccine only, and was cured. He had no further attack in England, and went back to Chili in the following September. In August, 1909, he had a slight relapse, for which he again employed vaccine. I received a letter from him this week, in which he reports that he has been well ever since. That may be a coincidence, but I think there is something more in it. It may be regarded as a coincidence of time, but my own belief is that it was a sequence of treatment and result.

I now come to the other group of cases, in which the disease is generalized from the first. Dr. Colebrook showed you on the last occasion the charts of the opsonic indices in a typical case. That patient, a young physician working in the laboratory of St. Mary's Hospital, came under my treatment when he developed that dreadful disease of which he died—glanders. He was suffering from fever of a mild type, he was not very ill, and I thought that his disease was influenza. The patient himself made up his mind from the first what was the matter with him, and after a few weeks the diagnosis was made from his blood. There was no focus of disease and no local symptom for nearly two months after the beginning of his illness, and then there was pain in the spleen. He had the advantage of the help of all his colleagues in the laboratory, and everything possible was done for him. His index was taken every day, his temperature recorded every four hours, a close watch was kept on his condition, and he was treated on the best advice procurable in London. I think one can truly say that the vaccine treatment had no effect on him from beginning to end. The disease varied a great deal. Sometimes he was much better, and, for a period of a month or two, he seemed to be nearly well. Still the disease kept breaking out, and he lingered for eighteen months; his spleen slowly enlarging throughout, his only focus being a small inflammatory patch on the front of the leg, which cleared up after incision. Those are two extreme types of cases. What is the difference between them? The boil from the first may be severe, but is a local infection; the other may be mild, but is a general infection. The disease in one case cannot stimulate the blood to create sufficient antibodies to destroy the microbes, and what antibodies are formed cannot reach the disease, because the boil as a rule forms a wall round itself, which prevents due interchange between it and the circulation. A

vaccine in such a case introduced into a healthy part of the body will immediately stimulate the creation of antibodies, and what blood does reach the boil will be powerfully destructive of germs. In the case of a general infection, germs are introduced almost direct into the circulation and distributed all over the body, and therefore stimulate all the tissues they reach to create antibodies. But in that case what seems to be wanting is not stimulation to create antibodies, but a lack of power of the tissues to create them. Therefore it was reasonable to expect that a vaccination treatment will not be so successful in that type of case as it would in the local disease. It does succeed in certain cases, but in some it fails.

I am watching another case which I believe is very similar, but of milder type—namely, one of chronic staphylococcic poisoning. The patient is a delicate woman who has been treated by different physicians, including myself, for many years. Some said that she was probably tuberculous; but there are no signs of it, and her blood when examined was normal in reaction to tubercle. She has attacks of malaise, nausea, giddiness, and very slight rises of temperature to 99° F. or 99·5° F., and with these attacks there is often an outbreak of minute acne, like pustules on the face. Dr. Matthews kindly tested her blood against staphylococcus, and found that she had an index varying from 0·5 to 1·3, and under vaccine treatment she improved slowly for a time, but she is now as bad as ever. At present she is under the treatment of a well-known dermatologist, who is convinced that her trouble is due to disorder of digestion. After six weeks' treatment she tells me that she was worse last week than she has ever been before. After a very long day of fatigue she had a very severe attack, and was compelled to keep to her bed, and felt the same sensations as she experienced after an inoculation; then she rapidly improved, and her spots suddenly disappeared. I think that was a clear case of auto-inoculation, due to over-fatigue; during the negative phase she was forced to keep to her bed, and during the positive phase she rapidly recovered. To-day her complexion is natural, and I think that if I treat her carefully, watching the opsonic index, I shall eventually cure her; but it is a very difficult case, and will require great attention to details, the neglect of which accounts for my previous failure.

I attended a boy two or three years who had very acute attacks of middle-ear disease, high fever, great pain, delirium at times, a sudden discharge from the ears, and then relief. He had five or six of these

attacks every year. I took him to an eminent aurist, who thought the attacks were due to adenoids, and that if they were removed he would recover. They were taken out, and the boy remained well five months. After that the attacks came back, and he had three or four acute ones in six weeks. Then the late Dr. Wells saw him with me in one of the attacks. There was a clear discharge from the ear, in which there was a pure growth of streptococcus. He was vaccinated with that germ, and has remained well ever since, though the treatment was commenced two and a half years ago. That, I believe, was not a coincidence in time, but a sequence of events.

I have treated about twenty cases of chronic bronchitis, and a striking case was that of a lady whom I attended in her fifty-fifth year for a very severe attack of measles. During that attack she developed acute bronchitis, and made a very slow convalescence; but she never was quite rid of her bronchitis, except for short periods in warm weather. In February, 1908, I asked her if she would allow me to treat her with a vaccine, confessing to her that I had never treated bronchitis previously in the same way. She said "Yes," she was willing to try anything. We got her sputum, a vaccine was made from it by Dr. Freeman at St. Mary's Hospital, and I vaccinated her. Within three days she was very much better, and in a fortnight she was cured. There has been no other treatment. That was nearly two and a half years ago. Last winter she developed acute influenza, but no bronchitis. I gave her, however, a dose of her vaccine as a precautionary measure. Again, is this case a coincidence of time? Am I unjustified in calling it a sequence of events?

I will now relate an interesting case of appendicitis. I thought it would be instructive if I could, during the attack, find out the offending organism. So, following the method which has been employed in colitis, I obtained a specimen of fæces containing mucus, which was examined by Dr. Fleming. Several germs were isolated, but there was one coliform germ to which this patient's blood was continually on the move; sometimes a high index, sometimes a low one. A vaccine was made from it, and I put it to the patient whether he would have vaccine treatment as an experiment, or whether, as this was his second attack, he would have the appendix removed. I advised him to have it removed. He agreed, and Mr. Arbuthnot Lane operated. At the operation Mr. Lane said there was a smell at the seat of trouble which made him suspect that it would suppurate, and he took the precaution of inserting a drainage tube. The appendix was placed in a sterile bottle, and in it

Dr. Fleming discovered in pure growth apparently the same coliform germ. The wound did suppurate, and acute fever developed. The vaccine being ready, no time was lost, and there ensued another coincidence of time! It is interesting to speculate whether a course of vaccination could have cured the appendicitis without operation.

I will contrast that with a case which I have at the present time under my care, that of a lady from whom a fibroid of the uterus was removed five or six years ago. She was concurrently affected with what was thought to be Paget's disease of the nipple, but, as it seemed to dry up with simple treatment, we did not take very much notice of it. Three months ago she came with an evidently malignant growth of the breast, which was removed. I thought it would be interesting to find out what germ was associated with Paget's disease, so, under aseptic precautions, I obtained a piece of the breast not involved in the disease, placed it in a sterilized bottle and sent it to Dr. Matthews, who examined it. It produced a tremendous growth of streptococcus. Unfortunately, I did not have a vaccine made, as I did not anticipate danger; but in a few days there was a fearful explosion—high fever and suppuration of the whole wound. She is still ill, although ten weeks have elapsed since the operation. There has been a profuse discharge of pus, and the wound was reopened four weeks ago from end to end. But even a fortnight ago suppuration broke out again in a new spot. I contrast that strongly in my mind with the previous case, and ask whether, if I had had a vaccine made for it, she would not have made a rapid recovery? It has taught me a great lesson, and I shall never miss the opportunity again.

I should now like to speak of influenza. I have been vaccinating cases of influenza for nearly three years, and, as far as the treatment of the acute influenza is concerned, it is very difficult to say anything about it. We have for many years tried different treatments—antipyrin, bicarbonate of potash, salicin and aspirin, &c., &c.—and have obtained very much the same results whatever we tried. Most cases recover in a few days, but some few have had distressing symptoms and complications, and some were simply tedious and long. I do not think we can foretell which cases are going to do well and which will do badly. So I commenced by vaccinating only in the prolonged and unsatisfactory cases, and obtained some very good results and some which were doubtful.

The usual effect of vaccination is that there occurs in a few hours a negative phase in which the previous symptoms are exaggerated and

accompanied by a rise of temperature. And I am fairly certain that, if an effective dose of vaccine is given and no response follows, the case is not one of pure influenza. In fact the vaccine is diagnostic of influenza as tuberculin is of tubercle. It took me some little time to discover what was the effective dose, and I finally came to the conclusion that 15 to 20 millions was right in most acute cases.

In the cases where no response occurred I began to have the sputum carefully examined, and found that many of the cases which did not do well were not influenza, not due to the specific germ, but were apparently due to some other germ or germs (mixed infection). I can mention two cases of that type which occurred to me in one week. Both cases occurred in ladies well past middle life, who were subject to influenza and thought that their present attacks were influenza. One of them told me that the day before she was taken ill she stayed at a small inn where there was a disagreeable smell. She had what seemed to be typical influenza, with a temperature of 103° F. to 104° F., and was very miserable and depressed. I gave her 20 millions of influenza vaccine, but it had no effect. I obtained some sputum and a swab from the throat, and a pure streptococcus was found, but no influenza bacillus. The other case was very similar. She was suddenly taken with very acute pain and fever one night at half-past ten. She believed she had influenza, and so did I. On seeing her next morning I gave her 18 millions of influenza vaccine, but the day following her condition was unaltered, and a swab and sputum showed nothing but pure pneumococcus. I gave her 15 millions of pneumococcic vaccine (stock), her temperature being 102·8° F. The effect was marvellous. She had a severe reaction for about three hours. After a rise of temperature to nearly 105° F. her pains began to go, she fell into a beautiful sleep, and next morning she awoke with a temperature of only 99° F. Again, was this case a coincidence of time?

I have used influenza vaccine in a few chronic recurrent cases, one of which I will mention. A man, aged 53, came to consult me in October, 1908. From 1900 onwards he had yearly six to eight attacks of influenza with slight fever, great prostration of strength, and severe mental depression with suicidal tendencies. In the spring of 1907 he gave up all work and travelled in the East for a year, remaining well while away. Returning to England in June, 1908, he was ill with influenza in less than a fortnight, and reported three further attacks before I saw him. He was then just recovering from an attack, and I vaccinated him with ten millions. This made him ill for three days; so

much so that he was reluctant to have another dose. He was afterwards regularly inoculated, and has remained in good health, although he thinks he has had one or two very slight attacks. He has been able to resume all his work and his former life in every respect. Unfortunately it has been impossible to obtain bacterial proof of his disease.

I always have by me a stock pneumococcic vaccine, but, until I came across the following, I have thought my cases of pneumonia were too mild to need it. But this was a typically severe pneumonia. On a particular Monday evening a man, aged 42, began to feel ill. He was a heavy drinker, and therefore a bad subject. On Tuesday midday his doctor saw him and found him with a temperature of 104° F. and a pulse of 130, and he was very ill. I saw him next day at three in the afternoon; his temperature had not gone below 104° F., nor his pulse lower than 130. The liver was below the umbilicus, and the skin of the face was congested and had a yellowish tinge. There was loss of resonance over left lower lobe, and fine crepitation and commencing tubular breathing. His tongue was dry, and altogether it was a very bad case. He was given 20 millions of the vaccine on the spot, and in two hours he felt better. His temperature again never rose above 103·5° F., and in three days he was practically well. There are many people who will say that was another coincidence of time; I feel almost inclined to say it myself, but surely it was more a sequence of events. Here was a pneumonia only commencing in a patient of a bad type, in which the crisis occurs on the fourth day; not a complete crisis, for the temperature rose for a few hours to 103·5° F. on that day, but was never over 100° F. after.

If those cases were coincidences of time, there was nothing in the vaccine treatment; if they were a sequence of events, then in each case there was a wonderful effect from the treatment; and I could, did time allow, relate many more striking cases. I can vouch for the facts; I have watched all the cases. I am convinced, after watching these and other cases for the last four years, that the vaccine treatment has come to stay. I was extremely interested in Dr. Hale White's cases, and the only difference between us is that I think we ought not to postpone vaccine treatment until every other treatment has failed. My own belief is that we should try it in every case which we think will be severe, and try it at once. It is not fair to try it only where the patient is *in extremis*. We do not postpone operation until a patient is dried up and moribund: if we think a patient will get acute peritonitis, we operate at once; if we suspect diphtheria, we inject the serum before

we are sure of our diagnosis. If we are to make a success of vaccine treatment we must begin early, and do it faithfully, with great attention to details, carefully watching the effects of each dose. It is not always an easy treatment; in fact there are many difficulties as to size and spacing of doses; some patients are very susceptible, and will only bear small doses, whereas others not only will stand but require enormous doses.

Mr. H. W. Bayly: As Sir Almroth Wright has pointed out, the value of vaccine treatment for staphylococcic infections seems established, and also the prophylactic value of antityphoid and antiplague inoculations seems considerable, though not nearly as absolute and complete as in the case of vaccination against smallpox. It appears to me a remarkable thing that, after seven years of very extensive trials all over Europe, the opinion as to the value of tuberculin treatment of tuberculosis is still so divided. It is obvious in a disease like tuberculosis, so prone to sudden changes either for better or worse, that the result of any method of treatment must be scrutinized with the greatest possible care.

Four years ago I had the privilege of working for several months in Sir Almroth Wright's laboratory, and shortly afterwards Dr. Latham was kind enough to appoint me his clinical assistant at the Brompton Hospital for Consumption. One day a man applied for admission to the Hospital. He had a five-lobe infection, and the larynx and one hip-joint were also involved in the tuberculous process. He was obviously an unsuitable case for hospital or sanatorium treatment, and death appeared inevitable in a very few months at longest. I was unsuccessful in obtaining his admission into a Home for the Dying, but a charitable lady of my acquaintance kindly offered to support him in a cottage in the country till such time as I could obtain his admission into a Hospital for Incurables. Within six months he had put on 3 st. in weight, and now, after more than three years, my charitable friend is still supporting him, for though a different man, the disease is not eradicated, and he is unable to undertake hard work. He has never had tuberculin treatment. Sir Almroth Wright said that improvement in desperate cases might be taken as evidence as to the value of treatment, and therefore I imagine that this extremely desperate case, if he had been subjected to tuberculin treatment, would have been triumphantly brought forward in proof of the value of such treatment.

I only quote this one case of many similar ones in order to emphasize Dr. Slater's remarks as to the extreme importance of a critical and

unbiased examination of all evidence. In any investigation of the value of vaccine treatment one should obviously take every precaution not to confuse *post hoc* with *proper hoc*. Personally, I am a firm believer in the value of therapeutic inoculation, and have seen excellent results follow the vaccine treatment of staphylococcal, streptococcal, gonoccocal, pneumococcal, *Bacillus coli* and *Bacillus influenzæ* infections. I have not been so impressed with the value of tuberculin as with the other vaccines that I have mentioned.

May I mention two cases in which it appeared to me that the improvement was almost certainly due to the vaccine treatment? One was an old gentleman, aged about 80, who had an acute coli infection of his bladder. Rigors were frequent, and the patient appeared to be *in extremis* when I first saw him, and indeed the medical practitioner in charge of the case did not expect the patient to live many hours. I considered that the case had gone too far for vaccine treatment, but was urged by the relations to give the patient the benefit of a forlorn hope. I had previously prepared a vaccine from an atypical *Bacillus coli* isolated from the urine. After the first dose of vaccine the temperature came down, and no rigor occurred for three days. Then I gave a second inoculation, and a week later a third. No rigors occurred after the second inoculation, and the patient recovered. The other was a child, aged 12, in which phthisis was suspected. There had been cough, expectoration, languor, and loss of weight for several months, getting progressively worse. The sputum was examined on six occasions. Tubercle bacilli were never found, but large numbers of influenza-like bacilli were always seen. A vaccine of this organism was prepared and inoculated, and after the first dose the child was decidedly worse. I had some difficulty in persuading the father that this probably indicated that we had got hold of the right organism, but the wrong dose. My advice to give the vaccine another trial was, however, taken, and the dose was reduced. After six inoculations at weekly intervals the child was completely cured. With streptococcal septicæmia and gonorrhœal rheumatism recovery has followed the vaccines and has apparently been due to them.

In regard to the comparative value of the administration of vaccines hypodermically or by the mouth, it is, I think, generally accepted that it is much easier to immunize animals hypodermically than by the mouth. That some toxins are destroyed by the digestive juices seems certain, and the amount absorbed during passage through the alimentary canal seems to vary greatly, and it is difficult to calculate how much will be absorbed.

There seems to be no possible advantage in the by-the-mouth over the hypodermic method from the point of view of the immunizator, and, in view of the greater accuracy of dosage obtained by hypodermic inoculation, the slight advantage from the point of view of the patient's comfort obtained by oral administration seems hardly worth considering. I think that perhaps better results may be obtained with vaccines sterilized by an antiseptic rather than by heat.

The medical world seems still to be divided on the question of the value of the opsonic index as a guide to the patient's resistance to the organism against which his serum is tested. One would hardly expect that the complex riddle of immunity, in which so many varying factors take part, should be completely cleared up by the examination of the serum alone. Surely the cell is the beginning of things, and the protective bodies being produced by the cell, the cell itself, and not the product of the cell, must be the ruling factor. In view of the fact that fatigue is well known to lower resistance to infection, whereas several observers had stated that fatigue had no effect on the opsonic index, I undertook two years ago a short series of opsonic indices in women at the conclusion of the second or third stage of labour. In each case I took the tuberculo-opsonic index twice, using for one examination the patient's own cells, and for the other my own cells. I found that in nearly every case the opsonic index was normal when my own cells were used, but below normal when the cells of the fatigued woman were used.

I think that it has been shown that toxins and poisons generally exert an inhibitory action on the phagocytic power of the cell. The opsonic technique is such that it is extremely difficult to entirely eliminate all sources of error, and personally I am not, after considerable experience in the method, satisfied that my results are always consistent or reliable. And I have taken every care, and have done my best. My results with the pneumococcus, gonococcus, and *Micrococcus catarrhalis* have been most unsatisfactory. I consider small excursions of the opsonic index as entirely valueless for diagnosis or as a means of controlling treatment.

I can but admire and envy the technical skill of the workers in the laboratories of St. Mary's Hospital, which enables them to obtain the excellent results that they have shown us; but surely a method of examination of serum that is beyond the power of any careful laboratory worker ceases to be a practical test, although still remaining an interesting laboratory phenomenon.

Royal Society of Medicine

June 1, 1910.

Mr. A. Pearce Gould, Vice-President, in the Chair.

Discussion on Vaccine Therapy: its Treatment, Value, and Limitations.[1]

Dr. William Bulloch: In rising to take part in the discussion on the administration, value, and limitations of vaccine therapy, I feel some difficulty in deciding to which of these questions attention should be mainly directed, as it is plainly impossible in the time at one's disposal to deal effectively with all three. It seems to me, however, that one of the principal questions refers to the *value* of the method, especially as there is a difference of opinion as to how the value is to be judged and who are to be the adjudicators. From what is being widely said, there appears to be a certain degree of resentment on the part of many of those who practise medicine among their fellow-creatures that others should enter the domain which, rightly or wrongly, they consider to be their perquisite, and should enter it especially in a spirit of commercial competition. And I take it that there are two classes to be considered —viz., the clinician and the scientist. The conflict or antagonism between these two is nothing new, nor is it confined to this country. For hundreds of years the clinician has relied on his unaided five senses to carry him through his life-work; but the last twenty years has witnessed remarkable developments, which show that this simple state of affairs can no longer obtain. To many of us, evidence appears lacking that occult things which are revealed to the majority only by the aids of chemistry or microscopy are revealed to a few by means of

[1] Third meeting (adjourned from May 25).

their unaided five senses. To these unaided senses medicine is, as was stated nearly two thousand years ago by Celsus, "a conjectural art," and one of the chief problems is whether it is to remain a conjectural art or not. Scientific medicine has sunk deep foundations on which a great superstructure of facts has been raised, and it is now known, although not always admitted, that two or more things which look identical to the unaided senses may have causes essentially different. The discovery of these causes has been brought before the notice of the practitioner of medicine by the medical scientist in nearly every instance. A glance at the history of medicine shows that in the evolution of medical knowledge we have passed through various epochs. At first the concept of disease was symptomatological only, and when this was found inefficient, an attempt was made to lay a basis on pathological anatomy. In turn this has had to give way to the ætiological concept. The end and aim of all medical research is that we may create a scientific theory of disease on the basis of which a rational can be substituted for an empirical treatment. All this work, however, shows that medicine and disease are much more complicated than was formerly believed.

Now the attitude of the practical man of medicine has at first been almost invariably one of indifference to the advances of science, and often, indeed, one of hostility. If the scientific method is not of easy application it has been said to have no "clinical" value, and in a large number of instances the clinician has delegated it to some other person. In this way a large number of people have arisen to whom the clinician has delegated a considerable portion of his work. Bacteriological work, chemical work, microscopic work, is now undertaken in laboratories at the request of clinicians, and for them. In recent times the physiologist has also tended to invade the field of practical medicine, and to him is largely due the advances which have been made on the subject of blood-pressure and variations, physiological and otherwise, in the heart. Chemists and pharmacologists are hard at work in the discovery of therapeutic remedies which are ultimately exploited by the practitioner. We frequently hear the clinician assert that the work of physiologists applies to the dog, but not to man. When the physiologist shows that blood-pressure can be accurately determined and recorded in man by scientific instruments of precision, he is told that, although this may be true, his method is not "clinical." Although a sphygmomanometer is accurate, it must be subservient to the highly-trained physician's finger, because the physician cannot carry an instrument with him

and because the sphygmomanometer is not clinical. I should like to raise the question what a "clinical" instrument is. The concept of "clinical" seems to be one of inches. If an instrument cannot go easily into the waistcoat pocket it is not clinical. A "clinical" thermometer is something less than 4 in. long. If it is a foot long it may be much more accurate, but it is not clinical. A hæmocytometer is apparently not yet a clinical instrument because Zeiss has not yet chosen to make it go easily into the physician's pocket. Personally, I cannot see why this should be so. The surgeon has emancipated himself from this sartorial tyranny, and does not hesitate to take with him any instrument he requires, even in the shape of a real clinical instrument, an operating table. From a practical point of view this makes a considerable impression. What, for example, can be more impressive than the arrival on the scene of labour of a relatively small obstetrician with a very large cephalotribe? If he produced from his pocket a clinical instrument in the form of a gimlet, I imagine he would be discredited in the eyes of his patient.

Coming, in particular, to the question of infective diseases, it appears to me that physicians have been studying these for hundreds of years without making any material advance. I know no instance where the contrary can be urged. We have, on the other hand, very extensive bacteriological research leading to the elucidation of the causes of a large number of infective diseases. These causes have been isolated, their effects and life-histories have been studied, and a great deal is now known about them. Hecatombs of animals have been immolated to determine their effects, and methods of prophylaxis and cure have been worked out. On the basis of all this knowledge, which has been accumulating for thirty years, the bacteriologist has at last said, "Let us try whether these results can be applied to man." What is the answer of the clinician? "No, *you* ought not to do this: you must demonstrate anew to us the grounds for your suggestion. We shall determine for you the value and limitations of this method." The question may, however, be asked: Has the clinician the requisite knowledge and experience to adjudicate in this matter? This is very largely the problem which has led to this discussion. To many practical physicians it seems illegitimate that a bacteriologist or experimental pathologist should have an opinion on a matter concerning man, who is assumed to have an organization entirely different from the animals which the experimenter uses in the course of his work. On the other hand, it cannot be denied that observations on animals can be carried out with

much greater precision than in the case of man, who is only partially open to experimental methods.

Now the immunity problem is one which has been very carefully studied in the case of animals, and I would submit that the bacteriologist and immunizator possesses very special knowledge with reference to infective disease. In spite of this, however, he has hitherto been classed as an appendage to the physician or surgeon. That this is not merely the point of view of the bacteriologist may be seen if one refers to the opinions of certain clinicians on this question. Thus I turn to the address in medicine delivered before the British Medical Association in 1908 by Dr. Kingston Fowler,[1] a man of wide experience and known temperate judgment. He says: "The bacteriologist, in some at least of the general hospitals, is now regarded as the servant of the physician, who orders that certain investigations shall be undertaken. This is not, I submit, the position to which he is entitled. In important cases in which his advice and assistance are required he should consult on equal terms with the physician at the bedside, and the investigations to be made or the treatment to be adopted should be the joint result of their deliberations. That any person professing ignorance of clinical medicine should independently attempt to treat disease is a position so unsound that it needs but to be stated to be condemned." My own experience would lead me to regard Dr. Kingston Fowler's views on this subject as advanced. There is still much of the spirit which moved Chassaignac to exclaim, with regard to Davaine's experiments, "Laboratory results should be brought out in a circumspect, modest, and reserved manner as long as they have not been sanctioned by long clinical researches, a sanction without which there is no real practical medical science."

With regard to the methods advocated by Sir Almroth Wright, it may be said they are not clinical. They presuppose knowledge which is not revealed to the unaided retina or the organ of Corti. They are complicated and difficult methods. I think, however, that they have been embarked upon in a very light hearted manner in many instances. Writing on this subject five years ago, I suggested that before vaccine therapy was launched on the public it should be carefully worked out in hospital practice under conditions not readily obtainable in private practice, and when its value and limitations had been decided it might be applied as a practical method. This is the way we have tried to conduct this problem at the London Hospital. Cases are sent to us to treat, and

[1] *Brit. Med. Journ.*, 1908, ii, p. 253.

when our investigations are at an end, these cases are referred to the clinician for his opinion on the value of the treatment. It would appear, however, that this cautious mode of advance has not been followed everywhere, and the fact that considerable commercial possibilities were likely to accrue from the practice of vaccine therapy at once led to the appearance of a large number of persons who, in spite of a very limited knowledge of bacteriology, began to exploit the method practically. Instead of passing through the moultings necessary for the bacteriologist, they emerged as fully-fledged imagines, to sip with the physicians and surgeons from the flesh-pots. If rumour is to be credited, some of them have sipped so deeply that the physicians and surgeons have been perturbed. The contributions of these pseudo-immunizators to medical science are trifling and cannot well be criticized, as they avoid the bacteriological arenas of discussion. Their commercial aptitude would appear, however, to be in the inverse ratio to their bacteriological ability. From what they write they appear to consider that the whole problem of immunity to infective disease is solved by the injection of a vaccine which has been prepared from some morbid product. If one vaccine does not answer, another is tried, liberal payment being demanded all the time. The pseudo-immunizator extends his operations still further to prophylactic courses of inoculation against diseases which leave a trifling degree of immunity, the durability of which is evanescent. Instead of administering vaccines subcutaneously, a short boom was created by the exhibition of antigenic substances *per os*, although numerous experimental data were opposed to such a method. Indeed, the whole problem of vaccine therapy seems to have become so complicated that the value and limitations of the method are difficult to determine. In estimating the value of any therapeutic method the elimination of chance is of fundamental importance. There should be parallel series of cases, those with and those without the treatment in question; and it is only when this is done on a big scale that the true value of vaccine therapy will be known. At present we do not appear to possess accurate data of this kind, and thus we have to rely only on the personal impressions created in the minds of medical men who have witnessed the treatment of infective disease by different methods. It must, however, be clearly borne in mind that the medical man is not infallible, and he is subject to frailties common to other members of mankind.

So far as I have been able to learn, the profession can be resolved into four groups with regard to the question of the value of vaccine therapy. There is, first, the clinician, who boldly and publicly asserts that as a

result of his experience he is unable to subscribe to the view that the results of vaccine therapy are better than those obtained with other well-tried methods. This group appears to be small. There is a second and much larger group, who allege this in private, but, for various reasons, do not assert it in public. This seems to include a large number of clinicians. A third and small group condemn vaccine therapy by word of mouth, but practise it by hand. I feel sorry to refer to this group, and I only do so because I know it exists. Lastly, there is a group, in which I reckon myself, who consider that results can be achieved by vaccines which have not hitherto been obtained by other and older methods.

It must be remembered, however, that most of the cases which have come into the hands of the inoculators have been thoroughly submitted to other methods of treatment before. We may thus subdivide the cases treated into those that are chronic, severe, desperate, moribund, and dead, for in several instances the patient has died before the inoculator arrived on the scene at all. Had he inoculated, I have no doubt that the result would have been ascribed to a failure of vaccine therapy.

I believe we have hitherto seen the worst of vaccine therapy, and better results will be achieved when it is recognized that this method is not to be looked upon as a *dernier ressort*. Even in desperate cases, however, cure has been brought about in some instances. The attitude of the clinician is nevertheless remarkable. If a desperate case gets well on vaccines, it is alleged to be a question of chance; if it does not get well, the case but confirms the unfavourable opinion which the clinician expressed. Were this kind of logic applied to other methods of treatment, therapeutics would soon be discredited altogether. I have no doubt in my own mind that many cases which have been ineffectually treated over long periods by the most competent in the profession have been cured by inoculation at the end; but it seems very necessary to collect data on a much wider scale and much more accurately before the final value of the method can be determined. We need more science in our profession and fewer clinical impressions.

Sir WILLIAM B. LEISHMAN: Although I felt it an honour to have been invited by the Council of the Society to take part in this discussion, I had considerable reluctance in consenting, for the reason that my personal experience of the clinical side of vaccine therapy is so limited. From the laboratory side, however, I have a large experience of phagocytic work in connexion with the typhoid bacillus, and my remarks will be mainly in connexion with this organism.

I should like, however, in the first place, to put in a plea for a wider trial of the method of estimating the phagocytic index which I first introduced. This method I still think presents some advantages as compared with the modification of Wright and Douglas. These are (1) that the technique is simpler, takes less time, and requires no special apparatus; (2) that phagocytosis goes on in the medium of the fresh blood plasma, instead of serum, and the conditions therefore approximate more closely to what occurs *in vivo;* (3) I believe that it is better to test the power of the patient's own leucocytes instead of employing those of another individual; (4) in this method the cells are not exposed to repeated washing and centrifuging, processes which I cannot but think must damage them to some extent. At the same time I fully appreciate the value of the modification which was introduced by Sir Almroth Wright and Captain Douglas, and no one realizes more than I do the enormous importance of the work which has been founded upon it. I only suggest that the older method might be worthy of more extended trial for the reasons I have put forward.

On thinking over the work which I have done in connexion with typhoid vaccine, there are three points which I wish shortly to bring before you:—

First, in our efforts to improve typhoid vaccine—if that were possible—we have experimented with a large number of alternative vaccines, and, to judge of the effects of these vaccines, our principle has been to inoculate a group of rabbits, to pool their blood, and then, over a period of weeks or months, to institute examinations every day or two for the various protective substances. We have done that work for some years now, and have come more and more to the conclusion that the phagocytic index of these animals is the most reliable test of the value of a particular vaccine; more reliable, for instance, than that which is furnished by the bactericidal substances or agglutinins. I may add that we have to some extent confirmed this by a series of observations on cases of enteric fever and on convalescent patients who have recovered. We worked out an average curve, showing the course of the various substances, and those concerned in phagocytosis remained at a high level longer into convalescence than the others.

Secondly, in Sir Almroth Wright's eloquent introduction to this debate he alluded to a method which has been suggested many times—that is, the ingestion of vaccines as being a possible alternative to inoculation. Our typhoid work has given us some experience in this direction which may be of some interest. Sir Almroth Wright will

remember his own earlier attempts at immunization by the mouth by the swallowing of sterilized broth cultures of typhoid, and the results which he has recorded are very similar to those which we got in our experiments. The latter we conducted, however, on different lines. My colleague, Major Harrison, endeavoured to guide dead typhoid bacteria past the dangers of the stomach into the intestine, and, to do this, administered to various volunteers—our brother officers—pills containing dead typhoid bacteria suspended in a matrix consisting of fat and stearin. These were given at various intervals, and sometimes were continued for three or four weeks. Another series of experiments was done by enclosing bacteria, suspended in lard, in gelatine capsules. We analysed samples of the blood of these various volunteers, and concluded that it was certainly possible to modify the amount of protective substances in the blood by giving vaccine in this way by ingestion. But the results were extremely irregular. In one case, in a man who had been previously inoculated a year or eighteen months before, there was a remarkable initial rise in the bactericidal power, this lasted only a short time, and was followed by a fall to below the normal, which persisted until treatment was abandoned. In one or two cases there was no obvious effect at all, while in others a negative phase was apparently induced. In one case, in especial, considerable gastric disturbance was set up by the use of these capsules; and as the officer concerned was one who holds both the Victoria Cross and the Distinguished Service Order, we may believe that he held out longer than most. Generally speaking, the results of these experiments showed we had no control over this system of immunization. It might modify the substances, but we could not prophesy what would happen, and there was danger of producing bad effects in the shape of a negative phase.

The third point, which is more directly concerned with the subject of your debate, is the use of typhoid vaccine not as a prophylactic but as a therapeutic agent in enteric fever. That has been suggested often, and has been tried in various places—in Germany, America, and in India. Our own efforts in this direction arose in this way: I was very much struck by hearing Chantemesse's account of the results of his serum therapy in enteric fever, and these results he attributed to opsonification. He gave extraordinarily small doses of his serum, and claimed that the good effects were due to increased phagocytic power. If that is so, I thought we ought to do the same, or better, by the use of vaccine; and we have therefore treated some cases with our ordinary

prophylactic vaccine. So far, we have only got a comparatively small number of cases, but they are, I think, distinctly encouraging. On talking it over with Captain Smallman, who was shortly returning to India, I asked him if he would undertake this vaccine treatment, and, when he got back to Quetta, which used to be a hotbed of the disease, he carried it out in a series of cases of enteric fever. As we were working in the dark we had to go cautiously with regard to dosage, but we had found, previous to this, that inoculation of very small doses of typhoid vaccine, up to 50 millions, produced no evil effects at all. It was equally certain that there were no good effects from such small doses. On raising the dose, however, up to 300 or 400 millions, and repeating it every fourth or fifth day, better results were obtained. If the dose is given in the morning there is a reactionary rise of temperature of 1° F. or 1½° F. above what one would have expected on that evening. That is followed next day by a considerable drop in the temperature, which remains comparatively low for two or three days after the inoculation. After that it begins gradually to rise again. If at this moment the injection was repeated there was the same sequence of events, and after three or four inoculations the temperature fell to normal and usually did not rise again. There appears to have been no doubt of the great improvement in the appearance and general condition of the patients. In the ward in which these cases were treated I was told there was complete absence of the typical typhoid *facies* in the patients; they were rosier, seemed in good condition, and felt better. It is remarkable also that in several instances the men asked for the inoculations to be repeated, while, in the case of an officer, he said that in spite of the rise in temperature which followed his inoculation, he felt from that moment a definite change for the better, and never afterwards looked back. Captain Smallman treated thirty-six cases, of whom two died; but he noted an unusual freedom from relapse and complication in the remainder. The system has also been tried, but not on such a large scale, in a few other stations at home and abroad. Five places I have now information about, and the results generally are certainly encouraging. Some of the five officers who tried it started in a spirit of profound scepticism, and were converted, and are now anxious to try the method in other cases. I cannot give you information about the behaviour of the opsonic index of these cases. Captain Smallman was going to investigate that during the next enteric season at Quetta, but owing largely, I believe, to the extended use of anti-typhoid inoculation, he has not had sufficient material to work with.

My experience of vaccine therapy on the clinical side is so small as to make any general opinion of mine hardly worth listening to. Still, such as it is, I give it to you. I believe vaccine treatment to be the greatest step forward that medicine has made of recent years, and that it has immense possibilities before it. One of the most important keys to successful treatment I believe to be the question of dosage, and I do not see how we can arrive at the appropriate dosage in the absence of frequent estimations of the opsonic index.

Professor HEWLETT: I believe Dr. Bulloch is a Scotsman; I do not know whether he is a member of the Church of England. At any rate, his attitude towards vaccine therapy very much coincides with one of the sentences in the preface to the Book of Common Prayer—namely, he keeps to the *via media*, he avoids the extremes. And I think that it is very much my own attitude with regard to vaccine therapy. There is, I believe, immense value in vaccine therapy, and immense possibilities; but undoubtedly it has its limitations. I do not propose to deal with diseases treated by vaccine therapy, as this has been gone into fully by previous speakers. One has, of course, seen striking results in a number of various diseases. With regard to pulmonary tuberculosis, I must confess that I am very uncertain as to the value of vaccine therapy. I think pulmonary tuberculosis can be divided into three classes of cases. There is one class which, even if the conditions are but moderately good, tends to do well. There is a second class which, if very well looked after and well treated and put under good conditions, also tends to do well. There is a third class which, do what you will, goes downhill. And in this last class there are cases which run a very long course—ten years, perhaps twenty years, with remissions. So it seems to me extremely difficult to be sure as to whether tuberculin treatment is of service in pulmonary tuberculosis. Similarly as regards tuberculous glands. If they are of any size, it seems to me a dreary process to treat them only by vaccine therapy. To my mind the treatment to be suggested is immunization, or treatment for a period with a few injections of tuberculin, and then excision (of course I am only speaking generally), followed again by treatment with tuberculin from time to time to try to keep up immunity. As regards the mode of administration of vaccines, I was rather sorry to hear Sir William Leishman's results as to the uncertainty of oral administration, because I cannot help thinking that if oral administration could be adopted it would have many

advantages over subcutaneous inoculation. Personally, I should prefer to take a medicine rather than have it injected, and I think probably *prophylactic* vaccination would be much more extensively adopted if we could get over the inoculation difficulty. Moreover, with inoculation, accidents have sometimes happened, which make one wish that oral administration could be adopted.

Then there is the question of failure of vaccine therapy. Failures do occur, as I imagine everyone will admit. It cannot be expected that any form of treatment will cure every case, and the peculiarities of the person infected with the microbe must always influence the result. Early treatment also is always conducive to success with any form of treatment, and vaccine therapy should not be considered as a *dernier ressort*. Admitting these factors, however, we are still face to face with cases which we believe ought to do well—we may be mistaken in that, of course—and yet they do not do well. After all, I take it that vaccine therapy is a somewhat clumsy imitation of Nature's methods of curing disease. The fact that the culture is sterilized by heat, for example, must necessarily destroy properties which are possessed by the living microbe. I have suggested that probably microbial endotoxins may be more potent than sterilized cultures, and I think that their use is at least worth a trial. Again, there is Besredka's method, which, according to him, is extremely successful for *prophylactic* vaccination, at any rate; and it appears to me it is a possibility that it might be extended to vaccine therapy—namely, the treatment of microbes before injection with an immune serum—in other words, sensitizing the microbes. He claims that *prophylactic* vaccination by this method gives results which are superior to the simple injection of bacterial cultures. Again, as Dr. Hort has pointed out, in all probability the disease complex is due not only to microbial products, but also to toxic products of tissue disintegration. And it seems to me there is a very great deal in this suggestion. By vaccine therapy we can only hope to combat the one portion of the infection, the microbial one, leaving the somatic portion unaffected, as it were. Therefore I consider that auto-inoculation as a means of treatment has at least a sound theoretical basis, although I appreciate the difficulty of applying it in graduated dosage. Finally, by vaccine therapy we rely practically entirely on the responsive powers of the body; and, personally, I am still of opinion that, if we only knew how to apply it, serum therapy is the ideal method of fighting the bacterial part of the infection. And the results obtained by Flexner and others in the treatment of cerebrospinal fever with an anti-serum suggest that we

are not by any means yet at the end of our resources as regards serum therapy. And, lastly, there is the question of the opsonic index, which has not been touched on very much. The work during the last two or three years shows the extreme technical skill that is requisite for obtaining a true index. Such a technician as Sir Almroth Wright is perhaps able to obtain results which are to be relied on and which serve as a guide to treatment; but I think it is questionable whether the opsonic-index method is going to be generally applicable. It also seems to me that in a great many cases at least it may be dispensed with by carefully noting the general clinical condition, and so on. I quite appreciate that it is due to the opsonic method that the proper dosage of vaccines has been arrived at through the work of Sir Almroth Wright and his collaborators and their masterly technical skill. Having obtained those data, I think that, to a large extent at any rate, we may be able to have vaccine therapy without the opsonic control, though I admit it is desirable to have it. But if you are going to have it, it must be done by someone whose technique is beyond reproach.

Mr. K. W. Goadby: In contributing to the discussion I wish to make some remarks on the treatment of those diseases which affect the mucous membrane of the mouth and the gums. A large number of diseased conditions of the mouth have, and are, often included under the generic term pyorrhœa alveolaris, although the symptoms may be as diverse as profuse discharge of pus with destruction of the tooth sockets, and infection of the alveolar process, and loosening of the teeth, or a diffuse gingival or even periosteal inflammation associated with rarefying osteitis of the alveolar process, but unaccompanied by any discharge of pus at all. Between these two extreme variations any combination may exist.

A point already referred to by previous speakers is perhaps truer of disease of the mouth than of any other part of the body—namely, the incompleteness of our knowledge of the bacteriology, owing to the refractory nature of the organisms so far as obtaining cultivations are concerned. Some of them, such as the *Bacillus fusiformis*, described by Ellermann, and the common spirochæte of the mouth cultivated by Mühlens and Hartmann, are strict anærobes; a number of others require an environment of carbonic acid; others again are highly sensitive to the reaction of the medium in which they are growing, and anyone who has had experience of the bacteriology of the mouth knows to his cost what

a limited number of the bacteria which may be actually seen existing in oral pus can be ultimately obtained in pure cultivation. It is not surprising, then, that two main classes of oral disease caused by bacteria may be observed clinically. Firstly, there are acute infections, more often than not engrafted upon a chronic infection, in which some well-known, or even rare, pathogenic organism is the causative factor, producing general disturbances of the whole body with acute symptoms, often septicæmia, pyæmia, or toxæmia. Secondly, there are the chronic cases of long-continued and insidious mouth suppuration in which the products of bacterial activity slowly find their way into the body either through the gum margins direct or via the gastro-intestinal canal.

For some time I have had the opportunity of observing from the bacteriological standpoint, and, further, of treating by means of vaccine therapy, a considerable number of cases of mouth disease, and I can unhesitatingly say that, in my own experience, vaccine therapy has never produced any direct or indirect ill-effects upon the patient so treated, and that, at any rate coincidentally with the use of vaccine therapy, symptoms which up to the time of the use of vaccines had resisted treatment were ameliorated, and the local suppurative conditions improved rapidly and ultimately entirely disappeared. The following two cases are good examples of acute symptoms caused by disease of the mouth :—

The first, a lady aged 65, subsequent to a slight attack of appendicitis, from which she had entirely recovered, began to exhibit symptoms of chronic toxæmia, culminating in an acute alveolar abscess, for which two offending teeth were removed, but with no amelioration of the symptoms, and in fact the temperature, which before had oscillated between 99° F. and normal, rose irregularly until it had reached 102° F., varying between this and normal in the usual manner in chronic suppuration. All kinds of local treatment of every nature had been tried, but without avail, and the patient became so ill that no hopes were entertained of her recovery. At the point marked on Chart I (*see* p. 88) a vaccine prepared from the organism—a staphylococcus obtained from her own mouth—was injected. It was followed by an immediate fall of temperature within twelve hours, the temperature never again rose above 99° F., and after a further series of inoculations the patient made an uninterrupted recovery, and has now, two years later, had no return of her symptoms and is perfectly well. It will be seen on reference to the chart that the opsonic index as taken during the process of inoculation rose against the temperature, but the slight oscillations did not give any indication on the opsonic curve.

The second case was that of a lady who was taken suddenly ill with shivering and pain in her mouth. The gums in the upper and lower jaw were turgid, œdematous, and purple, there was no previous history of any disease of the mouth, the temperature rapidly ran up to 103° F., and on one occasion touched 104½° F. Careful examination of the blood and of the mouth showed the presence of staphylococci and streptococci in the mouth together with a large number of other bacteria, and the blood on the third examination showed a limited number of streptococci. These streptococci were isolated and subsequently compared with the ones obtained from the mouth, and gave the identical

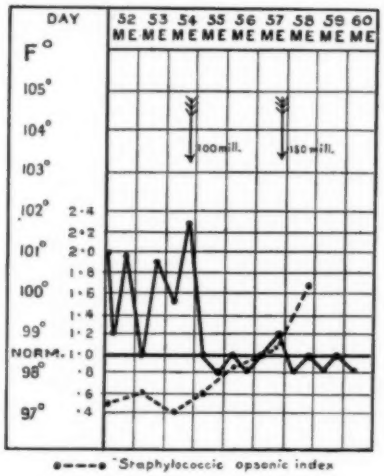


CHART I.

cultural characteristics, resembling olosely that form of organism, a streptococcus, described by Andrewes and Horder as the *Streptococcus fæcalis*. A dose of 100 million of a streptococcal vaccine, made from her own streptococcus, was injected as indicated, and was followed by a slight temporary fall in temperature (Chart II). The opsonic index was shown to be distinctly rising, and a further dose of five million streptococci was given. The index continued to rise, although upon one occasion the temperature showed another rise to 102° F.; but after this point no further rise in temperature above 99° F. was recorded, and the patient made a gradual recovery, in the course of which three abscesses

appeared in various parts of the body, the pus of which was found to contain the original streptococcus.

In both these cases, then, undoubted improvement in the patients' well-being was coincident with the use of the appropriate vaccine.

In the type of chronic mouth suppuration many symptoms in various parts of the body make their appearance, and if I were asked what was the commonest general symptom associated with chronic suppurative lesions of the mouth, I should answer with little hesitation, rheumatic, classing under such a term various forms of arthritic symptoms, often distended swellings of various joints, in addition to

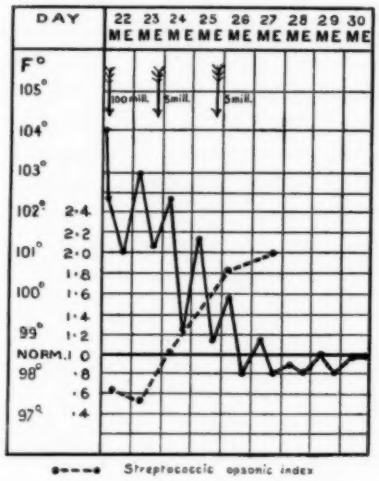


CHART II.

the so-called muscular rheumatism. The following three cases illustrate the association :—

The first, a man, aged 45, for some three or four years suffered from acute and subacute attacks of acute pain in various joints associated with swelling, and latterly becoming so pronounced as to confine him to his bed for four or five weeks at a time, and finally reducing him to the necessity of using two sticks to enable him to walk. He had unfortunately expended a considerable sum on embellishing his mouth with gold crowns and bridges, most of them affixed to teeth whose bony attachments, at any at the time I saw them, were seriously impaired

and around which considerable quantities of pus were to be found. The offending teeth and crowns were removed, and were followed by a smart exacerbation of his symptoms. Cultivations were made at the time of the removal of the teeth of the pus from the gum margins, and a streptobacillus, of which I shall have more to say at a later time, obtained. The symptoms did not entirely disappear, although they were considerably ameliorated by the surgical interference, and accordingly a vaccine was prepared from the organisms obtained from his mouth, and to which alone of the organisms present he showed a varying opsonic index. Immediately on the injection of the vaccine his condition began to improve, and he is now able to walk about without the use of his sticks, and he continues to make improvement.

The second case was that of a young woman, aged 32, who had an attack diagnosed as subacute rheumatism, followed by a very prolonged convalescence. As soon as she was able to be moved she was sent to Bath, where, after some three months' treatment, she was able to hobble about with the aid of two sticks, and she then returned to town, the symptoms showing no further signs of abatement. On examination, her mouth was found free from pus, but there was considerable thickening of the alveolar processes in the upper and lower jaw, and exploration with a sharp probe showed that the alveolus in many places was distinctly thinned, and that it was undergoing rarefying osteitis. A streptobacillus was isolated from her mouth, and, the opsonic index of her blood showing distinct variation towards this organism, a vaccine was prepared. Some exacerbation of her symptoms occurred after the first injection, but I persuaded her to continue, and I received a letter from her this morning, saying that she had discarded the use of her stick and was now able to go about her ordinary avocation with comfort.

The third case was a rheumatic type similar to the last, but in this instance the infection had not proceeded so far as to cripple the patient in walking. She had, however, swelling and tenderness of the extensor tendons on the back of the hand, attacks of pain frequently at night in her knee- and ankle-joints, from time to time swelling took place in the knee-joints. A similar bacillus to the one noted above was obtained from her mouth. In this case considerable suppuration existed. Inoculation with the vaccine was immediately followed by an improvement in the condition, no exacerbation taking place, and the patient made practically an uninterrupted recovery.

I should like to refer to two further points. Firstly, the value of he opsonic index in determining the infecting organism in any given

instance. In exploring a dark cave a source of light is of first importance, and the more brilliant the lamp, naturally the more advantageous to the explorer. But he would be a rash explorer who, because he only possessed a farthing dip and not an acetylene lamp, would prefer to blow out his much inferior light and trust to finding his way in the dark. Similarly, then, granting that the knowledge given us by the opsonic index is but meagre and by no means as adequate as we could wish, it is yet better than no guide at all. Where a number of organisms, sometimes eight or nine, are isolated, as from suppuration about the jaws, I have found that the organism most likely to be associated with the disease is the organism which shows variations in its opsonic index to the patient's serum. The organisms which may be termed the adventitious ones show very little variation, except those well within the limits of experimental error; but if either massage or other treatment be applied to the infected area in the mouth, and the opsonic index taken subsequently, the organisms which show variation in the index are invariably ones that are associated with the disease. As treatment progresses, the variation in the opsonic index towards the organism used as vaccine diminishes until the inoculation is followed ultimately by very little excursion of the index. If at this time the diseased condition has not entirely cleared up, it is not necessarily due to the fact that the vaccination by the organism in use has failed, but that other organisms are producing concurrent infection. In this my own work agrees with that cited by other speakers.

Finally, I have experimented with the effect of digestion upon a vaccine. I prepared a strong vaccine of the *Bacillus pyocyaneus*. This I divided into two portions, one of which I digested at 37° C. with pepsin and hydrochloric acid for two hours, and the other portion was untreated. At the end of the two hours the digested sample was carefully neutralized with sodium carbonate. Two sets of animals (guinea-pigs) were injected with the vaccine, two with the digested vaccine and two with the normal vaccine. Each animal received two doses at intervals of five days; the third animal was unvaccinated. At the end of a fortnight all three animals were injected with a fatal dose of a living culture of the *Bacillus pyocyaneus*. Both the vaccinated animals recovered, but the unprotected animal died in twenty-four hours. A second series of animals were treated in the same way. Both the vaccinated animals again recovered, and the control animal died.

It would seem, therefore, that two hours' digestion in the presence of pepsin does not entirely destroy the immunizing value of pyocyaneus vaccine, but it by no means follows that the absorption, via the stomach, will create immunity in a similar way to that produced by injection.

Dr. HENDERSON SMITH: Just as in the last few years there has been an attempt to introduce the practice of giving sera by the mouth, so also in vaccine therapy the oral administration of vaccines has found support. In the case of sera there is readily accessible a very large mass of evidence wholly against the efficacy of such a method, but the general use of vaccines as a curative procedure is comparatively so recent that no great body of results can be expected. There is, however, a considerable amount of evidence, the result of experiment on animals, to be found in the literature; and, as the question is one of considerable importance in practice, I propose to bring before you briefly the conclusions which that evidence forces upon us.

As you are aware, when bacteria are injected into the subcutaneous tissues, the recipient responds by the production of not one substance only but of a whole series of antibodies. These various reaction products doubtless have different values in the mechanism of protection. Some, such as the phagocytic and bactericidal antibodies, have an obvious direct influence in the scheme of defence; the importance of others, such as the agglutinins and complement-deviating bodies, may be doubtful or denied. But we are hardly yet in a position to assign to each of these substances its true relative value in the total defensive response which follows an inoculation of bacteria, or to safely reject any of the various means by which the organism acts upon its invader. Now, when killed bacteria are given by the mouth, it has been found that, as a general rule, one does not obtain the full response which is obtained after subcutaneous inoculations. There is certainly in most cases some response: the recipient does usually react to the dose of bacteria. But the reaction is nearly always imperfect. There is a difference not only in degree, but of kind. For example, after feeding with paratyphoid, or with typhoid, or with dysentery of the Shiga type, one may obtain some appearance of agglutinins in the blood, but no protection; or again, one may obtain some protection and bacteriolytic substances, but no agglutinins. There is no certainty as to the result which will be obtained in any given case. This was the experience of Sir William Leishman and his colleagues when they administered

typhoid vaccine by the mouth to men, and it is the usual experience of those who have experimented with the lower animals. The oral route is one which commonly fails to elicit from the patient the full reaction which we are entitled to look for and are endeavouring to produce.

Further, the degree of response is markedly less after oral administration. The amount of reaction, in such cases as we can estimate it by measuring the reaction products or by determining the protection conferred, is relatively quite small. This is probably to some extent a question of dosage—i.e., a question of the amount actually absorbed from the intestinal canal; not, of course, of the amount swallowed. In vaccine treatment what one is endeavouring to do is to stimulate the patient to produce his own protective substances; and one tries to do so by introducing into his system certain specific substances contained in the bacterial bodies. It is only when these substances reach his system that they are effective; and the effective dose after oral administration is merely the quantity absorbed unchanged from the intestine. In the case of bacteria, as in the case of serum, this amount absorbed bears no constant relation to the amount swallowed. The active substances are contained in the bodies of the bacteria. Before absorption can occur they must be brought into solution. (It is true some of the bacteria may be absorbed intact possibly, but the frequent absence of agglutinin formation is against the probability of this occurring regularly; and even if it did occur with fair frequency, it would simply be an indirect and unreliable method of doing what one can do directly by subcutaneous inoculation.) To effect this solution the action of the digestive ferments is required, and this very action is largely destructive of the active substances we are endeavouring to introduce. The attempt has been made to neutralize the digestive juices by administering sodium carbonate and by coating the bacteria with fat or keratin; but these measures, in so far as they are effective, would seem necessarily to delay the solution and so to retard resorption, which is in any case very slow, and must vary with the rate of peristalsis and the condition of the intestinal contents. No practical method of regulating the quantity absorbed has as yet been devised which gives fairly consistent results.

In the case of vaccines this question of dosage is all-important. Those who support oral administration have urged that it is possible to immunize animals against tubercle by oral administration of killed bacilli, and the same thing has been done with other pathogenic

organisms; but these animals were previously normal animals, where the effective dose may vary within comparatively wide limits without doing serious injury. But the case is very different if the animal is suffering already from the disease; then, as Sir Almroth Wright insisted on in his introduction to this discussion, accurate dosage is of the first importance, and this is what oral administration cannot yet give us. It is possible that repeated administration by the mouth, of which the advantages are obvious, might prove of very great service in prophylaxis, but as a therapeutic measure the evidence of experiment is against it. The efficacy of vaccines, as indeed of sera, depends so largely on factors which we cannot yet fully estimate, and their action even in the most favourable conditions is so uncertain, that it would seem premature to give up the method which admittedly allows of the greatest accuracy and control in administration.

Dr. WHITFIELD: In my opinion vaccine therapy has already been proved to be a method of great value in many cases, and I believe the number of diseases which may be successfully treated by this means will be increased as time goes on. It is, however, very important that we should review from time to time our method of procedure, and discard any part of it that proves either useless or fallacious. This brings me to the question of the value of the opsonic index. For the purpose of discussion the use of the opsonic index may be divided into two parts—namely, its value in diagnosis, and its value in regulating the frequency and dose of the inoculation.

As regards its value for diagnostic purposes, I may say at once that I am thoroughly convinced of its utility. What I may call an ultra high, an ultra low, or a variable index may be regarded as a sure sign of infection, though it may not be the prime cause of the disease. Further, if the index be taken on one or two occasions, an inoculation of the suspected organism be given, and the index be found to exhibit a sharp fall, followed by a rise, we may be very positive that we are dealing with a case of infection with the organism inoculated. This reaction I am in the habit of describing as Lawson's reaction, as I believe it was first discovered by Lawson, of Banchory, that the healthy person did not show a well-marked negative phase after inoculation. The absence of these variations in the opsonic index is not of equal value in negativing the infection. I may give three illustrative cases:—

The first was a case of obstinate whitlow which began acutely but did not clear up. At first one finger only was affected, but this spread fairly rapidly to a second, and then very rapidly to several toes. The onset as a whole was not at all like that of tuberculosis, nor was the local lesion in any way characteristic, the lesions rather resembling chronic staphylococcic infection. Dr. Emery carried out Lawson's reaction with a positive result, but the course seemed so utterly unlike that of tuberculosis that I began to have doubts as to its accuracy. I therefore gave a diagnostic injection of old tuberculin, causing a sharp constitutional reaction, and corroborating the opinion given on the result of Lawson's reaction. The second case was a child with a small chronic abscess on the cheek. Such abscesses are often staphylococcic in origin in very young children. The pus from the abscess was almost purely polynuclear under the microscope. Lawson's reaction was negative, and the index was always within normal limits according to Dr. Emery's estimation. A diagnostic injection of old tuberculin was, however, followed by a sharp febrile reaction, and a little later by healing of the abscess. This shows that the negative result with the opsonic index is unreliable. In a case of very early and doubtful lung trouble I worked out the opsonic index as follows: Before injection 1·28 and 1·25, given $\frac{1}{2000}$ mg. new tuberculin, index 0·9 and 1·32. The case was watched for three months and, as, the lung did not clear, was sent to sanatorium, where complete healing occurred in a comparatively short time. This case was seen so early that, although Dr. Beddard, who saw the case, suspected its nature, the expert at the sanatorium had to ask which lung was affected.

I now turn to the use of the index in treatment proper. I may say that, owing to the fact that the frequent estimation of the index is a laborious proceeding, the conscientious worker is apt to think that it is his duty to estimate it, and that he is neglecting his patient by not doing so. After three years' close application to the method in skin diseases, where in many cases one can see the actual changes taking place from day to day, I am in a position to make one or two statements with a considerable amount of assurance. In very severe acne the index, taken frequently, has generally proved high, though in mild cases it is often low. Again, it is a common experience that a patient who has got the better of the infection in the case of a severe boil which is healing may develop a new and virulent one at a distant part where the skin was apparently healthy and the blood supply unimpeded. I wish to lay especial stress on the fact that this is of frequent occurrence

and not at all exceptional. Such a patient has, in my experience, invariably a high index. If, then, the high index means immunity or high resistance, why this second boil? There is here no room for fantastic explanations based on the theory that the immunizing fluids cannot reach the infected site, as the skin was healthy during the time the index was high and at this time became infected. Further, it has been my experience on several occasions to give a dose in acne which raised the index satisfactorily after a comparatively slight fall, and yet was followed by severe malaise and aggravation of the eruption for several days. Here we have an opsonically correct dose which was clinically an evident overdose. This is not of great importance in acne, but in tuberculosis, where the clinical effects are not so well marked, it might be very dangerous. Yet it is especially in tubercle that the defenders of the treatment regulated by the index tell us we must always rely on this as a guide. I admit that I held this view for a considerable time myself, but this was in the earlier days of my opsonic work. Sir William Leishman has told us that in his investigations on the subject of typhoid inoculation he has found that the opsonic index is the most reliable guide as to the potency of a vaccine, but this is not the same as proving that the effect is directly an immunizing one, merely showing a lasting disturbance of the serum. I do not think that he has yet evidence proving that those vaccines which disturb the opsonic index most have conferred the greatest immunity. Lastly, I wish to deal with the statement that the doses that we use have been determined by the opsonic index. This is entirely erroneous. The first doses before any opsonic variations were noted as the result of injection were obviously not so determined, nor, as a matter of fact, were the subsequent ones.

When I began the opsonic work some years ago the usual dose of staphylococcus recommended by Sir Almroth Wright was from 1,000 to 2,500 million cocci, and these doses were used, controlled by the index, for some years. With such doses the negative phase is usually slight and transient, yet the doses now recommended are usually from 100 to 250 millions of the same organism—i.e., roughly one-tenth the amount that was formerly found correct by the opsonic index. I think therefore I am justified in saying positively that the present doses of staphylococcic vaccine have not been determined by the index, but by their clinical effects. I have also carried out an investigation of the opsonic index in a short series of cases which had been treated by the old method of giving massive doses of tuberculin R, and, although some of the indices were high and others low, none were strikingly either

high or low, and it was absolutely impossible for me to diagnose from my results which of the cases was doing well and which badly.

Finally, I should like to enter an earnest plea for accurate and patient work in the proof of the causal relationship of every organism before it is used as a vaccine. I feel quite sure that this must often entail a careful research by both a competent histologist and a skilled bacteriologist. In one disease two different cultures were used for making what purported to be the same vaccine, which I am quite certain were completely independent organisms. Theoretically nothing is easier than to separate organisms by means of plate culture and other methods, and to grow each separately on its own medium, but in practice it is often extremely difficult. Not a professional bacteriologist myself, I have had over ten years' experience in the work, and perhaps the most important fact that I have learned from my own researches is the enormous difference in dealing with infections made by the wonderful technique of the really high-class bacteriologist compared with that of the inexperienced and half-trained man, who differs only from the clinician in his lack of clinical knowledge.

Dr. JOHN FREEMAN: I wish to speak about some recent work on inoculating for whooping-cough. I shall speak of it not so much for the information I can give on the treatment of this disease as for the illustration the work affords of the difficulties we have to face in judging any method of treatment, and therefore of judging this method of vaccine therapy.

Bordet, about ten years ago, isolated a microbe from a case of whooping-cough which he considered was the cause of that disease. It is not necessary here to examine the evidence for and against his microbe, but it may be said that various people all over the world have isolated it from cases of whooping-cough, and I think the bulk of the evidence is strongly in favour of its being the cause of that disease. Bordet, having convinced himself that his microbe caused whooping-cough, made an ordinary killed vaccine from it and tried to immunize some children in Brussels against whooping-cough. He selected twelve healthy children and gave them prophylactic doses of the vaccine. Shortly afterwards chance sent a child suffering from whooping-cough into the ward where these twelve children had been placed. The result of this accident was distressing, for all twelve immediately caught the disease and had it very badly. This result was so dramatic that the mothers, who knew that

the children had been vaccinated, persistently believed that the disease had been produced by inoculating with the living virus. This result was interpreted by Bordet to mean that immunity could not be artificially produced against the microbe he had isolated. But to us here it seems to have a different meaning; we should say he had afforded the world the best possible proof that he was dealing with a microbe intimately connected with whooping-cough, and that he had produced the phenomenon known as a negative phase by giving an overdose.

The reasons for thinking this are as follows: With the vaccines that have been tested we know that different results follow from different sizes of dosage. Thus we know that a relatively small dose will produce a transient rise in immunity: a larger dose will give first a decrease in immunity, which we call the negative phase, and then an increase which we call the positive phase; and with a still larger dose the negative phase is increased at the expense of the positive phase, and may completely obscure it. If these phenomena follow from varying doses in the cases of a hundred tested microbes, we have a right to expect them with the hundred and first—i.e., Bordet's bacillus. And when we seem to have evidence that these children were more susceptible to whooping-cough after their inoculation, we naturally conclude that the dose of vaccine was excessive and had given a negative phase. If this is true, it is clearly of the highest importance to establish what size doses will be required to produce these different results.

There are two methods by which we can fix the dosage for any given microbe. First, we can test the effect of the vaccine by measuring the changes in immunity of the circulating blood. This is the method which has been employed for establishing the doses of practically all the vaccines now used in England; therefore when I started to work with Bordet's bacillus I naturally turned to this method. I found, however, that technical difficulties prevented me from testing the serum for immunity against Bordet's microbe. I could not measure the opsonic index. What Mr. Goadby has just called the light of a farthing dip was blown out, and I was left groping in the dark; that is to say, I had to apply the second method. This second method is the method of giving a dose and seeing what happens to the patient, the method of judging by the clinical result. It appeals to the medical profession, because on it has been founded nearly all the orthodox medicine of to-day. There is, however, an obvious difficulty, for we can only reach a sure conclusion by this method if we know whether a given change in the patient is the result of a previous dose or whether it is merely an accident. It has

been carefully pointed out in a previous paper that any improvement in a patient after a dose of vaccine is not necessarily the result of the dose; and, it might have been added, any grave symptoms following the dose may be in spite of, and not because of, that dose. Furthermore, if the patient gets perfectly well without any dose it is no evidence that a dose would not have hastened the recovery or made it more certain. All these accidents obscure the evidence as to the effect of the dose on the patient, and if we judge of a system of treatment case by case we are obliged to fall back on "The Clinical Instinct," that refuge of the puzzled practitioner, to decide between results and chance. As I have no great belief in my instincts, clinical or otherwise, I decided that half the cases must be set aside as controls to show what the vaccinated cases would have done without the vaccine. Therefore to every other child with whooping-cough that came to me I gave no vaccine at all, but only a little sterile salt solution under the skin, and I left the mothers in ignorance of the fact that I was in any way differentiating between the children. Lest any one should think that the control cases were unfairly treated, let me say that both vaccinated and controls got the orthodox medical treatment; that is to say, symptoms were treated and complications looked out for. After the children had been inoculated in the way I have described, I constructed statistics from the reports of the parents to see what effect, if any, had been produced by the vaccination. For this purpose the rambling remarks of the parents were reduced to one of five following simple statements: "Much better," "better," "no change," "worse," and "much worse." In doing this I quickly found I could not rely on my own impartiality. For instance, if the mother reported to the effect that the fits of coughing were more severe but not so frequent, I found that this statement tended to appear as "better" in the case of the vaccinated, and "worse" in the case of the controls! I adopted the obvious remedy of remaining in ignorance as to whether child was a treated or control case until I had got the result safely classified in my note-book. I am convinced that without these precautions the investigator will usually find merely what he expects to find.

In this way nearly 2,000 inoculations have been given, with doses varying from 2 million to 120 million bacilli. I naturally started with very small doses, as I did not wish to get any harmful result, and let me say at once that I have never had a bad effect produced by the vaccine; that is to say, I have found alarming symptoms to follow the saline injections of the controls rather more frequently than they have followed the injections of vaccine. It took about four months' work to convince me

that doses of 5 million bacilli gave better results than doses of 2 million. Since then, during two years, the dose has been slowly getting larger and larger, till during the last five months the average dose has been 80 million to 100 million. This dose has given much the best statistics so far with the comparatively few children who have had it. With the children who have come to me during the last few months, the average length of the disease has been 4·3 weeks with the vaccinated, and 7·4 weeks with the control cases—a clear gain of three weeks to the vaccinated cases. The longest vaccinated case with this higher dosage was for seven weeks under treatment before it was well; the longest unvaccinated case came to me for eleven weeks. Owing no doubt to the change of season, no new cases appeared during last month (May), and I have been in the comical situation of having to continue doses of saline to the control cases, all of whose vaccinated brothers and sisters had stopped coming because cured. I conclude, therefore, that this microbe of Bordet has some part in the causation of whooping-cough if it is not the only cause, and that 80 million to 100 million of the microbe make an efficient dose of vaccine, though possibly not the best of all doses. I understood Dr. Bulloch to say this afternoon that he advocated such statistical work as the basis of true medicine, but I doubt if it will be found convenient to employ it largely.

There is a quasi-statistical method of judging the clinical results, which consists in "giving the thing a fair trial" and recording your impression. This method has been well illustrated at these meetings. The disadvantage is that it only convinces the observer, and when he tells us of his conclusions, we are left speculating on his status in our profession and on his capacity for judgment; its advantage is that if any given man wants to know the value of any given treatment, this is the easiest way of convincing *himself*. It is the easiest way of testing vaccine therapy; first learn something about vaccines, then employ them. Most people who have done so have been convinced, I think, as to the value of this system of treatment.

It has been suggested to me lately that the curing of otherwise hopeless cases constitutes a third method of judging by results; if you cure your hopeless case, it is a score for your treatment. That sounds promising, but it is very disappointing in reality, because when you have cured your case people will not admit that it was a hopeless one. The first time that occurred to me will serve as an illustration. It was a case of pyæmia, seen in conjunction with a surgeon who thought, and thinks, that nothing but the knife will do much good; he had been

employing the knife freely on this patient, for all over the body had been cropping up abscesses which were forthwith opened. In spite of this the temperature remained up, the abscesses continued to develop, and ths patient was getting more exhausted and was clearly dying. The surgeon said the case was so hopeless that vaccines could do no harm, and the case was handed over to me. I inoculated her with a streptococcus obtained from the abscesses. She immunized rapidly, as shown by the changes in the blood, and coincidently with this change the patient got rapidly better, and in a fortnight she was out of hospital and recruiting on a farm belonging to a brother. The point of the story is that I did not receive the congratulations I expected from the surgeon; he merely told me that pyæmias sometimes get well spontaneously. So this is an excellent way of convincing one's self of the value of vaccines, but it does not necessarily convince third persons.

In conclusion, I re-state my belief that the best reason for the use of vaccines and the best method of testing them lie in immunity work and the testing of serum reactions in the laboratory.

Dr. BUTLER HARRIS: I am compelled to preface my remarks by expressing my deep sense of gratitude to Sir A. E. Wright for his opening address on Monday. His arguments and illustrations were marshalled with a dialectical skill and a spirit of toleration towards his opponents, which at once marked this debate as something more than an ordinary medical discussion.

In approaching this subject primarily from the point of view of the general practitioner, and secondarily from that of one who has devoted several years of close study to it, I am at once aware that my point of view is, perhaps, apparently a somewhat different one from that of most of you here. It most nearly approaches that of the general consultant; it differs most widely from that of the regional specialist and of the pure bacteriologist. Vaccine therapy is to me another weapon fashioned for the use of the medical profession as a whole; the interest has been to realize how far its uses are defensive and offensive, and, if these have already proved sufficient, to retain it in our therapeutic armoury. No weapon, however, is of much avail unless the wielder knows his art. I have listened to most of the important debates on this subject, and I have had many discussions with men occupied in many diverse branches in the profession, and I regret to say that adverse criticism has usually been in direct proportion to the ignorance of the critic of what I will term, shortly, clinical bacteriology.

Amongst the present limitations to the development and use of vaccine therapy, I would place a want of knowledge on the part of the clinician of methods of bacteriological investigation; and on that of the bacteriologist a lamentable need of practice in clinical matters. The fault does not lie at the door of the individual, but at the scheme of medical education in respect to applied bacteriology. It is as important nowadays for the general practitioner to be able to work out the simple pathogenic infections, which occur every day in his practice, as to be able to set a broken arm. The possession of an oil immersion is more necessary to a proper understanding of the majority of cases than an ophthalmoscope. I would have every dresser and every clerk take to the bacteriological laboratory the materials from his cases, and there learn in the outset of his career the paramount necessity of such routine examination. Every bacterial infection has its definite clinical picture, with its recognizable changes. I do not think the supplementary position of the bacteriologist to the clinician is altogether desirable, either in the interests of medicine or the scientific advancement of the individual.

I have thought this preamble necessary to my argument because of the enormous importance vaccine therapy must assume in general practice in particular. The general practitioner sees the beginnings of things. If he is able to recognize at once, clinically and bacteriologically, the species of infection he is dealing with, he is far more valuable to his patient than the man who jobs out the investigation. If vaccine therapy is to take its right place as the handmaid to both medicine and surgery, every practitioner, whether specialist or general, must, of necessity, be expert in rudimentary bacteriological technique.

I have claimed vaccine therapy as of vast importance even in everyday medicine, for the reason that the majority of cases met with are those of bacterial infections. I have, however, assumed without proof, that vaccine therapy is the particular weapon wherewith to combat bacterial infections. I do not think it is for me to traverse again the road of subtle argument which Sir A. E. Wright has mapped out to you, but I may adduce as a general argument that in every succeeding year during the last four years I have more and more employed vaccines, to the exclusion of other methods of treatment; and that conditions which formerly we regarded as beyond human intervention, now yield in ever-increasing numbers to this form of therapy. I suppose that everyone will agree with me that many modes of treatment have from time to time been in vogue, and that, with the gradually awakening

sense of failure, they have gradually dropped into disuse and have ceased to be. Vaccine therapy, however, has done much for our education in the regions of diagnosis, prognosis, and prophylaxis, apart from cure, in that we are learning the true relationship between the pathogenic parasite and its host, in order to properly apply the remedy. In other words, our practice is stepping upwards from the pseudo-scientific empiricism of drugging to a laudable imitation of nature herself.

What are the infections most commonly met with in everyday practice? In my experience they are those of the staphylococcus, the pneumococcus, the coli bacillus, and the tubercle bacillus. More rare are those of the streptococcus, the gonococcus, and the diphtheria bacillus. The catarrhal infections of the upper respiratory tract caused by the bacillus of influenza, the *Catarrhalis micrococcus*, *Paratetragonus*, and *Bacillus septus* are amongst the most frequent, and are most complicated to treat because of the combinations of these organisms.

Of the staphylococcic infections, we are agreed as to the value of vaccine therapy in boils and well-drained abscesses. I have found the *Staphylococcus albus* in severe intractable eczemata; an autogenous vaccine has proved an absolute specific in these cases. This is probably not the same organism that is associated with suppuration. With reference to the pneumococcus, time will only allow me to refer to my own results published in the *British Medical Journal*, June, 1909,[1] and the work of Parry Morgan and Willcox, of which you will hear later. I found that 20 millions to 50 millions pneumococci might be given in pneumonia without harm; that usually a drop of temperature was produced in a few hours, but that more frequently it rose again, but not to the same level, and that it was often necessary to repeat the inoculation once or twice. I am convinced that usually a distinct reaction in favour of the patient is produced and that it is wise to inoculate before the nervous mechanism of the circulatory system suffers much inhibition. The use of a pneumococcus vaccine in the sequelæ of pneumonia, and other pneumococcic infections, such as certain recurrent catarrhs, is in my experience as sure as that of staphylococcus in boils. To condemn vaccine therapy in pneumonia because it is powerless to check a virulent invasion after the whole organism has been overrun is illogical and unreasonable. In the *Medical Record*, of New York, for February of this year, Craig[2] gives the details of eight cases of pneumonia in men over

[1] *Brit. Med. Journ.*, 1909, i, p. 1530.

[2] *Med. Record*, New York, 1910, lxxvii, p. 259.

70 years of age, mostly alcoholic, treated by an autogenous pneumococcus vaccine. The inoculation was usually given on the third day; this was followed by a fall in temperature, with a tendency to rise later. A second dose was given about forty-eight hours after the first, with the result that in every case except one the disease quickly aborted. I think this is a very remarkable series of results, and if you will compare his account of the clinical history of these cases with those obtained by Parry Morgan and myself, all working independently, you will find that the effects produced by bacterial inoculation in pneumonia are practically identical. I would go so far as to assert that every case of pneumonia should be, from the outset, regarded as possibly a fatal one, and I would suggest that a stock pneumococcus vaccine, derived from one or more virulent strains, should always be employed as early as possible in a dose not exceeding 20 millions; that, in the meantime, an autogenous vaccine should be made as an additional safeguard. I think the time is not far distant when results will show that the pneumococcus vaccine is to pneumonia what the diphtheria antitoxin has proved to be to diphtheria.

Simple gonorrhœal and coli infections appear to yield very readily after a few inoculations. Coli-bacillus infections of the uterus, as well as those of the urinary tract, appear to be secondary very often to disturbances in the large intestine, which may not be so pronounced as to merit labelling as colitis. Quite recently a woman has come under my care suffering from coli infection of the bladder and urethra. Her cystitis and urethral pain disappeared after the second inoculation. A month later she miscarried at the sixth week. Coli bacilli were found in the scrapings from the uterus. There were, however, no constitutional symptoms, such as rise of temperature following the miscarriage, and she has made a perfect recovery. The manufacture of protective materials as a result of inoculations elsewhere evidently has safeguarded her from a spread of the infection.

With reference to colitis, the majority of cases which I have treated by inoculation have been passing mucus and occasionally blood. The action of the bowels is irregular, with attacks of diarrhœa and pain. There is loss of flesh and sallowness; some have had recurring attacks of acute indigestion in addition. One man recently had had an appendicostomy performed some three months previously, from which he had derived much benefit; but, realizing that he was getting back into his former condition, essayed inoculation. His opsonic index being persistently low—0·5 to 0·6—a coli vaccine was made; and now, after six

inoculations of 10 millions at weekly intervals, he has put on weight, passes no mucus or blood, and is better, he says, than he has been for years. The majority of these cases of colitis have the coli bacillus alone as the offending organism. Several of them are now well for over two years; two others, however, require occasional inoculation. The patients know when they are losing ground, and present themselves for inoculation. I venture to think that in severe cases of colitis, appendicostomy, followed by vaccine treatment, would be the most desirable course to adopt; certainly inoculation gives better results than washing out by the bowel.

Infections of the coli bacillus are constantly met with in everyday practice. In the urinary tract they appear to be not infrequently secondary to tuberculous or gonococcal infection; the results with a coli vaccine in these cases are certainly most encouraging. Where definite necrosis of kidney tissue has occurred in the tuberculous lesions, it appears necessary, unless the kidney is removed, indefinitely to continue the coli vaccine, but only at considerable intervals. It cannot be too fully recognized that certain infections cling to people for their life. We are familiar enough with the phenomena relating to the tubercle bacillus.

With reference to tubercle, my experience in inoculating in phthisis has not been encouraging. Though carried out under the most rigid conditions of opsonic regulation, I have failed to produce better results than occur with simply perfect resting conditions, such as Paterson has described. With regard to surgical tubercle, I have failed in lupus; but with non-suppurating glands and early hip disease during the past four and a half years it has not been necessary to resort to operation; and there is little doubt that the restoration of tissue in bone is very greatly accelerated by tuberculin. I find that Koch's B.E. works better than T.R., Denys's, or Beraneck's preparations.

I am inclined to think that the controversy about the opsonic index has been somewhat unnecessarily bitter. The clinicians deprecate it because it involves a good deal of labour, whilst the bacteriologists pin their faith to it because they distrust purely clinical methods. In my own work the practice has resolved itself into a compromise. I use it where it is necessary—that is, where clinical evidence is not conclusive. In tuberculosis, for instance, the diagnosis is often very uncertain, particularly in joint and bone infections; infection of the mesenteric glands in children is equally difficult to be certain about. A series of indices will usually settle the diagnosis. The first few inoculations,

both as regards amount and interval, must be regulated by a series of opsonic indices. The temperature and weight, and, where necessary, skiagrams, are sufficient guides for the future conduct of the case. The question of cure is answered by the results of another series of indices taken on the same day. In the acute infections, such as pneumonia, I have given up doing them. If inoculation is to be successful in this disease, it should be done early. It is much more important to know what the infecting organism is, and to make the vaccine. In many cases—such as staphylococcic infections of the skin—the local condition is a sufficient guide. In actual practice one finds that time and expense forbid the working of more than the actual series which I have indicated above. I admit that in the early days of bacterial inoculation it was very necessary to control each step by the opsonic index; and without a mass of carefully recorded results, the present, even imperfect, stage in the evolution of vaccine therapy could never have been reached so soon. But now those who have worked for some time at this subject, both at the bedside and in the laboratory, are able, I maintain, to recognize, from the clinical changes, the effects of their inoculations. The dosage and the intervals between the inoculations are, for the most part, now determined, so that the first barriers are swept away. I think it a thousand pities to limit the utility of so great a method of combating disease by making the process unnecessarily difficult and costly.

Royal Society of Medicine

June 8, 1910.

Sir WILLIAM S. CHURCH, K.C.B., President, in the Chair.

Discussion on Vaccine Therapy: its Treatment, Value, and Limitations.[1]

Dr. J. KINGSTON FOWLER: I did not originally intend to take part in this discussion, as my experience on the main questions is insufficient to justify the expression of an opinion; but important side issues have appeared in its course upon which I desire more particularly to speak. To the general question as to the value of vaccine therapy there can, I imagine, be only one answer. We are all agreed in recognizing the brilliant results which have followed the scientific researches of Sir Almroth Wright and his co-workers, and of the importance of the principle which he has established. I recall with pleasure that three years ago, on handing to him the Fothergillian Gold Medal of the Medical Society of London, which is given triennially for scientific work in connexion with medicine and surgery, I stated that it was fitting that a Society of which Edward Jenner was a distinguished member should be the first to publicly recognize the value of the labours of one who had made the immortal discovery of Jenner the basis of his work; that, whilst faddists and politicians have been, and still are, endeavouring to prevent the people of this country from rendering themselves immune to smallpox, vaccination had gradually acquired a wider and wider meaning, until the principle underlying its action had become the basis of the most advanced medicine of the day. My own experience of the

[1] Fourth meeting (adjourned from June 1).

use of this method has unfortunately not lain in the observation of those cases in which the value is most obvious—viz., the various staphylococcal and streptococcal infections, and in the preventive inoculation against enteric fever—and I have consequently become perhaps unduly impressed with its limitations.

Vaccines have been used for a long time in my wards at the Middlesex Hospital in all cases in which their employment appeared likely to be beneficial, including malignant endocarditis, bronchiectasis, pulmonary tuberculosis, gonococcal arthritis, some pneumococcal infections, and cases of catarrhal affections of the bronchi. Elsewhere I have observed their use in Malta fever, in streptococcal infection, in infections of the urinary tract with bacillus coli, and in other diseases; but if I were pressed to produce from my personal experience the evidence upon which my belief in their value is founded, I fear I should be unable to do so. I have had more considerable experience of the use in pulmonary tuberculosis of tuberculin, both in the manner of 1890 and in the fractional doses used in recent times, but I have not yet seen a case of that nature in which I am sure that benefit has been derived. I hope, however, that I have still a mind open to conviction, for when that is lost it is time to retire to the country and grow cabbages. On the last occasion my story of Sir Andrew Clark's illustration of his dictum with regard to age was quoted, but I think the dictum itself was omitted. "No man is old until he ceases to be able to adapt himself to his environment." That, not one's years, is the real test of age.

There are possibly some here whose professional memory does not go back to August, 1890, when Professor Koch, whose great work we all recognize and whose recent death we all deplore, announced the discovery of tuberculin. Of that event some years later I wrote as follows: "The great interest excited throughout the world by this announcement is still fresh in our memories; the lamentable lack of judgment, of self-control, and of those critical faculties which should characterize the members of a scientific profession displayed by many upon that occasion may serve as a warning for the future. The history of medicine in still more recent times, however, appears to prove that the tendency to exploit for selfish purposes each new discovery in therapeutics, and to recommend it as a panacea for almost every ailment of mankind, is ineradicable from the minds of the weaker brethren."

In view of the fact that to-day patients may be roughly divided into those who are swallowing the Bulgarian bacillus, or rather who think they are, and those who are being injected with a vaccine—a fact highly

flattering to Professor Metchnikoff and Sir Almroth Wright, but not marking a great advance in the scientific spirit of the profession—I propose to let that paragraph stand, if another edition of the work is required.

I should like to take this opportunity of airing a belief to which I have often given utterance in teaching—viz., that the difficulty in estimating the value of a new remedy is greatly exaggerated, and that when a drug has been in use for a definite purpose for several years, and opinion is still unsettled as to its value, it is usually because it has none. Please, however, do not suppose that I am suggesting that vaccine therapy has no value. I base this belief upon the observation, with which you may or may not agree, that nearly all the great advances in therapeutics which have taken place in my own experience have been accepted within a comparatively short period. I do not include the period required to bring the method or the agent to a state fit for trial, and it may take long to render perfect the technique or to determine the dosage, but that its use does produce a definite effect for good or ill is quickly acknowledged. The first of such advances that I can recall was the introduction of salicin and salicylic acid for the treatment of acute rheumatism. A very brief trial showed that the effect in giving relief to pain was most marked, and that in spite of the fact that, owing to the impurities in some of the preparations used, nearly all the patients suffered severely from vomiting. I will not trouble you with an analysis of all such advances which have since occurred, but I believe that it would certainly prove my point, My belief, long held, is that if the time should arrive, and I hope it may soon, when we are in possession of a remedy having a decided influence upon the course of pulmonary tuberculosis, its effect will be quickly discerned and will be manifest from the altered course of the fever.

The advantage of a prolonged pathological and clinical experience is that thereby a knowledge is obtained not only of the ordinary course of a disease, but also of its rarer manifestations and terminations. By such a mind no geese are likely to be mistaken for swans. When a person thus equipped is engaged upon the trial of a new remedy, the first case in which some unlooked-for event occurs attracts attention; the second serves to fix the mind upon the observation, and a third brings conviction that one is in the presence of a fact new to one's experience, and worthy of close attention. It was, for example, only necessary to see for the first time a few cases of pulmonary tuberculosis which had been treated at Nordrach to convince anyone, no matter how great his experience might have been, that he was looking upon something which he

had never seen before. It was not that tuberculosis of the lungs had undergone arrest—that is one of the commonest events in pathology—but that with arrest there should be such a complete change in the appearance and capacity of the individual. I felt that in giving my support to the movement for the establishment of sanatoria I was assuredly on firm ground, and it is some satisfaction, in reviewing again in thought the road over which one has travelled, marked as it is by milestones, each one of which recalls some cure for consumption, to find that there is only one upon which the writing is still legible, "To the Sanatorium, one mile." That sanatorium treatment is a method incapable of producing arrest in many cases is no more to the point than that vaccine therapy, like every other therapeutic measure, has its limitations.

One of the important side issues which is involved in the discussion of this method of treatment is as to the person by whom it should be undertaken. In an address delivered in 1907 I referred to this point in the following terms: "Let us now pass on to consider the physician of the future. Sir Almroth Wright is of opinion that he will be an immunizator. We shall all agree in hoping that it may be in his power to afford immunity to his patients from as many diseases as possible, and we shall also agree that as a student and subsequently he must have received a careful and prolonged laboratory training. But I submit that he must continue to be in the future as he has been in the past, above all things a man of wide clinical experience. No matter how great the advance of science may be, there will never be a royal road to medicine; it will be the common road that all must tread who aspire to treat disease, and, after the class-room has been left behind, it will lie through the wards of the hospital, the post-mortem room, and the clinical laboratory, and will always lead back to the bedside. The physician of the future will have to deal with human nature as we have had to deal with it. Times may change, ideals may alter, but water and human nature will ever remain weak. They are the only two things in this world on which it is safe to stake one's last shilling." I might have added that in the future, as in the past, all mankind will die of some disorder, and that many will need a physician; if fewer die of infective maladies more will die of other diseases.

In the future, as in the past, the first and most important thing will be the diagnosis of the patient's malady. Once that has been accurately determined, the rest is comparatively easy. Some may think that, as medicine becomes more and more firmly fixed upon a sure basis of

science, the diagnosis of disease will gradually prove an easier task. It will become more certain in competent hands, but it will never be easy, for, as science grows, it will continually place new burdens upon the physician and the practitioner. I have had many opportunities of observing that the recent increase in the number of laboratory tests available is surely leading to the disuse of the older methods of investigation, the employment, that is, of all the senses with which Nature has provided us; and it is a physiological law that atrophy follows disuse. It must have occurred to every physician to be called to a case in which the condition of the blood and of every secretion that lends itself to examination had been carefully investigated, not once only but several times, and yet the diagnosis was as far off as ever, if not further off; whereas, an intelligent use of the old-fashioned methods of inspection, palpation, percussion and auscultation showed that it was literally staring one in the face. In so saying I shall not, I trust, be thought to undervalue the very great assistance given to diagnosis and treatment by the newer methods. I am only urging that the older should not be allowed to fall into disuse.

Dr. Bulloch was severe upon the clinician, but he was, I think, perfectly fair, and I am in complete agreement with his views. The great advances of medicine in recent years have been made by the bacteriologist and not by the clinician, and I, at any rate, am anxious that he should have his full share of the credit and the reward. But nothing will, in my opinion, ever fit a man for the treatment of disease but clinical experience. No man can shut himself up in a laboratory for years and then come forth equipped for the treatment of disease.

The practice of our profession is capable of giving an insight into human nature which no other training can give, and the physician may be required at any moment to focus the experience of a life-time upon the answer to be given to that which is often the most difficult of all questions, "Is this a case of functional disorder or organic disease?" That point can rarely be decided by a purely laboratory training. There are only two possible solutions of the situation. The clinician may become more of a bacteriologist, and the bacteriologist may become more of a clinician; but if this method of treatment is to be entrusted to one person, he must possess both kinds of knowledge. The clinician must keep in continuous touch with bacteriology; the bacteriologist must keep in continuous touch with clinical medicine. It is apt to be forgotten that the profession exists for the advancement of medicine and the service of the public. The public does not exist for the profession, and

will not pay for the services of two people when one is sufficient. For the younger generation of physicians there is no excuse if they have not undergone a prolonged training in a clinical and bacteriological laboratory. When they no longer lecture on materia medica, hygiene, forensic medicine, and like subjects, for the teaching of which they may have shown no special aptitude, they will have more time to devote to what is to be their life's work. For most of those who, like myself, belong to a pre-bacteriological age it is impossible to find time to master the technique of bacteriology; we must be content to accept the assistance of those who have done so, if we wish to practise this method.

I should like to take this opportunity of repeating that, in my opinion, it will aid in the solution of the difficulty to which the advent of this method of treatment has given point, and also will prove to the advantage of the physician and the consulting physician, that their relative positions should in the future be free from all ambiguity. The physician is now supposed from the beginning of his career to be a consultant. This is now and always has been a pure fiction, and the sooner it disappears the better. He is a man who has decided to attempt to attain a certain position, and in order to succeed has undergone a very prolonged training and has limited his practice to medical cases. He may be consulted by other medical men, just as a man in general practice may be so consulted; but he is, within the limits which he himself or the holding of some diploma has marked out, free to practise as he likes. Sidney Smith was once asked by an anxious mother what profession he recommended for her son. He replied, "Madam, I should bring him up to be a bishop: I have observed that it is a very respectable calling." But the bishop of to-day was once a curate.

I have often urged, and I now do so again, that some recognized means should be provided whereby a physician may be able, if he so desires, at some period of his career, to let it be known that he has become a consulting physician. The distinguishing feature of his practice should then be that he does not undertake regular medical attendance. I refrain from suggesting the exact means by which this change of status should be brought about—it would be analogous to that which a junior at the Bar undergoes on becoming a King's Counsel—but I am not advocating a measure which I have not myself adopted for many years, or one which there is any serious difficulty in carrying into effect. The claims of humanity and friendship will, of course, always remain paramount. I believe that by the adoption of this step the profession, which, like other professions, is judged from those near the top and not

from those near the bottom, would attain a higher position in the estimation of the public, and the general practitioner and the physician would thereby obtain an assurance that by reference to a consulting physician they could not possibly be supplanted in the confidence of their patients.

Dr. ARTHUR LATHAM: This discussion, so far as it has gone, shows that in the opinion of the speakers vaccine therapy has taken a definite place in our methods both for combating and, it may be added, for preventing disease. It is a great advance that such recognition should be forthcoming. Most of us from time to time are met with what may be, without offence, described as uninformed criticism of this method. It was for this reason that this discussion was suggested. The time seemed ripe for a "stock-taking," and there can be little doubt that the discussion will strengthen the hands of those who believe in the future of vaccine therapy. It is a matter for regret that many of those who do not hesitate to cavil at the value of this method in ordinary conversation should have resisted all my blandishments and refused to take part in our debate. It may be of interest to state that the reason given is that their knowledge of the subject is so limited.

My own experience justifies me in regarding Sir Almroth Wright's work as being one of the most brilliant and useful additions to our methods of diagnosis and treatment in my time. Vaccine therapy is a great addition to our methods of treatment, but that does not mean that it will displace the whole fabric of our clinical knowledge. We are far from knowing as yet all that there is to be known of the value of vaccines in the treatment and prevention of disease, and if we are conscientious observers we must recognize that, whilst vaccine therapy often achieves brilliant and lasting results, it is not infrequently attended with either complete or partial failure.

We must all recognize the truth of the saying of Hippocrates that experience is fallacious and judgment difficult. In spite of that I would say that from my own experience I am certain vaccine therapy does good, and I am equally certain it is capable of doing a great deal of harm if it is given in an unskilful fashion. I am in entire agreement with previous speakers in believing that a knowledge of bacteriology is essential to the clinician of the present day. I am equally of the opinion that the place for the bacteriologist without clinical knowledge is in his laboratory, and that he is out of place at the bedside.

In the experience of us all staphylococcic infections yield in most cases to appropriate vaccines, but even here failure sometimes occurs, and it may be added failure is more common if we confine our methods entirely to the use of vaccines. In *Bacterium coli* infections better results have been obtained as the result of vaccine therapy in combination with the usual clinical methods than by any other method; but here again a number of cases have failed, and will probably always fail, to receive any real or permanent benefit at the hands of even the most expert bacteriologist when unaided by ordinary clinical methods. There can be no question as to the value of vaccines in chronic pneumococcic and gonococcic infections. Our experience does not justify so dogmatic an opinion on the part likely to be played in the future by vaccination in acute pneumonia. In cases in which I have formed the opinion that in all probability the patient will recover under ordinary treatment, I have seen the use of a stock pneumococcic vaccine followed by the abortion of the disease on the second and third or fourth day in a sufficient number of cases to be certain of the value of the method. In severe cases—cases in old people or in alcoholics, cases which come under observation late in the disease, or cases which are associated with much bronchitis or a marked tendency to heart failure—my experience, as might be expected, has not been so satisfactory. There are obvious reasons for this, but one reason is our want of knowledge as yet of the proper dose in a given case. It is not sufficiently recognized that pneumococcic vaccines differ considerably in their virulence, and that we cannot always compare one particular vaccine safely with another. It is possible that the usual dose of 20 to 50 millions is far too large, for there have been several instances in which the employment of such doses by bacteriologists has seemed to be not wholly free from the responsibility of causing disastrous results. Apparently excellent results follow the use of doses of 10 to 5 millions, or even less, and I would urge a trial of the smaller doses. In my experience the use of appropriate vaccines—usually *Micrococcus catarrhalis* and pneumococcus, and occasionally Friedländer or streptococcus—in cases of recurrent bronchitis does much to relieve the patient and to lessen the frequency of the attacks. Again, I have seen a case of commencing bronchiectasis, due to the impaction of a small foreign body in one of the divisions of the bronchi, and in which the patient had daily expectorated 1 oz. or more of evil-smelling pus for over a year, relieved to the extent of losing all purulent expectoration by the use of *Micrococcus catarrhalis* and pneumococcus cultivated from the expectoration, and remain well to the present time, two years after

treatment. I have seen a long-standing case of bronchiectasis, in which 8 oz. of stinking pus were expectorated daily, lose all the odour of the expectoration, which became bronchitic in character, and the patient able to resume her work and to mix with her friends. She is not cured —there is too much damage to the lung—but her life has been completely changed by the use of vaccine therapy. In this case there were seven micro-organisms in the expectoration, and the appropriate one—a streptococcus—was discovered by the use of the opsonic index taken at rest and after exertion. I shall mention later a case of infective endocarditis associated with infarctions in which a streptococcus was isolated and used, with the result that the temperature became normal.

Tuberculin is a most potent remedy. At the same time it cannot be too emphatically stated that it is capable of doing irreparable damage if it is used without adequate knowledge and guidance as to dosage and spacing of doses. It is certainly more successful, or at any rate more rapidly successful, in non-pulmonary forms of the disease than in the pulmonary forms. I have seen what appear to be permanent "cures" —that is to say, "cures" which are maintained for two years and upwards—in most desperate cases. Thus three cases of acute tuberculosis under my care have recovered, in two of which there were no localizing signs, and in one of which the patient suffered from tuberculosis of the peritoneum, hip-joint, knee-joint, shoulder-joint and pleura, with fever to 105° F. In all the diagnosis was suggested by clinical methods and confirmed by means of the opsonic index. All three patients are without any evidence of ever having suffered from tuberculosis to-day, some two years or more after treatment. A case of glandular tuberculosis of the neck with secondary staphylococcic infection, and with a history of five extensive operations previously, and upon which two surgeons refused to operate again, was apparently cured and remains well. A waiter, suffering from long-standing tuberculosis of the knee-joint of such a degree that amputation was strongly urged, was given a movable joint of the same size as the healthy one, and enabled to resume his occupation. It is said that such cases sometimes recover spontaneously. That is true, but in my experience when a desperate case responds to vaccine therapy, recovery is much more rapid than when spontaneous recovery takes place; and, moreover, it is frequently possible to demonstrate the effect of the different doses. Tuberculin appears to be a specific for tuberculous peritonitis with effusion. I have seen fourteen consecutive cases, all associated with fever, in many of which there had been previous attacks, and in many of which there

had been an operation, lose all fluid and febrile disturbance and remain well. In the dry form of tuberculous peritonitis my experience is limited and is not so satisfactory, but these cases probably require very much smaller doses. Tuberculin appears to be a specific in any form of tuberculosis where there are no localizing signs, and merely the symptoms of the disease are present, the diagnosis being backed up by a positive finding with the opsonic index. Anæmic children suffering from lassitude, slight fever, and it may be slight enlargement of the glands, respond in most instances to a surprising degree.

With regard to pulmonary tuberculosis, my experience is that we are faced with more severe limitations for vaccine therapy than in the non-pulmonary forms. In long-standing cases with fever we usually fail in the great majority of cases to effect permanent good; in some we do harm. The same is true of very acute cases. In both classes, however, we see some striking instances of improvement which should be attributed to tuberculin rather than to coincidence. I have seen, in the last eighteen months, four cases in which sanatorium treatment—in two instances for more than two years—had failed in so far as there was constantly fever at night from 99° F. to 100° F., which was increased by exertion, but in which, after a course of treatment by tuberculin and absolute rest, the fever disappeared. All these patients were able to take active exercise to the extent of eight or more miles walking after a further period of sanatorium treatment combined with the continued use of tuberculin. In early cases of pulmonary tuberculosis, and in chronic cases without much fever, my experience is that tuberculin is most valuable, and bears out the experience of other observers on the Continent and in America. The most striking statistics in this connexion are those of Ritter, of the Edmundsthal Heilstätte, Hamburg, who, it should be mentioned, was using infinitesimal doses of tuberculin many years ago. Pottenger's experience—and he, again, is a man who, in many instances, used very small doses as the result of clinical experience several years before we did in this country—coincides with the above.

With regard to the use of the word "administration" in the title of this discussion, it is disappointing to find that it has been interpreted in so narrow a sense. We had hoped to have raised a more ample discussion on the question: How are we to be guided in the determination of the size of the dose and its appropriate time of administration? Is the opsonic index rather than clinical methods to be our guide as to the dosage and spacing of doses in the use of vaccine therapy in infective disease? That seems to me to be the most important question at issue,

and I will return to it in a minute. Let me say a few words first on the subject of the administration by the mouth. Many attempts like those described by Sir W. Leishman have been made to obtain absorption of vaccines of various kinds, but chiefly tuberculin, by internal administration, which have resulted in failure. In all these cases various devices, such as the employment of capsules, coatings of fat, &c., have been used in order to insure the passage of the vaccine through the stomach. It is owing to this fact that failure has occurred. One of the chief functions of the stomach is absorption, and the method I employ is to give the vaccine suspended in a fluid isotonic with the blood on an empty and healthy stomach. To insure this it is best to give the vaccine first thing in the morning. It is doubtful whether any appreciable quantity of gastric juice is brought to bear on a vaccine if it is given in this way, and, even if it is, Mr. Goadby's experiments suggest that no appreciable alteration occurs. None of those who have criticized this method have done so as the result of experiments carried out on exactly similar lines. Some have criticized them on the basis of experiments which are not comparable, others have condemned the method on a priori grounds; and certain bacteriologists, who inveigh against the want of the open mind of the clinician, condemn the method out of hand in the best "clinical manner."

There are certain grounds for believing that part of the immunity many of us enjoy against certain infections is due to small doses of the appropriate vaccine administered in this way by a beneficent Providence. Definite proof that vaccines are absorbed when given on an empty stomach is afforded by the following facts:—

(1) In any case of infective disease we can do harm, causing a rise of temperature and an aggravation of symptoms by giving too large a dose by this method.

(2) Vaccines administered in this way have the same effect on the temperature, the opsonic index, and the clinical symptoms, and yield the same results of treatment as when they are given under the skin.

(3) If we arrive at a dose which always has a definite effect when given by the mouth in any given case, a dose of rather less than half will have precisely the same effect when given under the skin in the great majority of instances.

(4) A case of malignant endocarditis with all the clinical symptoms, including infarction on more than one occasion, is at present under my care at St. George's Hospital. A streptococcus has been isolated on eleven separate occasions from the blood. A vaccine was prepared, and a dose of

one million streptococci given by the mouth. The temperature, which had previously been irregular, being raised to 99° F. to 100° F. at night for five or more weeks, has become normal, and the general condition has markedly improved. Dr. Hunt found that the patient's blood before the administration of the vaccine did not agglutinate the streptococcus. Within twenty-eight hours of the oral administration of the vaccine marked agglutination was present. Dr. Hunt's blood did not agglutinate the streptococcus, but forty-eight hours after taking a dose of the vaccine by the mouth his blood did agglutinate the organism. The same experiment was repeated on a laboratory boy with the same result. Further, at the commencement of the treatment, the streptococcus was present in such numbers in the patient's blood that it was successfully cultivated from a drop of blood taken from the finger. It is now somewhat difficult to cultivate from large quantities of blood.

It is stated that we cannot trust oral administration because absorption must differ from time to time, and that one and the same dose in a given case may at one time be too small and at another too large. In answer to this I would state that clinical experience proves the contrary. In many cases an effective dose is found, any slight increase of which does harm. For example, a dose of $\frac{1}{30000}$ mg. of tuberculin may for weeks have a good and definite effect, but the slight increase to $\frac{1}{25000}$ mg. will do definite harm. Again, I have known the oral administration of as small a dose as $\frac{1}{200000}$ mg. T.R. followed in a few hours by a rise of temperature to 102° F. to 103° F. in three or four instances. These clinical facts have been observed by too many practitioners to be swept aside on merely a priori arguments, and my contention has been confirmed by laboratory workers such as M. Calmette, Dr. Spitta, and Dr. Inman.

I do not restrict myself to this method of administration. I often use the hypodermic method, especially when the stomach is disordered and the tongue furred. The oral method undoubtedly has great convenience on its side for the use of vaccine therapy in large institutions or in general practice. It is often of service in the case of children who fear the needle, and I know of a number of such cases in which the hypodermic method became impossible and was given up owing to the child's distress, whilst later on a cure was effected by oral administration. Finally, this method has an advantage in that it makes many cases of vaccine therapy less expensive to the patient.

Let us turn to the "limitations of vaccine therapy." Some of these may be due merely to our want of knowledge, and may disappear or be

overcome in time. Others seem to me to be inherent. In my experience vaccine therapy is much less likely to be attended with success when we are unable to control the process of auto-inoculation. This is usually due to the fact that we cannot give adequate rest to the seat, or one of the seats, of the disease. For example, vaccine therapy has hitherto failed in infective endocarditis attended with high fever. One reason for this is that one of the seats of the infection cannot be kept at rest. Similarly, tuberculin therapy is of little, if any, avail in cases of pulmonary tuberculosis which are associated with constant harassing cough or with uncontrollable vomiting, and, owing to the necessary movements of the lung, tuberculin therapy is less efficient in many of these cases than in so-called surgical tuberculosis. Another limitation is that vaccines prepared from the patient's own organisms are more effective than stock vaccines, for there are many cases of infective disease in which it is not practicable, nor even possible, to cultivate the micro-organism. It is apparently probable that we shall soon be able to cultivate the tubercle bacilli of consumptives by an easier and quicker process. If that occurs we shall be able to use more often an autogenous vaccine, and we should have better and quicker results. Again, it is a limitation of vaccine therapy that we do not know how long immunity, when acquired, lasts, and that we are in the dark as to when a further course of vaccine therapy should be given. We are often, indeed, still in the dark as to when we have established immunity. We are told that when the opsonic index remains level, and within normal limits, we may be satisfied with the immunity produced. Can we accept this statement? Certainly in my experience relapses are much less common if the administration of vaccines is continued long after immunity has apparently been established. This is true of some cases of simple staphylococcic infections, and is especially true of tuberculin therapy, which in many cases must be carried out for as long as a year after apparent recovery if we are not to have a relapse. Another limitation of vaccine therapy is the fact that a real cure is seldom effected if any considerable quantity of dead tissue is present and is not removed by clinical methods. For example, vaccines cannot produce their full effect in cases of tuberculous glands or joints in which much necrosis or the formation of pus has taken place, until an exit is made for the dead irritating matter.

The greatest limitation of all would be the fact, if true, that vaccine therapy necessitated the use of the opsonic index as a guide. In this discussion, so far, the question of vaccine therapy has been regarded

almost wholly from the point of view of those who have great facilities for laboratory work, either carried out by themselves or deputed. We seem to have forgotten the enormous number of patients for whom such facilities are, for one reason or another, an impossibility. If the opsonic index is essential as a guide, then the application of vaccines must be greatly restricted. We will all agree that vaccine therapy requires a guide, even if we differ as to whether it should be the temperature *and* clinical symptoms or the opsonic index. We will all agree that the more forms of control we can have the better, and that if the opsonic index affords reliable information, and if the temperature and clinical symptoms afford reliable information with regard to the immunizing process, we should use them all if possible. In matters of diagnosis of certain infections I often use the opsonic index, and less frequently in treatment, in an endeavour to throw light on some difficulty, taking the opsonic index under different conditions, such as at rest and after exercise, or when the temperature is at the highest point of the day, and again at the lowest. In this way it is often possible to obtain most useful information, but it is necessary to restrict such questions to the most highly skilled bacteriologists, who can be trusted to devote their personal attention to the matter, and who are willing to share the responsibility for the answer given. It is not always wise to accept the findings then, if they are clearly at variance with the clinical features of the case, any more than it is wise to place a whole-hearted reliance upon the presence or absence of a Widal's reaction in a suspected case of enteric fever. For example, I saw last year a lady in the country under the care of Dr. Rich, who for weeks had suffered from high temperature and all the clinical symptoms, chiefly abdominal, of an infective fever. We said that a bacteriologist might help us to ascertain the organism at work, and so allow us to employ vaccine therapy. A most elaborate investigation was made, and the blood tested against any possible micro-organism. The bacteriologist had an absolutely free hand, but failed to give us any assistance. In spite of this, we stuck to our diagnosis that some micro-organism was at work. A few weeks later the patient passed an ounce or two of pus by the bowel, and made a rapid convalescence. The pus was almost a pure culture of the pneumococcus, but no bacteriological evidence had previously been forthcoming as to the presence of this micro-organism, although the conditions seemed ideal for the bacteriologist.

The first question is whether the opsonic index is a reliable guide in all infective diseases. In staphylococcic infections the determination

of the opsonic index is comparatively easy, and its reliability appears to be established. The same is true, though to a less degree, of tuberculous infections. When we come to the opsonic index of the pneumococcus, gonococcus, streptococcus, and *Micrococcus catarrhalis*, we find the skilled bacteriologist much less ready to swear to the correctness of his findings. We find one set of bacteriologists full of enthusiasm for the opsonic index of all organisms. We find others, perhaps in the same laboratory, who confess themselves unable to give a reliable reading for the pneumococcus or the *Micrococcus catarrhalis* or what not. Let me read an extract with regard to the opsonic index of the pneumococcus from the paper of two workers in the bacteriological laboratory of St. Mary's Hospital[1]: "In many infections the appropriate dose of vaccine is indicated by the opsonic index, but in pneumonia certain difficulties arise. Sometimes the pneumococcus obtained was not susceptible to the action of normal serum, there being no phagocytosis under any circumstances, and then no opsonic index could be taken. In such a case one has to rely on indices obtained by using a suitable stock culture. Another difficulty is that of agglutination of the organism. This feature is seen late in the disease, especially in cases the course of which is protracted, and it causes great difficulty in the determination of reliable opsonic indices. We have found that sometimes the opsonic power of apparently healthy individuals to the pneumococcus obtained varies considerably, and thus there is difficulty in obtaining a reliable standard. This difficulty may be overcome by using for the standard a mixture of several serums from healthy individuals, or else by using in the same series of experiments serum taken always from the same individuals. In the present series of cases we have been unable to use regularly the opsonic index as our guide to dosage on account of the difficulties described. We have taken as our guide the clinical symptoms of the patient."

Again, in this discussion we have heard that Dr. Freeman, in the very able work he has done with regard to whooping-cough, was not only unable to rely on the opsonic index as a guide, but was compelled to use the purely clinical symptoms to establish the dose. The same sort of technical difficulties occur in our efforts to estimate the opsonic index of the gonococcus, *Micrococcus catarrhalis*, and other micro-organisms. In certain cases these technical difficulties can be overcome by highly skilled workers by the use of somewhat laborious methods,

[1] Dr. W. H. Willcox and Dr. W. Parry Morgan, *Brit. Med. Journ.*, 1909, ii, p. 1050.

but it is clear to any practical man that in a very large number of cases, probably an overwhelming number of cases, of infective disease, the opsonic index cannot be relied upon as a guide. This conclusion would confine the application of vaccine therapy to narrow limits if the opsonic index were the only reliable guide to the different phases of intoxication and immunization. But is it true that in any infective disease our only reliable guide to the different phases of intoxication and immunization is the opsonic index?

I have said that, in my opinion, vaccine therapy is the most momentous advance made in our therapeutics in the present generation. Criticism of the work of a great man often appears ungenerous, but I must state my opinion that Sir Almroth Wright is wrong in his contention that the opsonic index is the only real guide that we have for vaccine therapy. His contention may, indeed, retard advance in the subject. If so, it would not be the first time that an error in detail made by an inventor had retarded advance in the subject which owed its birth to him. When Sir Isaac Newton discovered the differential calculus, he, unfortunately, adopted an indifferent notation; and, as a result, advance in British mathematics was delayed for a number of years—until long after Newton died, in fact, and until the superior notation of Leibnitz was adopted in this country as well as in Germany. I think that case is analogous to the present one—that Sir Almroth Wright has adopted an inferior notation as a guide, and that, if he continues to champion it as he has in the past, he will do much to retard any further advance in vaccine therapy. Surely we cannot determine the doses of drugs or the doses of anæsthetics *in camera* by calculation for all cases of disease, and for all emergencies. In the same way, it does not seem possible that we can calculate the doses of vaccines in laboratories without regard to the clinical symptoms of the patient, or without regard to the personal equation of the individual; certainly not without grave risk of doing harm to the patient, or of bringing the whole method of vaccine therapy into disrepute. But is the opsonic index the only possible guide to the different phases of intoxication and immunization? Some years ago, when I considered this question, it seemed to me that when I dealt with a case of pneumonia, and it was found that at a certain period of the disease the temperature had fallen, the symptoms had improved, and the man was better, I was within my rights in making the deduction that a certain amount of immunity had been established. I asked myself: Why is it that the symptoms have improved? I said it was because there was a diminished amount of

intoxication. Why is it that there is diminished intoxication? I replied that there was a greater effort towards immunization. It seemed to me that as intoxication became less, the degree of immunization must be found to have become greater, because the lessening of the intoxication was due to an increase in the immunizing process. The converse appeared to me to be equally clear. In a crude way I took it that the immunizing process was comparable to an alkali, and the intoxication process to an acid. I thought that clinical observations might be compared to an indicator such as litmus paper, showing us when we were dealing with an excess of alkali or an excess of acid, or with a neutral fluid. I was told that the opsonic index was a better form of indicator—more reliable and delicate. I said to those who told me this that I thought that a clinician knew when his patient was getting well, that he knew when he was getting worse—he knew, in fact, whether the process of immunity or the process of intoxication was gaining the upper hand. But I was ready to learn and to put the matter to experiment. I asked Dr. Inman and Dr. Spitta to test it from the laboratory point of view in certain diseases, whilst I investigated it from the clinical point of view. I asked them to take the opsonic index frequently, whilst I recorded the clinical symptoms and the temperature.

When we plotted out the two sets of observations it was clear that there was a definite relation between the opsonic index and the clinical symptoms and temperature, and that both could be used as a guide to the immunizing process. We found that when the symptoms improved, the opsonic index had risen; when the symptoms were aggravated, the opsonic index had fallen; when the temperature was lowered or became flatter (even in subnormal temperatures), the opsonic index had risen; when the temperature was higher or fluctuated to a greater extent, the opsonic index had fallen. We found that, though we could not tell from clinical observation what the precise reading of the opsonic index was—whether it was 1·2 or 1·8—we could tell whether there was any marked variation in the height of the index, and whether it was rising or falling. I was gratified at this, and thought I could interpret the process of immunity by clinical observation in terms of the opsonic index. To my great disappointment, I find, in the course of this discussion, that two other skilled observers, Sir Almroth Wright and Dr. Colebrook, have since investigated the subject along the same lines, and that they state that the opsonic index has no definite relation to temperature or symptoms. One set of skilled workers says we are dealing with a black object, and the other that we are dealing with a white

object. Dr. Inman must deal with that question, but it is clear that when the opsonic index is asked to decide so comparatively simple a question, and we have two skilled workers giving us one answer, and two equally skilled giving us another, the only deduction to be drawn is that the opsonic index cannot be used practically on a large scale.

My experience convinces me that clinical observation forms a good, and in most cases an efficient, guide for vaccine therapy. A dose of vaccine which is followed within twenty-four hours by a rise of temperature or an increase in the fluctuation of the temperature, and by an aggravation of symptoms, is too large. A dose which has no effect on the temperature or the symptoms is too small. A dose which is followed by a lowering of the temperature, or a diminished fluctuation of the temperature (even in subnormal temperature), and an improvement in the symptoms, does good. When the effect of such a dose becomes less, the dose should be increased in size. These are simple rules, but they require careful observation and judgment. Occasionally one meets with a rise of temperature in the course of a specific infection which is not due to the specific organism. We are told that here the opsonic index is of great service, and it is true that it may be. It is, however, often quite clear from a careful consideration of the clinical symptoms that this rise of temperature is accidental. For example, suppose we are treating a case of joint infection and suddenly there is a rise of temperature. If at the same time the patient complains of a sore throat, it may be taken that the rise of temperature is unconnected with the joint disease. Numerous other and less crude examples will occur to any clinician. If we are in doubt, no harm will occur by waiting a day or two, as in the *majority* of cases the postponement of a dose of vaccine is a matter of no great importance. Again, we are told that it may be possible to be guided by clinical symptoms in simple infections, but we cannot do so when we are dealing with secondary or multiple infections. In most of such cases one infection is dominant, and must be dealt with before we can produce much impression on the others. There is no doubt that the treatment of a multiple infection is much more difficult than that of a simple one, but in most cases we can be guided by the clinical symptoms if we give the doses of the different vaccines on different days, and carefully consider the individual effects. Certainly the results obtained by the clinician with a working knowledge of bacteriology compare very favourably with those obtained by the bacteriologist.

Let us take another line of argument. Sir A. Wright has complained that although certain clinicians accepted the value of vaccine therapy in staphylococcic infections, they held that that was no proof of the value of vaccine therapy in other affections. He has argued, and I am not sure that his line of argument is quite sound, that if inoculations in one form of infection did good, inoculations in all other forms of infection must do good. When clinicians assert that it is possible to be guided in cases of staphylococcic infection by clinical symptoms, he agrees. He then goes on in the best "clinical manner" to say that that is no proof that we can be guided by clinical symptoms in other forms of infection. Sir A. Wright, however, holds that one case proves the whole, and, if we were prepared to accept that line of argument, the clinicians might rest satisfied with an easy victory. Let us examine the reasons why Sir A. Wright allows that in dealing with a boil on the neck we can dispense with the opsonic index. He says that in such a case we know what is going on, we can *see* when there is an aggravation of symptoms, and when there is an improvement. That is a remarkable admission, and means that when we know there is improvement or the reverse by a study of the case, we can dispense with the index. Is not it precisely the clinician's business to know whether there is improvement or the reverse, and is he not in most cases able to give a sound opinion? When we deal with pulmonary tuberculosis do not an increase in fever, increased expectoration, sudden onset of sweating, and a number of other symptoms tell us what we want to know, and cannot we gauge improvement in the same way? If we are treating a case of cystitis, have increased frequency of micturition, increase of pus, onset of hæmorrhage or increase of pain no meaning for us? The bacteriologist may be unable to make deductions from the clinical symptoms, and be restricted to the use of his eyes, but he is a poor physician or surgeon who cannot tell, by a careful consideration of his patient's case, whether a dose of vaccine has done good or harm or proved ineffective.

Vaccination by auto-inoculation is a more difficult business than vaccination by simple inoculation. We are told, and we accept it, that in the sanatorium treatment of pulmonary tuberculosis the physician in reality treats his patient by carefully graduated auto-inoculations. Here the clinician has been guided for many years entirely by the clinical symptoms. Are not the results of sanatorium treatment in early cases sufficient to show that auto-inoculation can to a large extent be controlled just as efficiently by a consideration of the clinical symptoms as by a study of the opsonic index? Does not this lead us to

the conclusion that treatment with vaccine therapy can also be controlled by similar methods?

Vaccine therapy is at present between two fires. On the one side we have the clinician, who is ignorant of the value of bacteriological methods; on the other we have the bacteriologist, who despises the clinician and undertakes the treatment of disease unequipped with clinical knowledge. The near future will show that the clinician must remain captain in matters of diagnosis, treatment, and prognosis. That does not mean that he will have to carry out himself highly technical laboratory work. It is impossible to undertake the laborious work of a clinician and at the same time to keep in constant touch with technical laboratory work. I gave up a year to bacteriological work; it was a year well spent. It does not enable me to carry out technical laboratory work now, but it does enable me, as a clinician, to keep in touch with bacteriological advances and to recognize the value of the bacteriologist.

Sir Almroth Wright used to hold that the bacteriologist would displace the clinician, whom he regarded as an effete product. Is not this view of the value of the bacteriologist due to some want of comprehension of all that clinical medicine means? Dr. Slater, in a recent address, when speaking on this subject, reminded us that a comparatively small object, held before the eye, would obscure the sun. We have been told by more than one speaker that the clinician must carry out his own bacteriology, prepare his own vaccines and the like, and not depute it to a laboratory worker. It is strange that bacteriologists who have strong views of the iniquity of clinicians in this respect should do everything they can to lead us into temptation by supplying the necessary vaccines to commercial chemists. But if we put that aside, what arguments can be brought forward to support this contention which do not apply with equal force to digitalin, hyoscine, and all the drugs of the Pharmacopœia? We recognize the value of the pharmacologist and of his work, and we depend on him for the preparation of our drugs and for information as to the dose of his preparations, which are carefully standardized. We do not, however, find that the pharmacologist seeks to magnify the importance of his work, or that he asserts that he is the only person who is competent to treat disease by drugs. The man who makes the gun is not the only man, and is seldom the best man, to fire it. It surely is a strange contention that the practical application of any science must rest in the hands of the expert in the science itself. It is, however, perfectly true, and I think realized by all the younger school of clinicians, that a working knowledge of bacteriology is a

necessity at the bedside. It is also true that in the future we shall have more exact knowledge of the part played by micro-organisms in a number of diseases which at present do not show any clear proof of being due to an infective agent. Sir Almroth Wright has modified his opinion, and now holds that the clinician of the future must also be an expert bacteriologist. Is applied bacteriology to be the only advance in clinical methods? Is there not some evidence of the value of applied physiological chemistry, and is this not likely to be of greater service in the future? Are electrical methods, including ionic medication, to receive no consideration? Is applied physiology not making great strides in clinical medicine to-day? Are the hard-fought lessons of pharmacology to be discarded?

If we accept Sir Almroth Wright's reasoning with regard to bacteriology, must we not also hold that the clinician of the future must also be an expert chemist, physiologist, electrician and pharmacologist, as well as a bacteriologist? Sir Almroth Wright is in this respect in the position of the trombone player in an orchestra who scoffs at the conductor because he is not an expert trombonist. A good conductor knows *all that is necessary* about each instrument, and a good clinician knows all that is necessary about the kindred sciences; he need not be a master of each instrument in his orchestra. The clinician may even be hampered by too intimate a knowledge of a particular science. Different qualities are required for scientific work in the laboratory from the judgment, experience, and intuition so necessary at the bedside. The learned professor of anatomy does not make the best surgeon, and similarly the bacteriologist does not make the best clinician. A general knowledge of chemistry, physiology, electricity, pharmacy, bacteriology, &c., will enable a physician to make a decision on a matter of diagnosis or treatment of much greater value than that of a scientific specialist whose main work is in the laboratory.

I am equally convinced that many of these necessary investigations cannot be carried out by the clinician without diminishing his value. The properly educated clinician is the only one who can correlate all these findings. The captain of a battle-ship must know much of navigation, engineering, electricity, gunnery, tactics, and other things. If he makes a speciality of one to the exclusion of others, his ship will often be in danger. I could give you a number of examples which have come under my observation in which the bacteriologist has failed, and sometimes disastrously, owing to his want of clinical knowledge. Let us take one. A young woman consulted a bacteriologist for a glandular

mass in the neck. The opsonic index was taken and tuberculosis diagnosed. No other examination was made. Tuberculin was given, and the opsonic readings were found to be well above the line, and the bacteriologist was satisfied. The woman, however, became steadily worse. A month or two later I examined the patient and found evidence of extensive pulmonary disease, and that the temperature was 103° F. The woman had lost her chance and she died. In febrile pulmonary tuberculosis the opsonic index is frequently always above the line. The bacteriologist, however, limited his examination to the glands, and, finding the index well above the line, misinterpreted the significance of his readings.

I have said that the clinician will remain captain at the bedside. Similarly the bacteriologist will be captain in the laboratory. The best results of vaccine therapy, or indeed of any application in clinical medicine of the allied sciences, must come from the properly co-ordinated work of the clinician and the laboratory worker. If we are to advance vaccine therapy the bacteriologist must recognize the value of the clinician, just as emphatically as the clinician who knows his business must recognize the value of the bacteriologist. We shall not advance our science if we throw stones at one another. To my mind it is obvious that we must work together, and that we must have at least a working knowledge of each other's subjects. There can be no real rivalry between us. We have each of us a part, and an important part, to play, and I trust that this discussion will do something to harmonize our respective fields of labour.

Mr. Maynard Smith: I confess I have suffered some disappointment in this discussion, chiefly from an inadequate representation of what I may call the opposition. Personally I am a strong believer in the merits of vaccine therapy, because I have been able to show, to my own satisfaction, that I have had better results in certain surgical lesions treated with the aid of vaccine therapy than I have had in similar cases treated on the previously existing lines, without the aid of vaccine therapy. That, put concisely, is my position in the matter. I had hoped I was to have heard in this discussion some of those who are counted as opponents of vaccine therapy, and then I felt I might have had something to discuss. This privilege has been denied me, and I therefore propose to confine myself to a brief account of an investigation I made into the results of treatment in a consecutive series of cases

treated in the Inoculation Department of St. Mary's Hospital. I have taken one type of case only, as better lending itself to investigation—namely, tuberculous disease of the joints. I shall tell you the results of treatment in this series. There were 72 cases written to who had been under treatment at the time these results were tabulated. Of these, 34 attended. From my experience in writing about other cases, I find about 50 per cent. of attendances is as much as one can expect in writing to hospital out-patients. Of those who attended, only three were treated by inoculation from the time they first came under the care of a doctor. All the other cases had been treated by other methods before they came to the Inoculation Department for treatment. Of these 3 cases, two were cured, with complete restoration of function. Let me say what I mean by that. I mean such restoration of function that an observer who did not know the case could not tell which had been the diseased joint. The third was cured, but there was some deformity and limitation of movement remaining. We are willing to accept the word "cured" in cases where the tuberculous disease is quiescent, but in which there may be left some limitation of movement in the joint. We have thus 31 cases left, and in 16 of them splinting, rest, and in some cases operations had been carried out during twelve months or less before they came under treatment by inoculation; that is to say, of the 31 cases, 16 had been under treatment for either twelve months or less. The results in those cases were: In 10 a cure ensued, with complete and absolute restoration of function, so that one could not tell by clinical observation which had been the joint affected; two were cured with slight limitation of movement; four were improved, but were still under treatment at the time this investigation was completed. The remaining 15 cases had undergone more than twelve months' treatment by splinting, by rest, by open air, by operation, and so forth, before they came under inoculation treatment. Of these 15 cases, two had only been under inoculation treatment for a couple of months; two cases had been under treatment for six and ten months respectively, but with no improvement. Of the other 11, six were cured, five were improved and remained under treatment. I ought here to make a point of the fact that these were out-patients; they had to visit hospital, as a rule, once in ten days. Complete rest was therefore often impossible, and, moreover, such advantages as seaside convalescent homes were not available whilst they were under treatment.

It is my belief as a surgeon that these are better results than are usually obtained in tuberculous disease of joints. I can say, certainly, that

TABLE OF CASES OF TUBERCULOUS

No.	Age and sex	Disease	Date of onset	Duration and nature of inoculations	Treatment previous to inoculation
1	16, F.	Tuberculous shoulder (confirmed by indices of 1·46 and 0·58 at intervals of four days)	June, 1906	August, 1906, to May, 1907; tuberculin	*Nil*
2	13, M.	Tuberculous knee	June, 1906	December, 1906, to November, 1907; tuberculin	*Nil*
3	21, F.	Tuberculous dactylitis and hip	1892	Has had inoculations for two months only; tuberculin	—
4	4, M.	Tuberculous disease of tarsus	Oct., 1907	October, 1908, until present time; still under treatment; tuberculin	Plaster case
5	38, F.	Tuberculous wrist (as a child, had tuberculous elbow and dactylitis)	Jan., 1907	February, 1907, to October, 1907; tuberculin	*Nil* for wrist
6	16, M.	Tuberculous hip	1898	March, 1907, to January, 1908; tuberculin	In hospital with extension; also as out-patient in splint
7	4, M.	Tuberculous disease of ankles and tarsus	Jan., 1907	July, 1907, up to the present time; still under treatment; tuberculin and staphylococcic vaccine	Splints; three operations
8	22, F.	Tuberculous knee	1901	July, 1907, up to the present time; still under treatment; tuberculin	Plaster and rest in bed; in hospital for a year
9	5, F.	Tuberculous disease of ankle and tarsus	June, 1908	November, 1908, up to present time; still under treatment; tuberculin and staphylococcic vaccine	Plaster
10	7, M.	Tuberculous disease of knee	July, 1907	February, 1909, up to present time; still under treatment; tuberculin	Extension; Thomas's knee splint
11	12, M.	Tuberculous knee	Jan., 1907	September, 1907, to January, 1909; tuberculin	Thomas's knee splint
12	7, M.	Tuberculous knee	July, 1906	October, 1906, to April, 1909	Extension; poroplastic splint
13	10, M.	Tubercle of head of tibia; afterwards tubercle of knee-joint; secondary staphylococcic injection with many abscesses and extensive bone infection	Jan., 1907	April, 1907, up to the present time; tuberculin and staphylococcic vaccine	Operation on head of tibia

ARTHRITIS TREATED BY VACCINES.

Additional treatment during inoculation	Condition at start of inoculation	Condition in July, 1909
Poroplastic shoulder cap	Pain in shoulder; limited mobility, wasting	Shoulders normal, no difference on two sides
Splint	Knee flexed, no movement possible; wasting, pain	Knees normal, no difference on two sides
—	—	No change at present
Plaster case	Ankle swollen, rigid; pain; mother describes child as "quite transparent"	No swelling; about 10 degrees of movement; general condition of child excellent
Carr's splint	Wrist swollen, painful, hot; impaired mobility	No swelling or pain; some deformity of wrist; movement through 20 degrees; uses wrist and hand for light work
Splint	Great pain, wasting, ¾ in. shortening; trochanter above Nélaton's line; was getting worse	Walks fairly, occasional pain; head of bone deformed and dislocated; fair movement, very little wasting
Splints	Both ankles swollen, rigid, extended; two sinuses	Slight puffiness inner side of right ankle, otherwise normal; walks well; sinuses healed (healing was complete in July, 1908)
Splint	Knee swollen, painful, fixed, straight	Knee fixed in straight position; much less swelling and pain; walks, but pain and œdema of leg after much exercise
Plaster	Ankle and tarsus swollen, painful, inverted, rigid; sinus	Much improvement; sinus healed, some swelling over tarsus, still inversion and limitation of movement
Thomas's knee splint	Swelling, pain, flexion, limitation of mobility; not improving	No improvement up to present
Thomas's knee splint	Swelling, pain, limitation of movement, no flexion	Knees normal, no difference on two sides
Splint	Swelling, pain; almost fixed in straight position	Flexion possible to 60 degrees; no pain or swelling; still wasting of limb; walks
Fourteen operations, chiefly opening abscesses from secondary infection; splint	Multiple sinuses and abscesses around knee and tibia; great swelling of knee and rigidity; hectic fever, general wasting; amputation considered	All healed; no pain, general condition good, movement free, and walks without pain

TABLE OF CASES OF TUBERCULOUS

No.	Age and sex	Disease	Date of onset	Duration and nature of inoculations	Treatment previous to inoculation
14	21, M.	Tuberculous hip	1904	March, 1907, up to present time; tuberculin	Thomas's splint for three years
15	6, M.	Tuberculous ankle and cervical glands	March, 1906	August, 1906, to February, 1907; tuberculin	Plaster; operation
16	43, M.	Tuberculous knee	March, 1905	March, 1909, up to present time; still under treatment; tuberculin	Splint; operation
17	25, M.	Tuberculous knee (as a child, spinal caries)	1893	June, 1906, up to the present time; tuberculin	Splint
18	30, F.	Tuberculous knee	Aug., 1908	December, 1908, up to present time; tuberculin	Bier's bandage
19	18, M.	Tuberculous disease of head of tibia and knee-joint	Dec., 1905	March, 1906, to July, 1909; tuberculin and staphylococcic vaccine	Operation in February, 1906, head of tibia scraped out; splint
20	16, F.	Tuberculous knee	Dec., 1905	September, 1906, to January, 1907; tuberculin	Thomas's knee splint
21	22, F.	Tuberculous knee	1904	June, 1906, to September, 1907; tuberculin	Splint
22	51, F.	Tuberculous hip	Childhood; symptoms recurred, May, 1906	July, 1906, to July, 1907, and May, 1909, up to present time; still under treatment; tuberculin	Several operations; bone removed (as a child)
23	7, F.	Tuberculous knee, ankle, and hand (dactylitis)	Oct., 1907	February, 1908, to May, 1909; tuberculin	Operations on hand and ankle (erasions of joint and scraping)

[1] Since the above note was made there has been a slight recrudescence of disease, the

ARTHRITIS TREATED BY VACCINES *(continued)*.

Additional treatment during inoculation	Condition at start of inoculation	Condition in July, 1909
Thomas's splint	Pain in hip and knee	Can find no evidence of active disease; movements somewhat limited, still complains of pain
Plaster	Two sinuses; ankle swollen, rigid; "cries night and day"	Sinuses healed; runs and walks normally, no pain, "never complains" (condition in February, 1907)[1]
Splint; Bier's bandage	Very swollen, painful; starting pains at night; rigid	Patient says it is "a treat"; some thickening of synovial membrane, movement through 10 degrees, no pain
Splint; aspiration of pus, July, 1908; abscess in thigh opened and scraped, September, 1908	Knee swollen, painful, doughy, rigid; could not walk	No pain, movement through 5 degrees; thickening of synovial membrane has entirely disappeared; can walk without pain
Plaster	"Chronic synovitis of left knee"; much swollen; fainted with pain at night; only slight flexion possible	Knee appears normal to examination, except for some thickening each side of ligamentum papillæ; no fluid; movement free through 60 degrees; still complains of very great pain
Second scraping; splint	Sinus from head of tibia; joint swollen, limitation of movement	Sinus healed, knee movement normal; whilst inoculation was being carried out extensive ulceration of leg developed, which became gradually worse; these ulcers had the appearance of those due to syphilis, and the history of the case was suggestive of congenital syphilis; Wassermann reaction positive
Thomas's knee splint	Right knee much swollen, limitation of mobility, pain on movement	Knee normal, no difference on two sides
Splint and Scott's dressing	Knee much swollen, limitation of mobility, wasting of thigh, flexion, not much pain	Knee normal, no difference on two sides
Splint	Two sinuses; hip rigid and flexed	Sinuses closed and pain gone by July, 1907; then sinuses opened again, May, 1909, with swelling and pain; inoculation recommenced; rapid improvement
Splint	Swelling of synovial membrane, limitation of movement, wasting, no pain	All foci appear cured; movements good

ankle becoming puffy and painful; this is rapidly subsiding with rest and tuberculin.

TABLE OF CASES OF TUBERCULOUS

No.	Age and sex	Disease	Date of onset	Duration and nature of inoculations	Treatment previous to inoculation
24	$2\frac{10}{12}$, M.	Tuberculous ankle	Jan., 1908 (?)	February, 1908, to April, 1909; tuberculin, staphylococcic and streptococcic vaccines	Operation, February, 1908 (erasion and scraping of bone)
25	19, F.	Tuberculous elbow	March, 1904	March, 1906, to June, 1906 (then ceased to attend); tuberculin	Splint for one year
26	5, F.	Tuberculous hip	Dec., 1906	May, 1908, to February, 1909 (then ceased to attend); tuberculin	Two years in Thomas's splint; convalescent homes; during this time improved
27	18, M.	Tuberculous hip	July, 1906	October, 1906, to June, 1907; tuberculin	Rest; Thomas's splint; crutches
28	$4\frac{3}{4}$, M.	Tuberculous knee	Jan., 1906	April, 1907, up to present time; auto-inoculation test now negative, and treatment will be stopped; tuberculin	Thomas's knee splint; rest
29	40, F.	Tuberculous disease of both wrists and both ankles	1902, left wrist; 1905, other joints	May, 1906, to June, 1909; tuberculin	Operation, 1902, left wrist (erasion of wrist-joint); splints
30	$3\frac{1}{4}$, F.	Tuberculous disease of left knee (confirmed by auto-inoculation experiment)	Aug., 1907	September, 1907, to January, 1908; tuberculin	Splint (knee became worse)
31	58, M.	"Compound palmar ganglion" and tuberculous disease of carpus	Nov., 1906	July, 1908, to January, 1909 (then ceased to attend); tuberculin	Splint; operation, scraping out palmar synovial sac (melon-seed bodies); disease of carpal bones
32	18, F.	Tuberculous disease of wrist (confirmed by index of 0·64)	Sept., 1906	November, 1906, to March, 1907; tuberculin	Splint for six weeks; no better
33	12, F.	Tuberculous hip	1902, wore Thomas's splint for one year, then ran about normally; 1905, symptoms recommenced; Thomas's splint again	March, 1906, to July, 1907; tuberculin	Thomas's splint; said to have been getting worse all the time
34	5, F.	Tuberculous right knee	May, 1905	August, 1905, to June, 1908; tuberculin	Splint; said to have been getting worse all the time

ARTHRITIS TREATED BY VACCINES *(continued)*.

Additional treatment during inoculation	Condition at start of inoculation	Condition in July, 1909.
Plaster	Three sinuses; ankle swollen, painful, stiff	Normal ankle, sound scars; walks with slight limp
Splint	Elbow swollen, painful, rigid	When inoculation discontinued, no change; arm now fixed at right angle, no pain
Thomas's splint	Hip rigid and flexed; no pain except on attempted movement	Improvement continued in same way as before inoculation until abscess developed in February, 1909; patient then admitted to hospital and operated on; since then has not been inoculated; now discharging sinus
Splint and crutches	Pain, could not sleep; hip rigid, limb wasted	Hip normal
Plaster	Swollen, painful, limitation of mobility	Walks well, no pain, flexion possible through 30 degrees
Splints for wrist, bandages for ankles; did not lie up	Ankles swollen and painful; could not walk; wrists swollen and painful, fingers stiff and useless	Walks well, but left ankle weak; slight thickening about right wrist, uses hand and writes easily; no pain
Splint	Knee swollen, limb wasted, limitation of mobility	Knee normal, no difference on two sides
Splint; Bier's bandage	Sinus; wrist stiff, fingers stiff, great pain	Wrist moves well, no pain; sinus; slight limitation of movement of fingers
Splint	Wrist swollen, almost complete rigidity, movements of fingers almost lost	Wrist normal, all movements of wrist and fingers good; general health greatly improved
Splint until January, 1907; then started walking	Pain, rigidity, wasting, progressive shortening (noted as 2 in.)	Shortening 1½ in., rigid; walks fairly well
Leather splint	Knee swollen; wasting, pain, movements of knee good	Knee normal, no difference on two sides; general health improved

they are better than I have myself been able to obtain in the treatment of tuberculous disease of joints by other methods without the aid of vaccine therapy. I do not deny that there may be some surgeons who will be able to prove to you that their own results by the usual methods —splinting, &c.—have been better than those I have detailed to you. An especially expensive form of splint, a special form of operation, a particular brand of iodoform—any of these may have given better results than those I have described. But it is my belief that the cases I have quoted to you, and of which details can be found,[1] show better results than do cases treated solely by the previously accepted surgical methods.

I see that the discussion is, amongst other things, on the limitations of vaccine therapy. That there are limitations no one will, I think, deny. I will speak for a moment of the surgical limitations. Vaccine therapy cannot give rest. Rest is as essential as it ever was in the treatment of tuberculous joints. Failing rest, the patient will be getting inoculations from his own joint, and inoculations given by the vaccine therapist are likely, therefore, to be interfered with, and their effect ill-regulated. Further than that, one may increase the opsonic power of a patient's serum with vaccine, but one cannot make that vaccine penetrate to the middle of a tuberculous abscess. I know tuberculous abscesses may sometimes clear up under vaccine therapy—they sometimes clear up when not treated at all—but, generally speaking, when an abscess is present the surgeon is necessary. The formation of masses of inert material I regard as being a cause of one of the limitations of vaccine therapy, and so masses of carious bone in tuberculous disease are a cause of limitation of vaccine therapy. In other words, precisely those cases which it is difficult for the surgeon to get well, such as discharging sinuses leading down to bare bone, are the cases in which vaccine therapy finds the most difficulty. It is exactly as one would expect—the vaccine therapist is not starting a new means of cure, but only accentuating what is Nature's own cure—one would therefore expect him to meet with the difficulties that the surgeon does. Without discussing the question of the methods of administration, I would take exception to Dr. Latham's experience, that inoculation has had to be discontinued on account of the fear of the needle puncture. With the needles I have seen used there has not been that necessity, and I cannot call to mind that I have seen children cry to the extent necessary to cause even a fond mother to take them away from the vaccine therapist.

[1] *Brit. Med. Journ.*, 1909, ii, p. 1047 (*vide* Tables herewith).

In conclusion, I would say once more that it is a matter of regret to me that I have not heard surgeons speak here whose ideas have been based on their observation of cases which have shown that vaccine therapy is of no service. But so far, Achilles and his friends have remained in their tents, so I have only to tell you what has been my experience of the results of dealing with tuberculous joints by means of vaccine therapy.

Dr. ALEXANDER FLEMING: Most of the speakers, up to the present, have treated vaccine therapy in a general way, and have not confined their remarks to any one particular disease or organism. I wish, however, to refer only to one disease—namely, acne vulgaris. I do this because it is an exceedingly common disease, to which I have paid a good deal of attention, and although often ignored and untreated, yet causes very great worry to many individuals. It is a disease, also, where the lesions are very superficial, and one can readily see the effect of treatment without having recourse to any other method of examination than simple observation. It is interesting, also, because of certain diverse views which have been held as to the bacteriology of the condition, and in this respect it instances very well one of the limitations mentioned by Sir Almroth Wright—namely, that of "deficient bacteriological knowledge of the infection." For some years after vaccines came into more or less general use acne was treated with vaccines of staphylococcus with a fair measure of success. In most of the cases the administration of staphylococcus vaccine diminished the severity of the pustulation or removed it entirely. There were other cases, however, which showed absolutely no improvement with this vaccine, even when it had been administered for a very long time, while a third class of case improved for a time and then relapsed. When we came to inquire more closely into the bacteriology of acne the reason for this seemed fairly clear. The basis of the acne pustule is a comedo which has for many years been associated with a bacillus which has been called the *Bacillus acnes*. This bacillus is very common on the skin of all seborrhœic individuals, and can be seen in myriads if a film of a comedo be made. While authorities are more or less agreed as to the microorganism of the comedo, there is some difference of opinion as to the causation of the pustule. The large numbers of staphylococci which are present in the pus from some lesions, together with the results of vaccine treatment with staphylococcus vaccine, make it quite clear that

in many of these cases there is a staphylococcic infection, but in some cases the condition is very different. Here staphylococcus vaccine has almost no effect on the pustulation, and on examination of the pus one finds time after time only acne bacillus present in the films, and on cultivation a pure culture of the acne bacillus is obtained repeatedly. Only last Friday I planted out the whole contents of six pustules from one patient on six agar tubes (on which staphylococcus, if present, would flourish). These tubes were incubated until this morning (five days), when, on examination, five were sterile and the sixth showed two colonies of staphylococci. On a previous occasion two pustules had been planted out on a medium suitable for the growth of the acne bacillus. Of these one gave a pure culture of the acne bacillus and the other showed one colony of staphylococcus amongst hundreds of acne bacillus colonies. This seems to me clear proof that the acne bacillus is responsible for the suppuration in some of the cases. Further proof is obtained by the results of administration of vaccines of the acne bacillus, and I propose to illustrate the effect of treatment by reference to one or two cases.

The first case treated with a vaccine of the acne bacillus was a woman who had had very bad pustular acne on the face and shoulders for many years. She had been having staphylococcic inoculations at intervals of ten days for about a year. The condition had improved at first, but for some months had been practically stationary. She then had the same stock staphylococcus vaccine, combined with 20 million acne bacillus vaccine. This was followed by the appearance of fresh pustules. Ten days after she had another inoculation, this time of only 10 million acne bacillus, and she had regularly inoculations of the same dose of staphylococcus that she had previously been having, with 5 to 10 million acne bacilli. In three months almost all trace of acne had disappeared.

Another case was that of a man who suffered from pustular acne for many years. He had been treated off and on for two years with staphylococcus vaccine without any change in his condition, and examination of the pustules showed staphylococcus to be present in only very few, while the acne bacillus was repeatedly isolated in pure culture. In about six weeks under treatment with a vaccine of his own acne bacillus the condition had practically cleared up.

For the most part we have used stock vaccines of staphylococcus and acne bacillus in the routine treatment of our patients, but some cases will be found who do not improve much until a special vaccine of their own acne bacillus is administered. This is illustrated by the following

case: A girl, aged about 26, had had very bad pustular acne on the face for ten years. Her face was covered with large, indolent pustules. For three months she was treated with mixed stock vaccines of staphylococcus and acne bacillus with hardly any improvement. Then for the stock acne bacillus vaccine there was substituted a vaccine made from her own acne bacillus, and in four months the pustulation had almost entirely disappeared from the face.

Dr. Whitfield, at the last meeting of the Society, criticized some of the stock vaccines of the acne bacillus which have been used, on the ground that they were made of bacilli isolated aerobically, whereas he stated that the acne bacillus was an obligatory anaerobe. That the acne bacillus prefers anaerobic conditions is perfectly true, but I think Dr. Whitfield's criticism must break down in view of the fact that he has recognized as a true acne bacillus a culture isolated aerobically and grown through many generations in the presence of air for at least a year.

Dr. T. J. HORDER: If I consider the three points governing this discussion in the order of the increasing difficulty they present to me, I shall speak first of the *value* of vaccine therapy, then of the *administration* of this principle of treatment, and lastly of its *limitations*.

(1) Not that a consideration of even the first of these points is at all simple. The therapeutic argument is always difficult, indeed it is the most difficult in medicine. The difficulty is due almost entirely to the absence of proper controls in the particular experimental treatment that is being undertaken. How can this difficulty be got over in the present instance? In the individual practice of any one of us, however large, probably not at all. In hospital practice it should be possible, provided those in charge of the patients are willing to co-operate. One physician or one surgeon might well treat groups of patients on the principle of vaccine therapy, whilst another treated similar groups by other methods. I fear it is almost too much to propose not only that individual members of a hospital staff should co-operate in this way, but that some arrangement might be made by which all the patients in one hospital, suffering from a disease for which vaccine therapy makes claims, should be allowed to act as controls for all the patients suffering from the same disease in another hospital. And yet until something of this sort is attempted it would seem that the value of vaccine therapy can never be put upon a sound scientific basis. Until now little or no effort has been

made to apply the statistical method to vaccine therapy. The prophylactic use of typhoid vaccine is the only example of the use of vaccines in which scientific proof of their efficacy exists. In this failure to advance the question we are now discussing the clinician is doubtless much to blame. Some smaller matters might be investigated on the lines of comparative analysis: the duration of stay in hospital of patients suffering from such a disease as, say, gonorrhœal rheumatism, when treated by vaccine, might be contrasted with the duration of those not so treated. But, of course, to make conclusions of any value along this line of investigation, the number of cases dealt with must be considerable.

Failing the application of scientific methods in this question to human diseases, one wonders not a little that the chief advocates of the principle of vaccine therapy, if they believe it to be a very great asset in medicine—a belief to which I myself fully subscribe—have never undertaken a series of animal experiments which might prove their contention. This should not be very difficult, and the problem of controls would find easy solution. Not only would it be possible to compare the recovery or otherwise of a series of animals suffering from a local or from a general infection, treated by appropriate vaccines, with a control series similarly infected but receiving no assistance from vaccines; it would also be possible to test that vexed question, the value of the opsonic index as a guide to the choice of dosage. One series of animals might be treated according to the opsonic hypothesis, and another series might be treated without this guide. Two years ago, in conjunction with Dr. Andrewes, I carried out a series of experiments on rabbits with a view to ascertaining the prophylactic value of vaccines in pyogenic infections, and also to determine what was the effective range of dosage for such preventive purposes. But I do not know of any similar experiments on the lines of curative vaccine therapy.

Scientific proof as yet lacking, we are thrown back upon impressions, and these are very largely impressions based upon individual cases. The great triumphs of vaccine therapy are so often seen in atypical cases of disease, where comparisons are impossible. None of us at present, it seems to me, can do more than record his own impressions; to recite long lists of detailed cases really avails little or nothing. I suppose few practitioners exist in England to-day who have not seen many tediously chronic cases of infection, and not a few desperately acute ones, commence a state of speedy recovery after the use of vaccines (At least, I had so supposed, before hearing, in the course of this

discussion, that much doubt still exists on this matter.) My own experience includes so many such cases that my impression of the great value of vaccine therapy is a very deep one—so much so that I should consider I was denying patients suffering from certain infective conditions one of their best hopes of recovery if I did not employ this principle of treatment.

(2) I had thought the question of the *administration* of vaccines would raise such matters as preparation of the vaccine, the choice and time of dose, the route by which the vaccine should be given, and other important points. In this discussion, however, administration seems to have signified matters of medical politics, and to have raised the question of certain difficulties that exist in getting the principles of vaccine therapy properly tested. A note of discord has been sounded by the bacteriologist, who complains that the clinician has not responded with alacrity to the invitation to try the new treatment, that he has put obstacles in the way of the treatment, and that he denies its value. The bacteriologist has piped and the clinician has not danced. I have not myself found the practitioner tardy in availing himself of the benefits of vaccine therapy in the efforts made at curing his patients. But if this be the experience of certain bacteriologists, are there not some reasons for the fact that the clinician has not everywhere come quickly into line? The original claims of the bacteriologist in this matter were too extravagant; they almost attained to the level of an obsession. Here was a panacea at last. No need now for fresh air in the treatment of tuberculosis: London slums would serve, provided tuberculin and the opsonic index were available. The surgeon's art would shortly be defunct. Even cancer promised to yield presently to the "immunizator's" power. But to-day the phthisis sanatoria are full, and some of the inmates are sent by "immunizators" themselves. And I see no sign of diminished activity amongst surgeons. The pseudo-bacteriologist (as Dr. Bulloch aptly calls him) arose, too often bearing the hall-mark of correct training, with his glib and parrot-like repetition of all the master's phrases. This imitation was pardonable; but he spoke of "streps." and "staphs." and "gonos.," and of "coli infections," as though, holding such a sway over life and death, there was now no time for ordinary language, nor need of any grammar; knowing the innermost secrets of disease whilst as yet his contemporaries were perusing a temperature chart or undertaking a blood-count. Here was a spectacle little likely to impress the clinician. I have often been amazed at the tolerance and patience with which experienced practitioners still meet this young man in

consultation, or submit their material to him for expert examination and advice.

Then, again, consider this new departure of substituting the name of an infecting agent for a disease. Can this be an advance? asks the clinician. "What is the matter with Mrs. X.?" "Oh, *Bacillus coli;* she wants a course of vaccine." I was recently asked my advice as to the employment of a very expensive course of inoculation for a febrile patient, and the only answer I could get to my inquiries as to the nature of the disease was "*Bacillus coli.*" Even the seat of infection was not known, and no proper clinical examination had been carried out. The cultivation of a specimen of urine and an opsonic index had covered the whole field of diagnosis, and the treatment would doubtless have been embarked upon with confidence but for the fact that abdominal palpation easily discovered a large retroperitoneal sarcoma. The bacteriologist speaks too often only in terms of the infecting agent, and hence his calculations often miss fire. The clinician knows that the disease is greater than the infecting agent, for it is the interaction between the infecting agent and the tissues.

This brings me to the change of front that has occurred in connexion with the opsonic index. Five years ago, and even later, it was taught that opsonic-index estimations were essential to vaccine treatment; indeed, without opsonic-index estimations it was held that he who used vaccine therapy acted dangerously, almost criminally. But to-day what do we find? The facts of vaccine therapy have outgrown the hypothesis of the opsonic index—a big argument in favour of the efficiency of vaccines. Many who originally held that the opsonic index must control the treatment, themselves employ vaccines in large numbers of cases, successfully, with no such mechanical control at all. I think they explain this by saying that the opsonic index has taught them what doses to give in certain cases. The clinician considers himself justified in rejecting this explanation. The cases are never sufficiently alike. If the index was necessary once, it is necessary now. Moreover, experience has led the opsonist to change his doses; yet his curves change not. Wholesale chemists are now supplied by bacteriologists with vaccines which are sold to the profession with an advertisement stating that the use of the remedies is quite safe, and that good results may be obtained without opsonic controls. What is the clinician to make of this? The bacteriologist would seem to have solved that old problem of our boyhood—how to have our cake and eat it too! Altogether, I do not think it is surprising if the clinician is tardy in

handing over his cases of infective diseases to the bacteriologist for treatment.

To the further question, "Who should undertake the treatment of these cases—clinician or bacteriologist?"—surely there is but one answer. However sound a bacteriological training the young clinician has had (and this training must be regarded as an essential part of his education), this does but enable him to appreciate early the cases suitable for vaccine treatment and to collect his materials skilfully. If the clinician cannot collect his materials skilfully the sooner he learns to do so the better. But he needs the help of the expert bacteriologist to proceed past this point and carry out successful treatment. It is only now and again that it is justifiable to impose both clinician and bacteriologist directly upon the patient. Be this so or not, I think (seeing that fleshpots have been mentioned) the bacteriologist deserves, and should receive, his half of the credit, and of whatever else there may be to divide. But if the bacteriologist asks for more than this, I think his claims are unreasonable. When he appeals to the layman in the guise of physician, and describes himself as such in public directories, I regard him as much more dangerous than the clinician who affects a studied interest in the problems of bacteriology in their relation to the treatment of his patients. The sight of a physician unversed in bacteriological methods, faced with a case of obscure fever without physical signs, is no more pathetic than that of a bacteriologist emerging from his laboratory, his clinical medicine hopelessly rusty, attempting to struggle with the same case. The prophecy has gone forth: The physician of the future will be an immunizator. I think that prophecy likely to be fulfilled, but I would fain add: Let not the immunizator cease to be a physician.

(3) On the question of *limitations* I have little to say. Previous speakers have, in this discussion, seemed to deal with somewhat artificial matters rather than with the natural boundaries to the field of useful action of vaccines. Personally, I have found few or no *artificial* limitations imposed either by clinician, bacteriologist, or patient. I regard the doctrine of the opsonic index as the great, almost the sole, artificial limitation to the use of vaccine therapy. I stated this opinion now nearly four years ago, and am more convinced than ever that this doctrine is the greatest incubus to our advance in knowledge of vaccine therapy. Once the bacteriologist opens his windows and realizes that the condition of the patient, and not the state of the opsonic index, is the real test of treatment, we shall see much greater triumphs for this principle of therapeutics than we do to-day. As regards *natural*

limitations, I do not think these can be defined until the principle of vaccine therapy has had a much longer trial than is the case at present. Of some *difficulties* which I had hoped would be discussed, and which have raised questions in my own mind bearing on the natural limitations of the method, I will merely mention three or four.

(i) *The lack of some standard of virulence of the vaccine* is one difficulty: I find it a very great one. Particularly is it a great difficulty in dealing with such micro-organisms as the gonococcus, the pneumococcus, and Pfeiffer's bacillus. The variation in virulence seen in different strains of these and other micro-organisms renders tables of appropriate doses of vaccines prepared from them quite useless in practice.

(ii) *The ideal to be aimed at in the matter of freeing different parts of the body from micro-organisms* is another difficulty. Here, again, we want more statistical knowledge. A few *Bacilli coli* in the urine of a constipated patient, or in a patient suffering from diseases quite independent of colon-bacillus infection, are, I think, too often regarded as enough evidence upon which to base a diagnosis. If some recent work of Conradi's is confirmed, it would appear that not only certain mucous passages but also the solid organs of the body are, in health, constantly being invaded by small numbers of micro-organisms, *Bacillus coli* and *Streptococcus fæcalis* especially, and that these are as constantly being killed. A continual hammering away at the tissues by these and other micro-organisms is always proceeding. If this be so, are we right in believing that this occasional presence of micro-organisms necessarily means disease, or that we can hope to chase away all micro-organisms by vaccines?

(iii) A third of my difficulties is *the question of recurrences* in such conditions as boils and pustular acne, after prolonged courses of vaccine treatment. I have seen several such cases. And not staphylococcal cases only; the same thing happens in pyorrhœa alveolaris (*streptococcus*) and pyelo-cystitis (*Bacillus coli*)—and the response to vaccine therapy becomes less and less marked each time. What, then, is the net result of the treatment to our patients? Have we permanently weakened their resistance by the employment of artificially-introduced antigen, or are the recurrences quite independent of our treatment? I only mention this matter as one worthy of consideration.

I will conclude by a statement of my opinion that vaccine therapy is an effective method of combating an important factor—it may be the most important factor—in the struggle between the tissues and many

infecting agents. I believe, however, there are other factors in this struggle which are not touched by the use of the vaccines, and that this natural limitation to the use of vaccines does exist, though at present the limitation is quite undefined.

Dr. D. W. CARMALT-JONES: I am deeply sensible of the honour of being invited to take part in this discussion, and I recall that I was asked to say what little I have to say from the point of view of a clinician. I have heard in this discussion that a clinician is a person who relies on his unaided senses for his diagnosis, and that clinical medicine is an inferential art. The latter statement is true; but if it is an accusation, it comes strangely from a bacteriologist, who mixes bacteria serum and leucocytes together, and on the degree of phagocytosis resulting makes a diagnosis of a particular infection. If that is not inference, I think we have yet to learn what inference is. With regard to the clinician and his unaided senses, that statement is true. A clinician does employ simple methods, but he employs them upon every physiological system of the body, and he claims, and I think justly, to be able to determine by these methods whether each system is performing its functions within the limits of health, and which, in any syndrome, is the system primarily at fault. The essential point is that when he does his work thoroughly he examines every system, and knows enough about each to decide when a specialist's opinion is required. Now the lay public have some such opinion as this about their family physician, and as long as they have that opinion, to their family physician they will go, and it will be for him to decide whether a given disease is an infection and whether a skilled bacteriologist's help is required or no. The only alternative is for the public to go direct to the bacteriologist. Let us consider some of the consequences. A man suffers from attacks of abdominal pain; he consults a bacteriologist, who infers an intestinal infection, and spends a fortnight in separating organisms from fæces, with one of which he ultimately inoculates the patient. When the patient gets tired of the novelty he consults a clinician, who makes use of his unaided senses, and finds that the patient's pupils are inactive to light, and that no knee-jerks can be obtained. On the strength of this he places the lesion not in the gut but in the central nervous system, and introduces appropriate treatment. Now surgeons make mistakes like this sometimes, and surgeons are reputed clinicians: how much more will they be made by those who hold physical examination in contempt?

When a clinician has diagnosed a lesion of an organ, probably bacterial in origin, is there any reason why he should adopt vaccine therapy in preference to other methods of treatment? The reason is this: that he has otherwise no means, medicinal or other, of directly attacking bacteria in the tissues. Take such a condition as chronic bronchitis; this is a bacterial infection, and though you may relieve the frequency of the cough, promote expectoration, and relieve dyspnœa by means of drugs, you thereby do absolutely nothing to get rid of the invading bacteria and so cure the condition. The truth of this will be made abundantly plain by a walk through the out-patient hall of any large hospital between the months of October and March; and it is the cause of the profound lack of conviction with which I, for one, embark on the treatment of such a condition by means of drugs.

It has, however, been discovered in the course of the last few decades that the animal organism has the power of making antibodies to any foreign albuminous substances introduced into it, which neutralize or destroy the foreign substance. Further, these bodies are produced in excess of the amount required to neutralize the stimulus, so that the blood of animals so treated contains antibodies, some of which can be demonstrated by suitable means. Now bacteria are such foreign albuminous substances, and the phenomena of recovery and acquired immunity are thus explained, and it appears that when a disease does not end in recovery, but becomes chronic, the mechanism of immunity has broken down; but that if the infecting bacteria, killed to prevent spread of infection, that is in the form of a vaccine, are introduced into a part of body which has not been exhausted, new antibodies will be formed in excess of those required to neutralize the vaccine, and these will pass into the blood stream and be available against the chronic infection. The value of the method is that it can be counted upon to produce a certain effect, and is not an empirical remedy. Fibrolysin, for instance, is a drug much employed of late, and its friends claim great results from it, doubtless with justice; but we are quite ignorant of how it acts. As far as we are concerned, it is merely a fortunate accident that it produces the results that it does; whereas by means of vaccine therapy we are stimulating a physiological property without which life would be insupportable in a bacteria-infested world. This is the great difficulty in the way of accurately gauging the results of a treatment which consists in stimulation of the natural powers of recovery, so that much heart-burning arises over the question of whether a given improvement was spontaneous or induced.

These, then, are the reasons which have weight with clinicians with regard to the value of vaccine therapy.

The next question for the clinician is, how this method, so obviously satisfactory in theory, works in practice. In the course of this discussion some eminent clinicians have reported their results, which are sufficiently brilliant. We have, I think, heard at this conference what may be called the extreme case for vaccine therapy. We have heard of case after case snatched from the jaws of death; we have heard of striking benefits conferred on sufferers from diseases not primarily bacterial, such as cancer; even such an unpromising field as miscarriage has not proved sterile. I think nothing that I could say would add anything to this weight of evidence of what is attainable by vaccine therapy. In fact, I think it is high time for quite a different note to be struck. It is very likely that practitioners who are unacquainted with the method have attended these meetings, in order to learn what they may achieve thereby. I can imagine such persons leaving this conference under the impression that immediate success in the most desperate cases is assured if they can only obtain appropriate vaccines; they might even doubt if there was such a thing as failure. I do not suppose for one moment that that is the impression which the speakers intended to convey, but I do think that it is one likely to be left on the minds of an inexperienced audience. Well, I have been associated with St. Mary's Hospital Inoculation Department for nearly four years, and I have seen some remarkably fine results; but no impression of such unvarying success is left upon my recollection, and I fear that anyone who embarks upon vaccine therapy in that hope is laying up for himself considerable disappointment.

Last year I published in the *British Medical Journal* the results obtained in 367 consecutive cases of chronic tuberculosis treated at St. Mary's Hospital. I found that in tuberculous glands, 1 case in 3 was cured, and 5 in 8 much improved; of tuberculous ulcers, 1 in 5 was cured, and 3 in 5 much improved; of tubercle of the genito-urinary tract, 1 in 5 was cured, and 3 in 7 much improved; of lupus, 1 in 8 was cured, and 1 in 2 much improved. Considering what tubercle is, I think these results are extremely good and most encouraging. In the glands, for instance, there were 79 cases; 27 were cured, and 22 were much better. And these were bad cases: more than half had been operated on, some as many as ten times. Now the treatment of these cases lasted from six months to about two years, and sometimes longer, so a good deal of patience and perseverance is required to attain these results; but I think

they are such as anyone might hope to attain by taking sufficient trouble.

I think some very obvious limits are set to the usefulness of purely antibacterial treatment in disease, particularly in such a disease as tuberculosis, in which large areas of necrotic or caseous tissue occur. No blood-vessels pass through such tissue, and blood-serum containing fifty times the normal amount of immune bodies would produce no effect upon it; and, whenever practicable, it should be removed. Sir Almroth Wright did not lay stress on this point in his opening address, but he has done so on many previous occasions, and has demonstrated that pus from an abscess is poorer in protective substances than the circulating blood. I think it may be laid down as an axiom that all sources of irritation should be removed at the outset of treatment. Vaccines have no power whatever to remove them, and they must be removed by surgical methods. In cases of glands in the neck, carious teeth or pediculi must be removed, sequestra must be removed from ulcers, stones from bladders; the presence of these foreign bodies damages the tissues and lets in the infecting organisms, and unless these are removed it is quite unreasonable to expect a cure. Bacteriologists, I venture to think, are apt to overlook these points; it remains for a clinician given over to inspection and palpation, and such-like pharisaical observances, to make reasonable suggestions with regard to other methods of treatment. Dr. Bulloch told us with immense pride of a case of breaking-down glands in which he had suggested letting out the pus. To a clinician it appears merely common-place; the first piece of clinical work which the trembling fingers of the casualty dresser are allowed to perform is to stick a knife into an abscess to evacuate the pus. The reproach is raised that this is surgery. Of course it is surgery, and surgery is essential to the success of many cases under inoculation treatment. The same applies to some remarks of Dr. StClair Thomson at the opening meeting. If a nasal sinus is full of pus, no amount of inoculation will remove that pus—the case requires, as he puts it, not a gardener, but a plumber—but the removal of the organisms embedded in the mucous membrane within reach of the tissue fluids depends on the development of protective substances, either spontaneous or induced. At a recent meeting of the Odontological Section of this Society the use of vaccines in pyorrhœa alveolaris was under discussion, and several gentlemen regretted that no cases were recorded in which inoculation had been the only method employed. To ask for good results under such conditions is to ask

for the impossible. The mouth is a septic cavity, and teeth coated with tartar and impregnated with organisms must reinfect the gum. Inoculation will only touch the deep-seated organisms, the superficial ones must be treated by local antiseptic remedies.

The days of miracles have gone by; we no longer pour water over our sacrifices and expect fire to come down from heaven to consume them. Inoculation cannot be expected to succeed in the presence of dead tissues; surgery and medicine cannot deal successfully with chronic infections in which natural immunity has broken down. Leaving entirely out of account the value of proper clinical treatment of symptoms, we may feel fairly convinced that the best results will be attained by an intelligent collaboration of the clinician and bacteriologist, each having a wholesome respect for the province of the other.

Dr. F. Rufenacht Walters: My experience of vaccine therapy is almost entirely confined to the treatment of cases of pulmonary tuberculosis or phthisis in a sanatorium. Many of these recover promptly and satisfactorily under the ordinary sanatorium methods, but there are many others that do not do so. In many cases the fever persists or recurs, and the treatment is therefore exceedingly tedious and apt to lead to much destruction of lung tissue. These I now believe to be chiefly, perhaps entirely, cases of mixed infection. Here one requires something more than the ordinary hygienic methods adopted in sanatoria. Treatment by antipyretics is apt to be disappointing, and I think it is especially in this class of case that vaccine treatment is likely to be of considerable service. Until comparatively recently, owing to considerations of expense, it was only exceptionally that I employed vaccine treatment other than tuberculin; but lately I have been treating all the obstinate cases, and all those with high fever in which the existence of mixed infection was proved, with corresponding vaccines. My results are, to a very large extent, too recent to deal with now; I shall have to leave many to a future occasion. Tuberculin, however, I have been using for longer, chiefly in small fractional doses ($\frac{1}{100000}$ mg. to $\frac{1}{10000}$ mg. of T.R.). As far as my experience goes, one is likely to get satisfactory results in the vaccine treatment of cases infected with *Staphylococcus pyogenes aureus* or pneumococcus. My experience with *Staphylococcus albus* vaccine has been disappointing.

I may mention a rather striking instance of vaccine treatment for *Staphylococcus aureus* infection dating from 1906. A medical man,

aged 42, came under my care in April, 1906, with extensive lesions in the left lung and slight damage to the right upper lobe. Pulmonary tuberculosis had been present for fifteen months, and he had had during the year before admission two attacks of hæmoptysis, two of influenza, diphtheria, right pneumonia, and finally whooping-cough, which caused his final breakdown. The pneumonia does not appear to have damaged the right lung to a noticeable extent, and in spite of his repeated illnesses he kept at work most of the time until Christmas, 1905. On admission there were many râles after cough from the second rib anteriorly nearly to the base, marked friction above and inside the cardiac area, slight dullness in the left supra-spinous and upper interscapular regions with deficient air-entry, but no adventitious sounds there even on cough. Both bases were resonant. The temperature on admission ranged between 100° F. and 102° F. *per rectum*, and, in spite of complete rest in bed, rose more and more, until it rushed up on June 3 to 106·4° F. At this time the consolidation of the left lung extended downwards to the eighth rib behind and the cardiac area in front, with diminished breath sounds and a few râles, while at the side there was a basal patch with dullness, diminished vocal fremitus, but no tubular breathing or cardiac displacement—i.e., the signs were those of tuberculous rather than of pneumonic consolidation. There were also a few râles at the right base, front and back. A culture was made of the sputum, revealing pneumococcus and *Staphylococcus aureus* as well as tubercle bacilli. The opsonic index was nearly normal against pneumococcus, fluctuating against tubercle, low against the staphylococcus. A vaccine was prepared of the latter, and in the meantime a dose of tuberculin ($\frac{1}{20000}$ mg. T.R.) was injected *sub cute*. The temperature rose in six days to 105·2° F., after which it began to subside. Two doses of the staphylococcus vaccine (20 millions each) were given at intervals of about a week; and within three weeks the temperature was almost normal, with a corresponding improvement in the physical signs in the lungs. There were therefore eleven weeks of high fever, brought down within three weeks by one dose of tuberculin and two of staphylococcus vaccine.

I am not now in the habit of giving so large an initial dose of tuberculin as $\frac{1}{20000}$ mg. T.R.—at all events, where marked fever is present—although it did no harm in this case. Some people are of opinion that highly febrile cases of tuberculosis should not be treated with tuberculin, believing it to be a dangerous practice; but such is not my experience. I think it is very largely a question of dose. Dr. Latham has pointed out that the appropriate minute doses of tuberculin will bring down

temperatures in febrile tuberculosis, and I can confirm this. I think, if you start with a tiny dose, and feel your way—I am in the habit of relying partly on the opsonic index and partly on other considerations—you will find a dose which will reduce the temperature. It is a familar fact to those who have used tuberculin that a certain dose of tuberculin will raise temperatures; but it is not sufficiently recognized that an appropriate dose will reduce temperature. Personally I have found that a tiny dose of $\frac{1}{1200000}$ mg. T.R. to $\frac{1}{800000}$ mg. will cause a lowering of temperature next day, and, according to the dose, this lowering of temperature will persist for one day or more. The same may be said of auto-inoculation by exercise. We know the effect of railway journeys and over-exertion in precipitating an attack of high fever in a tuberculous patient. We do not realize enough that a judicious increase of exercise may have an opposite effect—namely, in reducing the temperature. I have had cases in which a patient has gone up to see a consultant in London, and the effect of the journey has been to bring down the temperature to some lower level. I have kept the patient at rest next day, and presently he had better temperatures and could take more exercise than he had been able to take before the visit. I do not think it can be attributed to the cheering effect of the consultation, but that the railway journey had something to with it, through auto-inoculation; and I can point to other cases in which an extra long walk has had a similar effect. We all know that an injudicious increase in exercise in tuberculosis may very easily raise the temperature for some weeks to an undesirable extent; and therein lies the value of strict rest in bed where a phthisical patient is suffering from high fever; but I have had cases under my care in which the fever has persisted for months, and only subsided when the patient has been allowed to be out of bed for a short time—at first every few days, later on every day. This is exactly comparable with the effect of doses of tuberculin. There are, I believe, two classes of cases in which tuberculin is likely to be of service—those in which even the slightest exertion causes an overdose of auto-inoculation, and those in which a large amount of exercise has but little immunizing effect.

Another point is the administration of tuberculin by the mouth. I have seen several cases in which the method has been most successful, and many in which it has done some good without any ill effects. But there are two drawbacks which are sometimes met with. There are cases in which it causes digestive disturbances, such as pain in the abdomen and nausea; another drawback is the uncertain effect of a

given dose. I had a patient to whom doses were given by the mouth gradually raised from $\frac{1}{40000}$ mg. to $\frac{1}{3000}$ mg. of T.R. They did not seem to have any effect, and I was doubtful whether they were large enough; they caused no febrile reaction or constitutional disturbance. I decided to give a few doses subcutaneously; and, relying on previous experience and the counsels of others (such as Dr. Latham) who have had a larger experience of mouth administration than I had, I gave $\frac{1}{100000}$ mg. *sub cute*. There was, however, a violent reaction, the temperature rising to 103·6° F., and for three weeks there was much more fever than was desirable. Then I gave $\frac{1}{120000}$ mg. *sub cute*, and even this caused a reaction to over 103° F. In this case a large proportion of the tuberculin was probably not absorbed from the alimentary canal, so that the effect was almost nothing, while the dose by the mouth was no guide to the required dose *sub cute*.

In conclusion, my own impression is that it is a mistake to regard phthisical cases as merely cases of pulmonary tuberculosis. For some reasons it is a pity that the old term "phthisis" is discountenanced. In some ways it gives a truer impression than the modern name. In many cases the infection is a mixed one, when it seems to me that the best treatment is a combination of all the appropriate recognized remedies, hygienic and medicinal, together with vaccine treatment, directed both against the tuberculosis and the associated organisms.

Royal Society of Medicine

June 15, 1910.

Dr. J. Mitchell Bruce, Vice-President, in the Chair.

Discussion on Vaccine Therapy: its Treatment, Value, and Limitations.[1]

Mr. W. Deane Butcher: The subject which I wish to bring to your notice is the "Production of Auto-vaccination by Electrical Methods." I do so with some diffidence, since it is outside the lines of the previous discussion, and I fear, in the time at my disposal, I may not be able to throw much light on this somewhat obscure subject, which is, however, of the deepest interest to one of your Sections, and, I believe, of growing importance to all.

The hypothesis of electro-vaccination, or radio-vaccination, has gradually grown up to account for a number of phenomena otherwise inexplicable. The evidence in any one direction is not very abundant or very conclusive, but the cumulative evidence along various converging lines is, I think, considerable. The first thing that led to the suspicion of the possible vaccinal action of the Röntgen rays was the fact that a number of skin diseases of totally different origin and nature seemed to improve under exceedingly small doses of the irradiation. Psoriasis, lupus, eczema—all were apparently benefited. An acne that had resisted treatment for years would disappear under the rays, just as if the case had been treated by the injection of a vaccine. What more

[1] Fifth meeting (adjourned from June 8).

natural than to suppose that the patient himself had furnished the necessary vaccine, under the stimulus of the Röntgen irradiation? The hypothesis received still further confirmation when cases of lupus were reported in which an irradiation of one region—the leg, for example—was followed by improvement in another region, such as the face. The same thing was observed in other diseases. Acne of the face was cured by X-ray and high-frequency treatment applied to similar lesions on the back and shoulders; high-frequency effluves applied to a crop of boils would apparently render the patient immune to further inoculation; lupus erythematosus of one region was influenced by X-ray treatment of an adjacent part, and even widespread carcinoma of the breast was influenced by irradiation of only a portion of the affected area. The hypothesis received further corroboration from the observation that in certain cases of lupus, and even of cancer, irradiation of the affected glands was followed by an improvement in the lesion itself.

Let me give you a few instances to show the sort of evidence on which we base our case. They are but impressionist sketches, but perhaps may give as accurate an idea of the facts as a more formal picture, duly authenticated by initials, dates, and curves:—

A young soldier—turned out of the Army as incurable—deeply pitted with variolar acne—face, neck, shoulders, and back covered with indurated nodules and suppurating abscesses. He is cured in a few weeks by X-rays and high-frequency effluves. No other treatment is given, either external or internal, and the abscesses are not even opened; they gradually shrink, the adjacent skin becomes dry and brawny; the fluid contents of the abscesses dry up, with the final evacuation of a tiny plug of inspissated pus. There is no further formation of pustules. The man is immune to staphylococcic infection.

A poor governess comes in much the same condition—pale, anæmic, half starved, with pustular acne of many years' standing. She is treated with high-frequency effluves—a mixture of sparks, X-rays, and ultra-violet light. The back and neck are chiefly treated, but the face also rapidly improves. The nodules disappear, the abscesses dry up, there is no further development of pustules: the case has been cured by electrical vaccination.

A young naval officer has been plagued all his life with slight acne. He is treated with two very slight irradiations by X-rays. The treatment is unexpectedly interrupted by his return to his ship. Nevertheless he is cured, and has no return of his life-long complaint. He has been rendered immune by radio-vaccination.

A nurse has lupus in the posterior nares, and begs for X-ray treatment, although she is told it will be absolutely useless. Only two small doses of X-rays are given, such as one would expect could hardly reach the affected region. Nevertheless the case is greatly benefited, perhaps cured.

A married woman is treated for psoriasis by X-rays, with the result that there is a premature menopause, and a menorrhagia of many years duration is cured.

One could easily add to these cases many others, to prove the profound influence of electric and radio-active treatment on the general organism. Even the ordinary process of Röntgen epilation appears to be a biological rather than a merely destructive phenomenon. It is, at least in part, a vital reaction—a phagocytosis, since it has been found that the process of epilation is more rapid in the diseased areas, and is delayed by the application of disinfectants, and the consequent depression of the processes of vital reaction.

But the strongest evidence of the vaccinal action of electrical treatment is derived from a consideration of the various modes of attack on rodent ulcer. In the whole purview of medicine there is nothing more impressive or more certain than the cure of a small rodent ulcer by electrical methods. But the cure may be effected in many ways—by Röntgen rays, by radium, by ultra-violet light, by the high-frequency effluve, or by zinc ions. What is the common factor in all these cures?—cure, I say, for the process is not one of destruction but of repair. It is not merely a bactericidal action, nor is it merely a selective destruction of tissue. It is a biological recoil—the resentment of the cells of the organism to the insult of the ethereal vibration. The habitual defence of living cells to such an insult is exactly the same as that to a bacterial invasion—namely, the production of an antitoxin or other antibody. It is a true vaccination.

I believe that Sir Almroth Wright is of opinion that the therapeutic effect of Finsen light and X-rays is in fact vaccinal in its nature, but that they act only by their congestive action, bringing a larger supply of lymph into contact with the diseased tissues. This is manifestly not the whole truth, as anyone may convince himself by contrasting the effects of Finsen light, of radium, and of X-rays on a lupus nodule. The supporters of our hypothesis maintain that the X-rays themselves assist in the elaboration of antibodies or the like, at the site of the irradiated lesion, or more probably in the adjacent glands.

Our hypothesis then supposes that the ethereal waves contribute to the process of auto-vaccination by the production of opsonins, by the detachment of the side chains of Ehrlich, by facilitating the reaction of toxin and anti-toxin, or by setting free the vaccine from its laboratory in the glands. This is rendered the more probable when we reflect that the toxin and anti-toxin carry opposite electrical charges, one flowing down and the other flowing up the electrical stream. Not only so, but we have recently had evidence that the micro-organisms themselves may be carried along an electrical current.

All this gives rise to more than a presumption that the therapeutic effects both of electro-therapeutic and radio-therapeutic treament may be due to the production or liberation of vaccines. We knew that a glandular tumour of the neck would melt like snow before the Röntgen rays, but we did not know why. We knew that the X-rays could produce both *in vitro* and *in vivo* a cytolytic serum, or even a spermolytic serum, but we did not know why. We now begin to suspect that the common factor in all these phenomena is akin to that with which we are familiar in ordinary vaccination.

One of the greatest arguments in favour of the vaccinal hypothesis is the latent period which follows Röntgen or radium irradiation, and which precedes the reaction. This ominous pause is, to my mind, eloquent, and indicates that all the resources of the organism are being called upon to resent the insult. The reaction is not merely a physical or chemical one, but a biological reaction, in which the energy of the recoil may exceed the energy of attack.

Let me explain. If I strike this table, it returns the blow immediately, and strikes my knuckles with the exact force expended; whereas if I were to strike my neighbour he would probably return the blow with interest, an interest that would be rather increased than diminished by a brief period of latency. Similarly the pause after Röntgen or radium irradiation is proportional to the degree of the insult, and is a participation of all the complicated mechanism of organic resistance called out to oppose the electrical or ethereal invasion.

It is not the place here to advocate the practical uses of electrical treatment, but I do urge the use of radio-active and electrical methods as instruments of research in the study of vaccination. For this purpose I would most strongly urge the collaboration of at least three of our Sections — the Dermatological, the Pathological, and the Electro-Therapeutical. Such a collaboration is the special note, the very *raison d'être* of the Royal Society of Medicine. It is useless for a pathologist

to assert that X-ray or radium irradiation has no influence on the opsonic index, unless he is familiar with the practical methods of giving accurate and measured doses of the irradiation; just as it is useless for an X-ray specialist to report cases of cure which are not verified by the pathologist and the clinician.

I can only briefly allude to two recent developments of electrical treatment which may have a practical bearing on the production and control of auto-vaccination. These are the methods of "thermo-penetration," and that of "muscular exercise electrically produced." By the former method, that of thermo-penetration, we are able to produce a considerable, a definite, and an easily regulated rise of temperature in a deep-seated tissue, such as a diseased joint, a gland, or an internal organ. It is easy to imagine how potent a means of control this method may give us for the production and regulation of auto-vaccination, or the supply of lymph to a diseased organ. Still more important results may be obtained by the second method—viz., "muscular exercise electrically produced." This method, which has been largely used in the cure of obesity and the like, gives all the good results of muscular exercise without its attendant nervous and mental fatigue. I would venture to suggest that it should find its place in the treatment of phthisis by auto-inoculation, as it can be better regulated and is not so exhausting to the invalid as exercise produced by an effort of the will.

Time alone will tell if our surmises are correct. Much time must elapse before we can hope for a practical means of producing or controlling auto-vaccination by electrical means. If the vaccine theory itself is correct—if the production of antitoxins and antibodies is our only means of resisting bacterial invasion—then you will allow that the electrical method of exciting the resistance of the cells, of calling out the home-levies, the militia of the organism as it were, is a great advance on the cruder methods of vaccinal treatment by the injection of foreign-bred vaccines.

In one of his most eloquent passages the "Autocrat of the Breakfast Table" compares the organism to a seventy-year clock, locked and enclosed in its case, and laments the impossibility of influencing its machinery from without. "If anyone would only contrive some kind of lever that one could thrust among the works of this horrid automaton and check them, or alter their rate of going, what would the world give for the discovery?" Hitherto we have striven chiefly by chemical means to influence the cell life which swarms within the closed case of

the automaton. I venture to predict that in the future much more use will be made of the various ethereal undulations which so readily penetrate the case, in order to stimulate, by a sort of wireless telegraphy, that resentment of the cell community which we associate with the phenomena of vaccination.

Dr. H. Lewis Jones: I have only to endorse Mr. Deane Butcher's very interesting and suggestive remarks. He has submitted ideas and experiences from the clinical side, but the phenomena clearly demand the study of the clinical pathologist. The technical difficulties of serum therapy and vaccines are considerable, and indeed the terminology of the subject is so difficult as to put these investigations out of the reach of the electro-therapeutic specialist. The observations which have been made by more than one observer on the healing of lupus in one part of the body after irradiation of a different part of the body have been received in the past with profound disbelief. These facts, if facts they were, demand an explanation. I have believed them, although unable to explain them, and Dr. Butcher has put forward an explanation which may prove the right one. That blood changes do occur after X-ray exposures is well shown by the action of X-rays in leucocythæmia, and experiments have also shown that leucocytes react to serum from an X-rayed animal in a way which differs from their response to normal serum. Even the irradiation of an isolated part, such as the ear of a rabbit, will suffice to set up general changes in the leucocytes of the general blood.

Dr. Crace-Calvert: I have only a few remarks to make this afternoon, and they are chiefly from the clinical aspect, and mostly in regard to cases of tuberculosis. I have now been working in connexion with vaccines almost from the beginning, in a sanatorium, and most of my experience is in regard to tuberculosis of the lungs. But I have had a few cases of other conditions, which I will mention later. I may, however, say that so far as I have gone, I have been very much impressed by the results of vaccination, using more especially the tuberculin emulsion (T. E.) in preference to T. R., which is often employed. Most of my cases I have selected because they were not doing well on ordinary sanatorium methods, and I have not therefore chosen many early cases; in fact most of the cases have been fairly

advanced, and cases which were largely stationary. They had improved to a certain extent under sanatorium treatment, and then they wanted, as it were, a fresh stimulus to enable them to complete the cure, using the word " cure " in the proper sense of arrest. In most of those cases I have found that the addition of tuberculin to sanatorium treatment has resulted in a stimulus to the case, so that the signs which previously had remained more or less stationary have tended to clear up and finally to disappear. I have noticed, I think, most of the signs which Dr. Latham and Dr. Inman mentioned in their papers—flattening of the temperature curve, decrease of the cough, lessening of the sputum, &c. I have had rather odd results at times, such as increase of sputum in one case and decrease in another, and I do not know exactly why, because it does not seem to depend quite on the dose. But the variation in the symptoms does not seem to have made any difference to the progress of the case, and I do not think it has depended altogether on the dose, but rather upon the patient. I have also inoculated a few of the early cases, and they have all done well. I do not think early cases of tuberculosis of the lungs are fair tests of the value of the vaccination, because with appropriate sanatorium treatment they nearly always do well without vaccination, and it is not fair to ascribe their improvement to the effects of tuberculin.

Just lately I have been doing what I think one is often told not to do, and that is, giving tuberculin in acute cases. I have had two or three patients with whom I was at my wits' end to know what to do; they were going downhill, the temperature rising and the physical signs increasing. In my first case every time I gave an exceedingly small dose there was at first a rise of temperature, but the result was extremely good, because that patient lost the whole of his physical signs; the temperature dropped to normal, he put on weight, and left the sanatorium with his disease arrested. He came back again four months later with typhoid fever and a serious relapse of his tuberculosis of the lung, the condition being mistaken for acute tuberculosis. The typhoid cleared off and his temperature dropped, but tuberculosis was left, and his temperature was rising again. I started him again on a very small dose of tuberculin, and he began to respond, and now the temperature is nearly normal. The physical signs have lessened and the cough and expectoration have almost disappeared, and he is apparently going to get well, though a larger area of lung was affected the second time. I have tried it in two other acute cases; one was a success, and in the other it made no difference. The point of that small experience

seems to be to show that a great deal depends on the dose. Tuberculin is very powerful, and if it is worked with in minute doses and given properly it will flatten the temperature chart and help to arrest the disease.

Lately I have not worked very much with the opsonic index, as I have given tuberculin and other vaccines by the mouth. That is not altogether because I do not believe in the opsonic index, but I have not time to work it out as I should like to. Much has been said against the opsonic index by those who have not been able to work at it properly. It means you have to stick at it, not only daily, but hourly, before you can be sure of accurate results. The first few cases which I worked at by the opsonic index gave very good results, and if I could I would do the same now; but my circumstances are rather altered, and I cannot give as much time to working it out as I should like. I have used vaccines with mixed infections, in tuberculosis and so forth, with very good results. There was one case in particular where a patient with slight tuberculous disease of one lung had a sudden rise of temperature to about 103° F., which took a week to subside, only to be followed a week later by another rise. This occurred three times, and then I obtained a pure culture of streptococci from his sputum and made a vaccine, which I gave by mouth, and he had no further attacks. But here, again, the number of cases treated has not been sufficient to warrant my speaking absolutely definitely, though in most of the cases I have achieved the result which I wanted—namely, to get rid of all the other organisms from the sputum, leaving the tubercle bacillus only. And where there has been an erratic temperature I have usually succeeded in bringing it down to normal. I have used a vaccine also for middle-ear disease, where the sole organism was the *Bacillus pyocyaneus*, and met with success up to a certain point. But just as the mischief was quieting down, the lady developed hay fever, and the trouble flared up again; the case is still under treatment. The vaccine has been stopped by the advice of the surgeon, but the ear is now discharging as freely as ever, and the organism is still limited to the *Bacillus pyocyaneus*. I had one case of cystitis where the infecting organism was the *Bacillus proteus*, and that has improved immensely by vaccinations with that organism. I had one case of tuberculous peritonitis which I treated with T. R. with good results, but in the one or two tuberculous-gland cases that I have vaccinated I have not met with much success.

To sum up, I am certain that in the case of tuberculosis of the lungs especially there is scope for a great deal of work to be done with vaccines, and I am sure that that work will repay the time and labour if the doses are used with care and not pushed unduly. Tuberculin is a powerful remedy, and so the doses must be very minute, but if it is used with judgment, especially in conjunction with sanatorium treatment to build up the patient's general condition and resistance, I feel sure that the results will repay us.

Mr. LEONARD NOON: I want to ask you to consider what is an ideal vaccine—what should be its constitution and what properties it should possess. As has been pointed out already in the course of this debate, a diseased animal receives into its system, from the site of infection, products which may be grouped under three headings: (1) The first of these corresponds roughly to the protoplasm of the bacterial body, and gives rise, in the animal, to the production of opsonins, of amboceptors, and of agglutinins; (2) The second includes toxins and endotoxins, a series of bodies which cannot be defined very easily, but which we may roughly describe as soluble products of bacterial growth, which exert a toxic action when injected into animals; (3) Lastly, there are the substances set free by the disintegration of the animal tissues at the site of infection. In the course of disease, and in response to this autoinoculation, the body elaborates antibodies corresponding to all these different products, and in many cases recovery is the result. But in many others, on the contrary, the immunity is not sufficient, and the natural process fails. This is the place where medicine steps in, and claims to improve on the processes of Nature; for I think that the moment we begin to practise therapeutics we are assuming a kind of authority over Nature's workings, and that we should recognize this fact and not try to shirk the responsibility which it carries with it. The masters of our profession who have claimed, in the past, to follow with humility Nature's lead were insisting on what was at the time the more important point of view, but a point of view which cannot be considered to embrace the whole truth.

Now in vaccine therapy we try to make use of a natural reaction, but we try also to increase this reaction beyond the limits it would attain without our interference. This we can do because we have learnt that in every disease some tissues are left at least comparatively healthy, and that these tissues may be stimulated to activity for the

benefit of more gravely injured parts of the body. We have also learnt that the auto-inoculations of disease may be either deficient or excessive, whilst our therapeutic inoculations can be regulated at will. Thus far most of those who have taken part in this debate will be agreed, but beyond this point there seems to be a wide divergence of opinion. There are those who consider the labour and uncertainty of further investigation, and would rather devote their time to reaping the harvest of our present knowledge. On the other hand, there are those more hopeful beings who are convinced that a more detailed analysis of the processes of immunization will be rewarded by the discovery of wider fields of practical utility in the future.

One line of inquiry deals with the constitution of the vaccine itself. The heated bacterial emulsion generally used is obviously a very different thing from the living bacteria themselves, and from the bacterial matter which escapes from an infected focus into the surrounding tissues. These differences can only be investigated, at present, indirectly, the chief method being that of inoculation into man or animals and measurement of the reaction set up. Investigation by this method will answer the questions as to whether our heated vaccine will give rise to increased production of opsonins, amboceptors, agglutinins, antitoxins, antitrypsin, and the like. Let us review the present state of our knowledge on these heads.

Opsonin.—Abundant proof is forthcoming that the opsonic power of the serum is increased by inoculation of heated vaccines. In the case of one organism I was able to compare the opsonic response to inoculations of living cultures and emulsions sterilized by heat. The organism was the *Bacillus pseudo-tuberculosis rodentium*. The experimental animals were rabbits; these animals are easily killed by the inoculation of living cultures, so that partially immunized rabbits had to be used. This had the further advantage that the reaction was more rapid than in the case of untreated rabbits, and the fallacy introduced by the multiplication of the living microbes after their introduction was thus minimized. A batch of rabbits was rendered partially immune, and then one lot were inoculated intraperitoneally with living, the other lot with killed emulsions. It was found that the emulsion sterilized by heat was competent to produce a rise of the opsonic index in doses smaller by many times than the living emulsion. The minimal dose of the latter was ten or fifteen times greater than that of the heated emulsion. It seems probable, therefore, that the active substance is held locked up

within the living bacilli, and set free in available form by the process of sterilization by heat.

Amboceptor.—If we accept Ehrlich's views, and the work of Bordet and Gengou on complement deviation, then we can measure the bacterial antigen in any emulsion by mixing it with a known immune serum containing the corresponding amboceptor and determining the complement-deviating power of this combination. The *antigen* thus measured is supposed to be the substance which stimulates the formation of amboceptor in the tissues. The available antigen in living and heated emulsions of the *Bacillus pseudo-tuberculosis* was compared by this method, and again the advantage was on the side of the heated emulsion.

Antitoxin.—Most of the toxins which can be obtained in workable quantities are destroyed by heat, so that our heated vaccines are, generally speaking, free of these toxins, and probably, therefore, do not give rise to antitoxin production in the body. The heated vaccine is not, however, to be condemned on this score, for the presence of a notable quantity of toxin would limit the dose which could be employed with safety. With such a vaccine the toxin would be the predominant factor in determining the dose, and we should, of necessity, aim at producing antitoxic immunity, whilst other forms of resistance would be lost sight of. But these latter are the more important, because they are directed to the removal of the infecting agent. It is therefore an advantage to use an atoxic vaccine in what is essentially an antibacterial treatment. At the same time, one may admit that there is room for an antitoxic treatment in some cases, either by the use of antitoxin as in the case of diphtheria, or by the inoculation of small doses of toxin, regulated to excite an active immunity. I would only insist that this latter scheme of treatment is one of which we have as yet no accurate knowledge, and which will probably present difficulties of its own. Its rules need to be worked out separately almost from the beginning. The attempt to induce antitoxic and antibacterial immunity by one and the same inoculation seems to me only likely to lead to confusion and failure.

Antitrypsin.—The products of disintegration of the animal tissues, which flow into the system from the site of infection, no doubt give rise to antibodies of various kinds, but at present we are ignorant of all these except one—antitrypsin. The inoculation of bacterial vaccines, as a general rule, increases the antitryptic power of the blood serum, this reaction being explained by the breakdown of some of the leucocytes which crowd the tissues at the site of inoculation.

From the above it is clear that the reaction to inoculation is a complicated one, corresponding to the complex constitution of the vaccine. We cannot expect to obtain a reaction which is the best possible in every respect. Where the conditions are such that ordinary clinical examination enables us to judge between success and failure in the case of each successive inoculation, we are watching a summation effect, and we say the balance is on the side of improvement or the reverse. But where the case is more difficult and we cannot thus sum up the results of an inoculation, we are driven back on blood examinations, and we have to select one or more of the components of the total immunizing reaction for measurement. The opsonic reaction derives its pre-eminent importance from the fact it is almost universally applicable. All animals use the phagocytic method of defence against bacterial invasion, and, among mammals at least, phagocytic activity is dependent on the opsonic power of the blood. This holds true for all bacteria yet investigated, and the opsonic power may be measured by one and the same method in nearly every case.

There is one point of practical detail which has been raised in this discussion to which I should like to refer—namely, that of stock vaccines. Dr. Whitfield treated this subject rather shortly at the end of his speech. He has found, and I think most of us will agree with him, that staphylococcal vaccines, prepared from recently isolated cultures, are more active than those made from old laboratory strains. The vaccine once prepared will keep for a long time. But these observations on staphylococci must not be taken to apply generally to vaccines prepared from other groups of bacteria. One need only mention the typhoid bacillus, which gives efficient vaccines after going through frequently-repeated subcultures for months or years; whilst, on the other hand, the vaccine prepared to-day will be useless in six months' time, and will have deteriorated in a shorter time still. In the case of pneumococci we are faced with another difficulty, for the pneumococcus isolated from one case of pneumonia may have little effect when inoculated into another patient. But it is very necessary to have a stock vaccine for the treatment of a disease so rapid as pneumonia, where every day is of importance. A stock vaccine prepared from several well-attested strains of the pneumococcus, isolated from cases of acute pneumonia, is found to give good results in nearly every case, and a first dose of this may be followed by the autogenous vaccine after a day or two.

Dr. W. PARRY MORGAN: I propose to confine my remarks to one acute disease, pneumonia. During the last two years, chiefly in conjunction with Dr. Willcox and Dr. Luff, to whom I am indebted for much help and kindness, I have treated many cases by vaccine therapy. In advocating any new form of treatment one is open to the charge of being biased, and this attitude is the more likely in pneumonia when Nature frequently does so brilliantly on her own account. Still she fails in so many cases that it remains the most fatal of acute diseases. This justifies us in looking for new methods of treatment.

Up to the present good nursing, symptomatic treatment, and surgery when necessary, constitute the recognized measures of treatment. These can in no way be called specific, and only help to maintain the patient in a condition in which he himself can deal with his disease. Specific treatment has been tried only in recent years. We have the two principles of passive and active immunity. The former first held the field, but it certainly has not been satisfactory. The latter has been responsible for brilliant results in other diseases, and the few records of its application in pneumonia are those of success. However, it has had but an imperfect trial, and I submit that my experience shows that, although we may expect some failures, further trial is more than justified. There is no doubt that lobar pneumonia may be caused by different strains of pneumococci; hence we cannot expect one vaccine to be efficient in all cases, and the method must not be condemned, as it is by some people, because certain cases do not react when treated with stock preparations.

Again, dosage is difficult and will depend very much on the individual applying the method. There is no doubt that idiosyncrasy is a most important factor in vaccine therapy; thus it is impossible to say that one dose will suit the same body-weight in all cases. This is what we should expect when we consider the variation in susceptibility to infection. In many other diseases the opsonic index has been our guide to the resistance and to the dose, but in pneumonia I have been unable to use it for this purpose. This has been given as an argument against the general use of the index for determining doses. It is quite unjustifiable to quote my failure with the pneumococcus as a reason against the use of the index in infections where its value has been proved. My difficulties have been as follows: Most pneumococci—at any rate, those obtained directly from the lung or pleural cavity—are not phagocytosed except when acted on by immune serum. So striking is this phenomenon that the field of an opsonic

film prepared with normal serum may be crowded with organisms, whilst the phagocytes are empty. Again, the opsonic power of serum of apparently normal people differs very considerably—a fact which is not surprising when we consider the frequency of pneumococcal infections, and the fact that infection leads to variation in immunity. Agglutination of the organism often takes place with immune serum, but the phenomenon is very irregular. Notwithstanding these difficulties I have made many observations, and find that early in the disease the opsonic power is never high, whilst nothing is more striking than the opsonizing power of some immune serum to an organism not acted on by normal serum. It is obvious, therefore, that the determination of the dose is a matter of extreme difficulty, and it will certainly happen that good results will be recorded with large doses which in other cases would cause failure. We must therefore commence with minimal doses and look for minimal effects. Soon perhaps, when we overcome the difficulties of using the opsonic index in pneumococcal infections, we may be able to use vaccine with the feeling that we know precisely what is taking place in the immunizing process.

In the treatment of acute disease there is one obvious limitation—time. What is to be done must be done quickly. Therefore I think that a suitable stock vaccine should be given at once. Subsequent procedure depends on circumstances, the ideal being to obtain the autogenous vaccine. Three methods of doing this are available, and are as follows:—

(1) *Cultivation of the Organism from the Sputum.*—I must emphasize that this method is open to the very grave objection that we might get an organism other than the one infecting the lung. Diplococci are so common in the upper respiratory passages that one may obtain an apparently pure culture, and that not of the pathogenic organism. I have seen this done in a case in which the pneumonia was due to an organism belonging to the coliform group, the diplococcus being obtained from profuse expectoration. Again, my observations point very definitely to the sputum organisms having different reactions from those obtained from the lung, notably as regards phagocytosis. I consider therefore that this method is by no means reliable.

(2) *Blood Culture.*—I have tried this method in several cases. As a rule, I have been unable to obtain any growth from 10 c.c. of blood, whilst sometimes I have not had a growth early enough to prepare the vaccine. I have no doubt that the frequency of extensive blood infection is as variable as the virulence, and although some observers find

the organism frequently, others will fail. Again, the time at which a blood culture is made is an important factor, for no doubt bacteræmia is more frequent early in the disease. A point in favour of this method is the relief it often gives the patient, and the confidence it gives him in the vaccine therapist.

(3) The third method, that of *lung puncture*, was described, not as something new, by Dr. Willcox and myself, and subsequently by Dr. Horder, as a method of diagnosis of thoracic conditions. This was afterwards severely criticised on account of certain dangers, but Dr. Willcox and I maintain that the comparatively fine needle (only a little larger than a hypodermic) which we recommended reduces these dangers to a minimum. I have never failed to obtain blood-stained fluid from a consolidated lung by this method, but I have often been unable to obtain a culture. Therefore, this method, the most rapid, of obtaining a reliable vaccine often fails, and so in many cases we are compelled to use stock vaccines throughout.

Regarding doses, I feel confident that no harm has followed the larger doses I have used, although I have repeated doses of 50 million of the autogenous vaccine. Still, I think my best results have been with doses of from 15 to 30 million for an average adult. If a dose is small, it can be repeated with impunity; whilst if larger, one hesitates because it has been possibly too large. The temperature may be a guide, but often it is not. One of the most noticeable features is the improvement in the general condition, without much change in the temperature. The anxiety which one often feels for a patient is relieved. Sleep comes readily, the appetite improves, and even if the pulse-rate does not fall, its strength is well maintained. At other times there is a fall in the temperature soon after the dose, sometimes even in a couple of hours. If this is the case, another dose is indicated when the temperature rises again. If there is no change in twenty-four hours, I repeat the dose, sometimes increasing, more often decreasing, it. Between subsequent doses I may leave longer intervals. One does feel, however, that this method of determining and giving doses is not satisfactory, and that some measure of the immunity is wanted. We look for some one to suggest an indication which is not dependent on so many functions of the patient as is the case, say, with the temperature which would often lead us to give a dose of vaccine instead of a pill.

Of the cases that I have treated, 24, with two deaths—one from nephritis after the pneumonia had subsided—were reported in the *British Medical Journal*, October 9, 1909. I have directly treated 19 more

since that time, all of which have recovered. Thus, 1 case out of 43 has died of the disease—a result which is not altogether unsatisfactory. Of the 19 cases, only 2 were treated as early as the third day, 4 were treated on the fourth day, 5 on the fifth, and the rest later. Thus it will be seen that my opportunities have not been of the best. Most of the cases have been treated with stock vaccine, but some have had autogenous. Of the 2 cases treated early, one proved to be a streptococcal infection. The other had his crisis on the sixth day and made a good recovery. In a child, aged 7, it was difficult to ascribe any effect to the vaccine. She had a somewhat protracted crisis, after which recovery was rapid. In 4 other cases there was a distinct crisis, one occurring immediately after an inoculation on the eighth day, the others on the sixth or seventh day. In several cases the temperature fell by lysis with marked improvement in the symptoms. These cannot impress any but those actually observing them. My judgment may be biased, but I certainly think that most of these cases were benefited by the vaccine, and this judgment was corroborated by others. If we shall be unable to produce an artificial crisis at will by using a vaccine, I maintain that if we can induce lysis with improvement from the beginning of treatment, we shall have made a great advance in the therapeutics of pneumonia. Some cases were complicated, and these are the more instructive and the most convincing. I will therefore give a brief account of some of them:—

W. H., aged 40, a typical alcoholic, who acknowledged his habit, was taken ill on October 13. He was admitted to hospital on October 15 with well-marked signs in the left lower lobe. The first night in hospital he was very restless, and the next day he was obviously much worse. Towards night delirium tremens threatened, and he was given 30 million stock vaccine. He had a very good night, the restlessness passing off, and was much improved in the morning, when the temperature fell to 99·8° F. Later in the day it rose to 101·6° F. He had another good night, and the temperature in the morning dropped to 98·2° F. On its rising later he was given a further dose (sixth day), after which the temperature only once rose above 99·4° F. It became continuously sub-normal on the ninth day. Recovery was very rapid. All signs and symptoms disappeared within a fortnight.

S., a girl, aged 15, who had previously attended a dispensary for consumption, on November 15 was seized with shivering and pain in the left side. On November 20 she was admitted to the hospital very seriously ill. There was consolidation of the whole of the left lower

lobe and part of the upper left and lower right; pulse 144, respiration 68. The next day patient was no better, so 15 million stock vaccine was given. At the same time a blood culture was made, from which I obtained a profuse growth of pneumococci. A comfortable night was passed, and, although the patient was most seriously ill, the whole aspect of the case had changed by the next day, the temperature falling to 99·5° F. It rose again towards night, so a further dose was given. I had prepared the autogenous vaccine, but the stock had such a marked effect that I repeated it rather than give the new. The temperature did not come down, although the patient was obviously improved, sleeping better and coughing less. I gave 10 million of the new vaccine on November 23, and 15 million on November 24. The temperature fell to 99·2° F. on November 25, and to 97·8° on November 26. However, it did not remain normal, and ranged several days below 100° F.; after that it became intermittent, and I began to look on the case as a failure. The lung was twice explored with no result, no cultures being obtained. I therefore estimated the tuberculo-opsonic index on four occasions, one being before, the others after, forcible deep breathing. There was such a variation that the presence of pulmonary tuberculosis was made certain. Tuberculin was given in very small doses until the patient left hospital for a convalescent home. Some signs still remained, but no cough nor sputum. The last report, for which I am indebted to Dr. Willcox, is that she had quite recovered.

My next case is that of an old man, aged 61, with very hard tortuous arteries. He had been ill for a week when admitted to the hospital on November 28. The whole of the upper two right lobes were consolidated, whilst both lower lobes were full of crepitations. The pulse was very irregular and feeble, and the patient's mental condition confused. The prognosis appeared most grave, and Dr. Luff gave no hope. Sixteen million stock vaccine was given. Next day the old man was better, the temperature and pulse lower. That evening a further dose was given. Two days later the temperature was normal. However, it began to rise again after that, and became intermittent for nearly a fortnight. Notwithstanding, the old man appeared very well and cheerful, eating and sleeping well. The pulse was much improved. The consolidated lung cleared up, and the crepitations disappeared after a time, and on January 6 he went to a convalescent home quite recovered.

G. H., a girl, aged 1 year and 9 months, was admitted on March 2 with signs at the right base. The temperature fell slowly until

March 5, when it rose steadily until March 8, when I was asked to inoculate. I gave three injections of 3 million stock vaccine, but with no result, the patient getting gradually worse. On March 15 the prognosis seemed hopeless; nevertheless, I did a lung puncture, and within eighteen hours inoculated her with 4 million autogenous pneumococcal vaccine. The effect was most striking, the temperature falling immediately. Further doses were given, and three days later the temperature had fallen by a steady lysis to normal. It began to rise again on March 25, but fell immediately on giving a further injection of 4 million. She steadily improved, but some dullness remained in the right chest when she left hospital. This has since completely cleared up.

E. H., a woman, aged 31, who had been treated for pulmonary tuberculosis at several sanatoria, was admitted to hospital for treatment with tuberculin, under which she improved, the temperature which she had on admission becoming normal after several weeks. However, the cough remained bad and expectoration profuse, with large numbers of tubercle bacilli, whilst the appetite was very indifferent. About the beginning of the third week in February she developed a temperature again, with pain in her right side. Dyspnœa became marked, and the general aspect of the case serious. The sputum was examined, and a few pneumococci and many tubercle bacilli found. A dose of 15 million pneumococcal vaccine was given. A few days later the pleural cavity was aspirated, and a considerable quantity of fluid obtained. This clotted very quickly, and in the clot were found tubercle bacilli and many pneumococci. I was unable to obtain a culture of the latter, but it was obvious that the patient was suffering from pneumococcal pneumonia complicating the phthisis. I gave stock vaccine, after which the patient rapidly improved. The whole result of the attack was a well-marked change for the better in the tuberculous condition, for the cough and sputum almost entirely disappeared, so that she could sleep through the night, a thing she had not done for two years. The physical signs, too, showed a corresponding improvement in the lungs. A point of extreme interest in this case was the behaviour of the opsonic index. It was above 1·10 on twelve consecutive occasions. On account of these very high readings tuberculin was withheld. Subsequently it was again given, the dose being regulated by the index. The patient continued to improve, and the last reports from the sanatorium, where she is undergoing graduated exercise treatment, promise complete cure.

S. B., a child, aged 10 months, had been ill for three weeks. On May 31 she became rapidly worse, and was admitted to hospital the next day with very obvious dyspnœa and cough. There were well-marked dullness and bronchial breathing at the right base, with crepitations. She was given 4 million vaccine at 6.30 p.m. on June 2; the temperature fell steadily to 100·4° F. at six the next evening. At ten it was 102° F., so I gave 15 million; the temperature again fell steadily to 99·6° F. next evening. When it rose I gave a further 4 million. It again fell, this time to 90·2° F. The general condition of the child meanwhile improved in a most remarkable way, and, except when coughing, she had a healthy appearance. However, it became obvious that she had pertussis as well. Although so severely ill when she was admitted to hospital, it was thought quite safe to send her away rather than isolate her. I saw her yesterday, when her condition was quite satisfactory.

A. T., a man, aged 44, with typical signs in the right lower lobe, was inoculated with stock vaccine on the third and fourth days. There was no apparent effect, the patient being very restless and getting worse, so a lung puncture was made, and from the cloudy fluid aspirated from the pleural cavity a pure culture of *Streptococcus mucosus* obtained, and a vaccine prepared. Fifteen million of this was given on the fifth day. The patient afterwards passed a good night, and the next day the temperature fell to 98·8° F. On its rising again a further dose was given, the temperature falling on the morrow to 98·4° F. Further injections were given, the temperature never rising as high as before the injections. The temperature did not settle to a normal course before the tenth day, after which convalescence was well maintained. This case is of special interest on account of the failures of stock vaccine and the success of the autogenous. The former could not be expected to succeed, seeing that it was a streptococcal infection, and not a pneumococcal.

My cases have not been selected. I have treated all those I have had the opportunity of doing. Several have come into hospital late, and recovered before I could treat them. These, if I had chosen, would have improved the statistics had I inoculated with a falling temperature. I have described some of the worst, and I think I can claim to have done them good. If vaccine were calculated to do harm, one would hardly expect such excellent results when the prognosis is of the worst. I think, therefore, that with care and judgment pneumococcal vaccine can be used with the conviction that only good can result.

Mr. J. COURTENAY MACWATTERS: During the past three years I have applied the principles of therapeutic immunization to all cases in my practice that have seemed suitable to me, and am in every way satisfied with the results taken as a whole. I have carefully considered this line of treatment, and compared the results with those obtained by other methods, and, being a general practitioner, I think I may claim exceptional opportunities, as I have access to a large variety of cases for comparative observation. I have notes on some 300 cases, and in no instance have I had any reason to regret its application, and in the large majority I have obtained better results than by older methods.

In my hands vaccine therapy has yielded such uniformly good results that I have formed the opinion that, in cases of localized bacterial invasion treated by this method, cure is bound to result in almost every case, if one can succeed in three things — viz., the isolation of the offending organism, the preparation and administration in suitable doses of a vaccine made from it, and in promoting the flow of the resulting highly opsonized lymph through the infected tissues. Where failure has occurred, it has been my experience that the fault lay with the would-be immunizator, and not with the method.

Of the ordinary localized infections I need say nothing, nor need I weary you with a recital of successful cases of staphylococcic skin infections, catarrhal mucous membranes, &c. I would, however, direct your attention to the results I have obtained in a few cases of pyorrhœa alveolaris, pneumonia, rheumatism, and glycosuria.

Pyorrhœa alveolaris is of such frequent occurrence, and so far-reaching in its results, that it deserves far greater attention than it till recently has received; and now that vaccine therapy has afforded a means of successfully treating this hitherto intractable disease, its early recognition and treatment by bacterial vaccines are imperative. I have now treated over 50 cases, either sent to me by members of the dental profession or occurring in my own practice. I have considered as pyorrhœa those cases where there is an exudation of pus from recesses round the necks of the teeth, usually associated with the deposition of a greenish-brown tartar in a variable degree at the alveolar margins; the teeth as a rule are sound, but often loose from destruction of the alveoli. I have noted the presence of pyorrhœa from the age of 12 up to advanced years. In these cases I have considered as cured only those in which I failed to obtain pus on several occasions after the lapse of some weeks from the cessation of treatment. In addition to the obvious local symptoms, such as bleeding from the gums,

a bad taste in the mouth and foul breath, I have frequently noted morning vomiting, gastric ulcer, profound anæmia, constipation with irregular attacks of diarrhœa, vague rheumatic pains, susceptibility to sore throats and colds, irregularity of the heart, depression or even melancholia—all these symptoms disappear with treatment. I have frequently found that a patient suffering from an intractable dyspepsia, with anæmia and loss of weight—cases that have been a recurring testimony to the inefficacy of other treatment—have pyorrhœa as the underlying cause. Treat the pyorrhœa with vaccines, and the case clears up after years of futile treatment by older methods.

Bacteriology.—In all cases I have treated I have found a streptococcus in the pus, a streptococcus occurring singly and in chains of up to five or six members; other organisms present in order of frequency were: *Staphylococcus aureus*, *Micrococcus catarrhalis*, pneumococcus and leptothrix. In cases in which I have estimated the opsonic index, I have found a lowered resistance to this streptococcus, and this has risen to, or above, normal after vaccine treatment. It may be argued that streptococci are present in most normal mouths, and are frequently non-pathogenic; I believe that many so-called non-pathogenic organisms only require a local reduction in immunity to render them pathogenic, and the fact that improvement and cure result after the administration of these streptococci as vaccines is sufficient proof of their pathogenicity.

Additional Treatment.—It is of prime importance that the mouth should be rendered as clean as possible by the removal of hopelessly decayed teeth, and of accumulations of tartar, which afford beneath them a hiding place for the invading organisms, and, what is more important still, pen in the discharge of the affected alveoli, and prevent lymphagogue lotions from reaching the ulcerated surfaces. It is my practice to leave loose teeth which are otherwise sound, and with half or more of their roots still embedded in the alveoli, and almost invariably I have found that they become firmly fixed, as soon as the discharge is much reduced; and many teeth that seem at first sight beyond hope are thus saved. With a view of putting an end to any digestive disturbances as soon as possible, I usually advise the patient to disinfect the mouth thoroughly, immediately before each meal, by the free use of carbolic tooth-powder, but for local help I place greater faith in a mouth wash of sodium chloride 4 per cent. with sodium citrate 0·5 per cent. flavoured to taste; this by osmosis increases the flow of the highly opsonized lymph through the diseased tissues. Beyond this I have purposely avoided the use of local applications. Frequent

saline purgatives, I believe, are helpful in the early days of treatment, possibly by carrying off any swallowed pus. With regard to the application of vaccines, when possible I give the patient a vaccine prepared from his own streptococci, but I find that quite a large number of cases do well when given a stock vaccine prepared from half a dozen mixed strains obtained from similar cases. As a rule, I find a dose of 5 million is sufficient, repeated every eight days. In two instances, however, I have found this excessive, producing great aggravation of symptoms for some days, and therefore should the constitutional symptoms be very marked, I usually begin with a dose of 3 to 4 million; as soon as a dose is well borne, I endeavour to increase it to 10 million.

The duration of treatment I have found to vary greatly, and the duration does not seem to have any bearing on the apparent severity of the case, but depends rather on the immunizatory response at the patient's disposal. In one case cure resulted after four inoculations, but the majority require systematic treatment for six months or more. I invariably found improvement after a few inoculations, but the final clearing up and total absence of pus on repeated observations is always much longer deferred, and I now prefer to give fortnightly doses of vaccine for some months after the disappearance of pus. As to the permanency of the cure, it is too early for me to speak from my own cases, as I do not feel it is justifiable to claim a cure until a year or more has elapsed after the cessation of treatment; yet the results in about 75 per cent. of my cases have been so successful, and the improvement in constitutional conditions so marked, that I feel confident in advising the adoption of the method in all cases of pyorrhœa alveolaris.

I have frequently noted the presence of pyorrhœa in cases of melancholia and other mental aberrations, and have noted these symptoms disappear when the pyorrhœa improves while under treatment, so that I would suggest the possibility of pyorrhœa being at least a contributory factor in these cases, and will quote one case bearing on this point. The frequency with which this disease manifests itself in two or more members of a family, particularly in a husband and wife, suggests the possibility of infectivity; and the fact that several cases have been noted in which the tendency to sore throats and colds in the head, &c., have disappeared after treatment by vaccines is worth remembering. The following cases are typical of others:—

A thin, sallow-complexioned, haggard-looking man, aged 38, and a chauffeur by occupation, had for two years previous to treatment by vaccines been treated by myself and another doctor for progressive loss

of weight, and a dyspepsia associated with morning vomiting and a sinking pain in the epigastrium two or three hours after food, which was usually relieved by a meal. He had a persistent foul tongue and bad taste in the mouth, his gums bled easily, and pus was present in quantities round the lower front teeth. He frequently found blood and pus on his pillow in the morning, and suffered from colds in the head and sore throat, and was subject to vague rheumatic pains in the shoulders and arms. He had lost nearly 2 st. in weight, and could with difficulty follow his occupation, and consequently had become very depressed. Treatment on usual lines did him but little good, and he continued to lose weight. All this time he had cleansed his teeth regularly with carbolic tooth-powder. I carefully overhauled him on several occasions, but could find no trace of organic disease other than pyorrhœa. On September 4, 1909, his weight was 6 st. 10 lb. The pus obtained from the alveoli, after wiping away the first drop exuded, yielded a culture of streptococci. On September 7 his opsonic index to these organisms was 0·6, and I gave him five million of his own streptococci. This was followed by increase in the tenderness and in the discharge of pus from his gums for two days, after which improvement took place. A week later he expressed himself as being much improved in health and able to work, and enjoy it, and but little pus could be expressed from his gums. Three weeks from the commencement of treatment he had gained 4 lb. in weight, his opsonic index had risen to 0·9, and no pus was found in the alveoli. In eight weeks I had given him six doses of five million each; no pus could be obtained, his dyspepsia was a thing of the past, and he expressed himself as being quite well. After eleven weeks of treatment he had gained 10 lb. in weight, and at the expiration of fourteen weeks he had gained 21 lb. He had now received his eleventh inoculation, and was quite well. He had gained 1½ st. in weight, and his dyspepsia was quite cured. No pus was found, and his skin had regained a healthy colour. At the end of five months, after the cessation of treatment, he had maintained his weight and his freedom from dyspepsia, and no trace of pus could be found in his gums. He expressed surprise that during the past winter he had been free from colds and sore throats.

Another case: A thin, pale woman, aged 62, in November, 1909, was suffering from melancholia, with symptoms of religious mania for the previous four or five weeks; and this condition had become so serious that her family begged for her removal to the county asylum, where she had been on three occasions during the previous four years. She had

been detained for six to eight weeks each time. Her breath was most offensive, the fangs of her lower teeth were exposed to the extent of about half their length, and there was a large accumulation of greenish-brown tartar and pus round them. The disease was so obvious that I thought it might have some bearing on the causation of her condition, as it was associated with severe dyspepsia, pains in the abdomen, morning vomiting, and emaciation. Pus obtained from the alveoli yielded a copious growth of a short streptococcus, and I therefore gave her an initial dose of 5 million of a stock vaccine of mixed strains of streptococci, obtained from other cases of pyorrhœa. This was followed for two days by an aggravation of the local symptoms. She was instructed to thoroughly clean her teeth and mouth with a carbolic tooth-powder, followed by the use of a mouth wash of citrate of soda and salt, four times a day. I removed enormous accumulations of tartar from her molars as well as from her front teeth. At the end of a week she was decidedly better, and her friends were only too glad to let her remain at home. After four weekly inoculations she was almost free from depression, her dyspepsia had ceased, and she had gained ½ st. in weight, and but little pus could be expressed from the gums. After three months' treatment by weekly inoculations, she was fatter and in better health than I had known her during the previous six years; she was in good spirits and all her delusions had gone; she had long resumed her household duties, and was quite free from dyspepsia, expressing herself as feeling better and brighter than for many years. I continued inoculations at fortnightly intervals till April last, and I still find her improvement maintained.

To summarize, the cases I have treated number 48; of these, 4 discontinued treatment after one or two inoculations, 30 have completed treatment. These, I find, have received an average of ten inoculations each, 21 showed no return of pus two months after cessation of treatment; the remaining 9 have greatly improved.

With reference to *pneumonia*, my experience in treating cases of this disease is limited to 6 cases; in 3 of these the results were most encouraging. The cases were of ordinary lobar pneumonia, in which the pneumococcus was found in the sputum, or was obtained by puncture of the affected lung after the method advocated by Dr. Parry Morgan. In each case inoculation was begun with doses of a stock vaccine of pneumococcus obtained from other cases of lobar pneumonia, and in 2 it was noteworthy that no response occurred till the patient received a vaccine prepared from his own organism. Yet it would

appear that good results may follow from a stock vaccine in some cases. In 1 case, an alcoholic, aged 50, with a dilated heart and hyperpyrexia with delirium, death resulted on the seventh day, but this appeared to be a hopeless case from the first. It appeared to me that the other cases were much more comfortable during the course of the disease, the cough looser and less distressing, the expectoration was more copious and less tenacious; and, lastly, the pulse, respiration, and temperature were less disturbed than one sees in cases treated on the usual expectant lines. In 2 cases the temperature fell to normal by the fourth day; in 1 case lysis began on the third day, and the temperature was normal by the fifth day. The remaining cases appeared unaffected by the vaccine as regards duration of the disease, crisis occurring on the seventh day.

Rheumatism.—The following case of sub-acute rheumatism is of interest from the apparent bacterial cause of the disease, and from the ray of light that it may possibly throw on the pathogenesis of many cases of rheumatism: J. R., a labourer, aged 26, has several times in each of the past five years attended me for attacks of pain and swelling in the joints of his hands and feet, and occasionally in his knees. These attacks have lasted from two to four weeks, and of late have been more severe. In October of 1909 a more severe attack than usual led me to admit him to hospital. His condition was then such that he could not undress himself, the joints of his hands, wrists, and fingers being swollen and painful; his right knee, both ankles and feet were similarly involved; his temperature was between 99° F. and 100° F., and he appeared to be a typical case of sub-acute rheumatism. After treatment for eight days by salicylates and a milk diet he had improved, and so long as he was kept under the influence of salicylates—20 gr. every two hours—he was without pain, and most of the swelling disappeared. On reducing the salicylate to half the quantity the pains and swelling reappeared, relief being again afforded by return to the larger doses. This sequence of events occurred four times. Iodides, arsenic, alkalies, and milk diet, all failed to maintain definite improvement, or to procure results as good as the salicylates, which were then continued till December 20, and after eight weeks in hospital he was but little better than when he was admitted. During this time I had repeatedly examined his urine bacteriologically, and invariably found Friedländer's bacillus present. I also found that he had a reduced opsonic index to this organism. On December 20 I withheld all medicines, and his joints became more troublesome by the next day. I then gave him 10 million of his own

bacilli as a vaccine. This was followed by improvement, the patient expressing himself as feeling better than he had felt for months, and his joint troubles remained in abeyance for five days, in spite of the fact that salicylates had been withheld. On the sixth day I repeated his inoculations, as there was some return of pain and swelling, and this was again followed by improvement. After the expiration of another week a dose of 20 million was administered; this was followed by a slight exacerbation of symptoms lasting three days, after which improvement took place, and a few days later the patient was discharged. He was instructed to report himself in a fortnight, when he received a dose of 15 million; after this he returned to work and remained away for four weeks, then he presented himself with a slight return of symptoms, which yielded to a further dose of 15 million. Since February he has followed his work regularly, and has been more free from rheumatism than for many months. Considering how frequently reduction in the dose of salicylate was followed by a return of swelling and pain, and how any other treatment had failed to give relief, I think one is justified in crediting the vaccine with producing the amelioration of the patient's symptoms and ultimate cure, and in considering that the rheumatism was an exhibition of a toxæmia produced by the presence of the bacillus of Friedländer. In connexion with this I would direct your attention to the frequency of so-called rheumatic pains in cases of influenza, tonsillitis, pyorrhœa alveolaris, &c. I can recall a case of what I considered to be muscular rheumatism, which yielded *Bacillus pyocyaneus* from the urine; another, of rheumatoid arthritis, which yielded a streptococcus from the urine, sputum, and fæces; and both of these cases were markedly relieved by the corresponding vaccine.

I therefore contend that many of the cases of so-called rheumatism may be simply an expression of a toxæmia due to bacterial invasion of some remote tissue, by one of a variety of organisms, not necessarily the streptococcus described by Poynton and Payne, Beattie, and others, as the specific cause of acute rheumatism. I do not deny that in the case of typical acute polyarthritic rheumatism it is probable that there may be some specific organism to which the disease is due.

Glycosuria.—Inasmuch as it is well known that cases of glycosuria are liable to invasion by staphylococci, as evidenced by manifestation of carbuncles, boils, eczema, &c., it has occurred to me that the glycosuria may be the result of, and not the cause of, the lowered resistance to this organism, and with this point in view I would draw your attention to the following cases which I have had under observation :—

A large well-nourished man, aged 56, had had attacks of carbuncles and boils for the past three or four years. These ran a protracted course till 1908, when he came to me to be treated by vaccines for a carbuncle. It was at this time sugar was first noticed in his urine, and was present on every observation up to August, 1909, in spite of codeia and careful dieting. On August 2, 1909, he received a wasp sting on the forearm, which was followed by invasion of *Staphylococcus aureus*, which in two days caused an extensive cellulitis of the arm and a temperature of 103° F., although the focus of invasion was extremely minute. His urine contained 6 per cent. of sugar, and his staphylo-opsonic index was 0·6. I accordingly gave him 200 million of an auto-staphylococcic vaccine. Three days later the arm was much better, his urine contained 2 per cent. of sugar, his staphylo-opsonic index was 0·91, and I gave him 200 million of his vaccine. At the end of a week from his first dose of vaccine his urine was free from sugar and his index had reached 1·1. A week later his index had fallen to 0·82, and a trace of sugar was found. I therefore gave him another 200 million of his vaccine, and ten days later his index was 1·2 and sugar was absent from his urine. Three weeks from the commencement of treatment the patient was feeling much better, no sugar was present in his urine, and he was allowed ordinary diet, including sugars and beer. At eight weeks no sugar was present, and his index 1·2; at twelve weeks his index had dropped to 0·87, and sugar was present in his urine; he therefore received a dose of 200 million, which raised his index to 1·1 and freed his urine from sugar in two days. From November to March he kept in excellent health, and once in every two or three weeks received a dose of his vaccine, his urine all this time showing no return of sugar on periodic examination, and a normal opsonic index to staphylococcus was maintained. He then was away for some weeks, and I saw nothing of him till May this year, when I obtained a sample of his blood; this, I found, showed an index of 0·86, and on examining his urine found sugar present to the extent of 3 per cent. I gave him 250 million of his vaccine, and a week later the sugar was 1 per cent., and thirst and increase of urine that had been present had almost disappeared. Since this he has had one more dose of vaccine, and on my last examination of his urine a few days ago sugar had decreased to 0·5 per cent. In this case it is noteworthy that sugar had been present on every examination for over a year previous to systematic treatment by vaccines, in spite of administration of codeia and a restricted diet. Yet from November, 1909, till May, 1910, his urine was free from sugar, although he was

taking ordinary diet, and receiving no other treatment than vaccine, also on many occasions the appearance of sugar in his urine was synchronous with the falling of his staphylo-opsonic index.

Case II.—The patient, a builder by trade, was an emaciated man, aged 60, who three years ago suffered with boils; he appeared acutely ill and had a haggard and cyanotic appearance. Two years later he had a large carbuncle on his neck, and for a year previous to his commencing treatment he was known by my partner to be passing sugar in his urine. Three years ago his weight was 12 st., and in February last, when we started treating him by vaccines, his weight was only 9 st. 3 lb. He complained of great thirst, to relieve which he was in the habit of taking a quart jug of water to his room every night, and this he finished by the morning. He had to get out of bed six or seven times each night to pass urine, voiding every night one or sometimes more chambers full; he complained of absolute loss of appetite, and of a most disagreeable sweet taste in the mouth. The man had at this time ceased to work, as he felt too weak to continue. His urine contained 9 per cent. of sugar, and was of a specific gravity of 1042. Treatment: On February 3 he was given 300 million of an autogenous vaccine of staphylococcus; this was followed by marked improvement, the amount of sugar excreted was diminished, as was also the quantity of urine voided, and all the above-mentioned symptoms were distinctly relieved. A week later he was given 500 million of the vaccine, and this was followed by a marked negative phase, and during the three days that this lasted there was an augmentation in the amount of both total sugar and of urine excreted, and, of course, a temporary return of thirst, &c. By the expiration of a week, however, he improved and was given a smaller dose, which was followed by marked improvement. I need scarcely say that he has received no other treatment while being treated by vaccines, and has not had any restriction placed on his diet. Now, after three months, his urine contains a mere trace of sugar, he does not have to get out of bed at night, the sweet taste and disgust of food have gone, and he eats well and expresses himself as better than for many months, and has gained 2 st. in weight, having, of course, lost the haggard, acutely ill, and cyanotic look that was noted at the commencement of treatment. Although this patient was a man of advanced years, yet he was not one of the obese type that resist the disease so well and are prone to periods of marked improvement; on the contrary, at the outset of treatment he appeared to be running down-hill rapidly, and he at least thought his days were numbered.

Case III.—My third case is not so satisfactory, as far as final results go, yet there are points of definite interest in the case. The patient was aged 38 when sugar was first discovered in his urine, and it was three years later that he came under my observation, and, as he seemed absolutely doomed, I was particularly anxious to apply this line of treatment. He was exceedingly emaciated, and smelt strongly of acetone; he had marked peripheral neuritis; his vision was $\frac{1}{60}$, owing to diabetic cataract; one of his great toes had become gangrenous after a burn. He was passing over 100 oz. of urine, and his daily output of sugar was over $\frac{1}{2}$ lb. He suffered from intolerable thirst and prostration after the least exertion, which, added to this train of severe symptoms, rendered it more than probable that his drowsy condition was the forerunner of coma. The administration of 250 million of a vaccine prepared from a staphylococcus isolated from his gangrenous toe was followed by a reduction in the daily output of sugar to one-half, and the volume of urine by one-third within three days, and after five days a similar dose was followed by a further reduction of the sugar to $3\frac{1}{2}$ oz. and the volume of urine to 50; up to this time the patient had gained 5 lb. in weight. Subsequently a dose of 500 million was followed by a great increase in the output of both sugar and urine, these reaching 11 oz. and 144 oz. respectively; this increase was followed by a corresponding fall, and after four days the quantities had fallen to 38 oz. of urine, with 2 oz. of sugar. A fluctuating improvement followed for a week, after which the patient became more drowsy, the output of sugar increased, and he lapsed into persistent coma, which terminated the case.

Let it be understood that, in reporting these results in glycosuria, I do not claim to have established an incontrovertible theory as to the pathogenesis of the disease in question. I trust, however, to have provided food for thought, and a suggestion for further investigation in this direction, in the hope that a method of successful treatment may be established.

Speaking from the point of view of the general practitioner, I would strongly urge all those who have sufficient patience and perseverance, combined with a desire to do the best possible for their patients, to render themselves not only *au fait* with the principles underlying the application of vaccine therapy, but to devote sufficient time in a good *clinical* bacteriological laboratory to enable them to carry out the necessary manipulations in preparing vaccines. To those whose chief aim in life is the reduction of personal effort to a minimum, and who will grudge the many hours spent and the inevitable disappointing failures in

laboratory technique during their apprenticeship, I would say leave vaccines alone, for in my experience, as I have already stated, when failure results it is far more frequently due to fault on the part of the would-be immunizator than on the part of the method.

In bringing these remarks to a close, I would like to take this opportunity of recording my great indebtedness to Sir Almroth Wright and his staff at St. Mary's Hospital Inoculation Department for the great kindness they have shown me during the all too short a time I have been able to spend there.

Dr. F. Ashton Warner: What has struck me at these meetings more than anything else has been the regular chorus of consent as to the value of vaccine therapy, and in that I beg to join. My experience of the use of vaccines is purely clinical, and I am dependent on the trained bacteriologist for the estimation of the opsonic index, culture of the particular micro-organism in each case, and the supply of the necessary vaccine. I find it is practically out of the question to make use of the opsonic index as a matter of routine owing to the necessary expense which the work entails and the disinclination of patients to pay the fees, so that in the majority of cases I have to depend on the clinical signs as a guide to the amount and frequency of the doses. But in dealing with tuberculin in cases of infected cervical glands, I make it a rule to rely on the opsonic index as a guide, and not on the conditions of the glands as judged by appearance and palpation. For it is impossible to judge of the condition of the deeper glands in the chest, and consequently the superficial ones do not afford a reliable guide.

Now, as to the administration of the various vaccines, I have made use of them by the mouth and hypodermically, and, so far as my experience goes, most satisfactory results are to be obtained by either method. Patients certainly prefer oral administration to the hypodermic, and especially children, so that in their case, at any rate, it is of great advantage to be able to dose them by the mouth. Another advantage is that in cases of acne the dose of vaccine may be taken at intervals with only occasional visits to the medical man, the dose being regulated in accordance with the local reaction, which can be noted by the patient—a distinct advantage to the patient when the treatment is lengthy. The simplest way to give vaccines by the mouth is to direct the dose to be taken in a sherry wineglassful of milk, the first thing in the morning, on an empty stomach. The milk must be first boiled.

The following short notes are of cases treated with the vaccine, and have not been selected, but taken in the order in which they were dealt with :—

(1) W. H., a young officer in the Army, contracted a gonorrhœa in July, 1907, which was followed by arthritis of the left knee, right hip-joint, and, in a lesser degree, the left ankle. He was confined to his bed for nine weeks, and was then sent to Bath, where he went through a regular course of baths and was put on a very restricted diet. He was seen by me for the first time in January, 1908, and his condition was pitiable. He was depressed, pale, wasted, and hobbling about the house with the aid of a stick. The left knee-joint was swollen, painful, and movement was restricted. He also complained of pain in the right hip-joint. I first put the patient to bed and gave him a dose of stock gonococcal vaccine hypodermically. On my visit to him the next day he complained of pain in the knee and the right wrist and a slight headache. There was no apparent change in the knee, nor could I detect anything amiss with the wrist. The second dose was given nine days after the first, and in the course of the treatment, which extended over four weeks, he had five injections altogether. By the end of six weeks from the commencement of the treatment he was able to drive his motor-car and was fit to join his regiment. No medicine was given, but the patient was put on a very full diet.

(2) W. B., a fine, healthy lad, suffered from eczema of the right ear, from which there was a copious discharge of serum and pus. This condition had lasted off and on for several months, and was a source of worry and annoyance to the patient as well as to me. No treatment that I adopted had any but the most temporary effect. A swab of the discharge was sent for examination, and gave a culture of the *Bacillus coli communis*. A special vaccine was prepared, and two doses given by the mouth at an interval of a week. After the first dose my patient told me that the irritation was decidedly less, but the amount of discharge appeared unaltered. After the second dose, towards the end of the third week, the irritation had ceased, and the discharge was very much less than it had been all along. By the end of the fourth week there was no discharge, and the ear, though slightly red, caused no discomfort. I have seen the patient frequently since, and there has been no recurrence of the eczema. But for a plug of sterilized cotton wool in the ear to prevent the discharge trickling over the lobe, there was no local treatment after the first dose of vaccine.

(3) These two cases were patients, both young ladies, who had suffered from acne for years. Miss C.: Severe facial acne, which debarred her from going into society, and necessitated her wearing a thick veil to hide the disfigurement. A special vaccine made from the contents of a pustule was prepared, and the doses given by the mouth. For the first three or four months there was marked improvement, but after this progress was very slow, until a mixed vaccine of staphylococci and acne bacilli was given; then almost immediate improvement began, and at the end of ten months there was not a pustule to be seen. Miss L.: Acne of the back, which prevented her wearing a low-neck dress. In this case, by way of contrast, I used *stock* vaccine. The doses were given by the mouth, and at the end of eight months' treatment the result was as satisfactory as in the case above. In both the vaccines were given by the mouth, and I saw the patients at intervals of three weeks. The interval between most of the doses at the commencement was a week.

(4) Miss S., aged 18, complained of great pain in her throat and both ears, and she was particularly careful to impress upon me that the throat was not sore, but painful. The pharynx, soft palate, and fauces were intensely injected and of a bluish-red colour—very like a piece of underdone beefsteak. The glands at the angles of the jaw were enlarged and tender. The tonsils were inflamed, but there was no follicular secretion. Temperature varied between 101° F. and 102° F. There was intense headache, no cough, and nothing to note in the chest. Local treatment gave no relief, and the pain was severe enough to necessitate an opiate on three nights. A swab of the throat was taken from which "an almost pure culture of pneumococci" was obtained. After the first dose of specially-prepared vaccine, given hypodermically, there was marked relief. The vaccine was given in the early afternoon, and the patient was able to obtain sleep the same night without the aid of drugs. A second dose was given four days after the first, and the temperature, which was above 100° F., fell and remained below normal, and all the throat symptoms improved, pain ceased, and the patient gradually got well. She looked much more ill than was to be expected from the local conditions, and assured me more than once that she felt very ill. This case of painful throat was one of many that came under my observation about the same time, but was by far the most severe, which was my reason for having a bacteriological examination made.

(5) A. M., aged 55, sent for me one evening, and I found him suffering from an attack of acute cystitis, passing small quantities of bloody urine, with a temperature of 103° F. to 104° F. The acute stage

passed over, and then pus appeared in the urine, which persisted for three weeks with no improvement. The patient during this time was much bothered with frequent micturition, which interfered with his rest at night. Treatment had very little effect on the amount of pus, which measured about 1 in. at the bottom of the specimen glass. A catheter specimen was examined for me and the *Bacillus coli communis* found in quantities. Treatment was commenced with a specially-prepared vaccine, and, after the first dose, given hypodermically, the patient expressed himself as feeling much better, and there was less frequent micturition. The amount of pus and mucus rapidly diminished, the urine cleared up, and at the end of a month the patient began long bicycle rides. The first dose of vaccine was given on July 19, and the last on August 21, 1909; and the patient, whom I saw at the beginning of this month (June, 1910), remains perfectly well. The rapid improvement and complete recovery were in marked contrast with cases of cystitis before the vaccine treatment was introduced, when washing out the bladder night and morning with solutions of boroglyceride was the routine practice.

(6) Miss L., aged 47, was advised by me to have her appendix removed, as she had suffered from several attacks of appendicitis, none of them severe. She was of a nasty clay colour, languid, and complained of frequent attacks of headache, which she did not connect with the attack of appendicitis. A well-known surgeon operated and had no difficulty in amputating the appendix, which was found inflamed with some old, and more recent adhesions. The wound broke down, and there was a discharge of most foul-smelling pus. A stock vaccine was used until one prepared from the patient's own pus could be obtained; but, notwithstanding this, the patient went from bad to worse, and died of general septicæmia in the third week. The use of the vaccine had absolutely no effect on the temperature, the discharges, or the general condition of the patient. I think in this case the time to use the vaccine would have been before the operation, and not after. The patient was evidently, I think, suffering from a toxæmia long before the date of operation, which would explain the headaches, the colour of the skin, and the depression.

(7) Mrs. T., a lady whose child had been seriously ill with pneumonia and empyema following upon influenza, also suffered from a comparatively mild influenzal attack. She recovered up to a certain point, but for weeks subsequently complained of headaches, and the fact that her temperature was never below 100° F. Treatment with drugs

and change to the seaside had little or no effect, and I decided to see what effect the use of a vaccine would have. Captain Douglas was kind enough to supply me with the necessary vaccine, and I gave it hypodermically. After the third dose, at the end of two weeks, the temperature fell below normal and never rose again; the headaches ceased, and the patient assured me she felt better than she had done for weeks past.

From the comparatively small experience gained in the course of treatment of the above and other cases, my feeling is that—

(1) Brilliant results are to be obtained from the timely use of vaccines.

(2) They may with advantage be given by the mouth.

(3) Whenever possible, the opsonic index is to be used, along with the temperature and clinical conditions, as a guide to dosage.

Royal Society of Medicine

June 22, 1910.

Sir WILLIAM S. CHURCH, K.C.B., President, in the Chair.

Discussion on Vaccine Therapy: its Treatment, Value, and Limitations.[1]

Dr. W. D'ESTE EMERY: The discussion which is now drawing to a close has been of a somewhat desultory nature, touching on many subjects, and amongst others on one of extreme importance at the present day—namely, the relation which exists and that which should exist between the clinician and the pathologist. That this is not at present all that it should be is, I think, apparent to all, and I have no doubt in my own mind what the solution of the problem will be in the future. Sir Almroth Wright, in his opening speech, said the time would come when every physician would be his own bacteriologist. I agree with this most heartily. It is an ideal which I have always used my best endeavours to bring about. But it does not follow that every bacteriologist will be a physician. There will always be those who have studied the subject more deeply and have spent more time in acquiring technique than the physician in the active practice of his profession can afford to do. Their duties will be to carry out the more delicate and laborious researches, such as exist now, and such as will become more and more important in the future. But when the all-important subjects of clinical pathology and bacteriology have taken their proper place in the medical curriculum, when as much time is devoted to them as to subjects of not greater importance, such as anatomy or physiology, and when the student is encouraged to study what takes place in the body in disease as well as in health, then

[1] Sixth meeting (adjourned from June 15).

I think the difficulty will right itself, and the pathologist, called in to make a difficult research in a difficult case by a physician who is himself a competent pathologist up to a certain point, will be regarded not as a drudge but as a colleague. Signs are not wanting that this process is commencing, and the clinician of high attainments in pathology is already with us. But there is a real danger that the pathologist may be devoid of clinical experience. This I regard with profound apprehension, and should forbid any one to specialize as a pathologist until he had had a good clinical training. Without this the clinical pathologist is as great a danger as the physician without a practical knowledge of pathology and bacteriology in their modern developments.

In discussing the more immediate object of these debates I shall not attempt to produce lists of cases, but to record my general impressions of the results obtained. I have had both successes and failures in many branches of vaccine treatment, but I think it only fair to say that many of the failures and some of the successes have been in patients in whom ordinary clinical methods had been given a full trial and had also failed. A few marked successes of this kind—that is, rapid and complete cures in cases in which no good at all can be effected by the means formerly employed—make a deep impression, and it was one such case that convinced me of the value of vaccine treatment. It was a very severe case of gonorrhœal arthritis of more than two years' duration, which had had most thorough and careful treatment both in England and abroad, but without benefit. When I saw him both knees, both ankles, one shoulder and many small joints of the hand were affected. He made a complete cure under vaccine treatment in less than three months, and the improvement was most obvious within a week of the first dose.

To summarize the results I have seen and obtained, in *staphylococcic lesions* of the skin the method is most successful, and I suppose no one would attempt nowadays to treat such cases without vaccines. In general, boils are more amenable to treatment than is acne, but in the latter case the addition of vaccines of the acne bacillus leads to much better results, though of this I have had but little experience. Next to staphylococcic lesions I should place infections of the urinary tract by *Bacillus coli*. Here, as a rule, the effects are marked and immediate, only a small proportion of cases failing to show improvement within a few days. I must, however, admit that if the bacteriological test of cure be adopted, in most cases the cure is incomplete. All symptoms are

removed, and from a clinician's point of view the patient is well: but as a rule some bacilli, though in comparatively small numbers, persist in the urine. *Gonococcic* infections, such as arthritis and iritis, are uniformly benefited, and are often completely cured in a rapid and striking manner. I have had little experience of the treatment of urethritis, whether acute or chronic. *Pneumococcic lesions* are sometimes cured quickly and completely, whereas at other times they are most resistant. As an example of the former I may quote a case of suppuration of the frontal sinus of some years' duration, twice operated on (to secure drainage) and showing not the slightest tendency to spontaneous cure. The discharge ceased after two injections; the patient received three more, and there has been no recurrence of the disease after three years. I have also had a very striking case of cure of a case of frontal sinusitis due to Friedländer's bacillus. I must also mention a very interesting case of failure after the use of pneumococcic vaccines. A patient under the care of Dr. Murray Leslie had suffered from a chronic pneumococcic abscess of the lung for more than two years. I gave him four courses of vaccine treatment during the course of the next two years, and left him neither better nor worse. I mention this case because the patient gave the method a most careful trial, and every possible form of modification of the treatment was tried. He had the most careful opsonic control, and small, large, and colossal doses were tried at short and long intervals. In all probability the abscess was shut in by a thick wall of fibrous tissue, and this constituted the great obstacle to the cure. With regard to *tubercle*, my experience (which has mostly been in cases of surgical tubercle, since in them the value of the treatment is more easily watched than in phthisis) is this: the use of tuberculin on Wright's principles is of some value, but it rarely if ever effects a cure of large lesions—that is, those in which active tubercles are protected from the blood by masses of fibrous tissue or by caseous or necrotic material. In such cases there is often a decided benefit, especially to the general health; but the direct effect on the lesion is usually inappreciable or at most very slight. In cases where there is a thin layer of tubercles in contact with healthy vascular tissues much better results can be obtained. Thus I shall describe in fuller detail subsequently two cases of tubercles of the iris quickly and completely cured by tuberculin. I have also seen a few chronic sinuses heal rapidly, and one large and severe tuberculous ulcer get better up to a certain point in a very extraordinary manner. It then hung fire, and, though a complete and lasting cure was obtained ultimately, it took several months.

I am perfectly aware that many vaccinists claim much better results than I am able to bring forward. I wish therefore to state that most of the results quoted were obtained whilst I made use of the opsonic control, and that since I have abandoned the method my results have been neither better nor worse. In concluding this brief account of success and failure I ought to add that, even allowing for all the disappointments, vaccine therapy has established itself as a permanent addition of great value to our methods of cure, and its discovery is a triumph for English medicine and for Sir Almroth Wright.

I pass on now to discuss a question apparently highly theoretical, but really of great practical importance—that is, the meaning of the opsonic index, and the importance to be attached to it as a guide to the exhibition of vaccines. I may say at once that I disagree absolutely with those who hold that it is impossible to estimate the opsonic index with accuracy. I have never studied the question in its mathematical aspects, but have made numerous practical tests, such, for instance, as working out the indices of a series of sera collected by someone else, and containing duplicates; I have found the figures for these duplicate specimens always corresponded very closely. I have also prepared various dilutions of the same sera, and prepared curves indicating the indices, which, from their regularity and constancy in different cases, afford in themselves a proof of the accuracy of the method. Lastly, the curves of the indices obtainable after inoculation of a vaccine are also a sufficient demonstration of the same fact. Given careful technique, it is quite easy to show the absolute accuracy of Sir Almroth Wright's account of the series of changes following the injection of a vaccine into an infected patient. I might, however, be allowed to state that the method is laborious in the extreme, and that if correct results are required, the most careful attention to every detail is necessary, any attempt to cut short the process rendering the results absolutely useless.

We are not dealing here with the diagnostic value of the opsonic index. I may just remark, however, that I am quite in agreement on this point with the opsonic school, more especially in regard to the value of determinations of the index before and after an injection of a vaccine of the organism with which the patient is supposed to be injected. I believe this method to be one of great value; the only objection to it being the technical difficulty arising from the necessity of making several determinations, each demanding the greatest accuracy.

When, however, we turn to the meaning of the opsonic index in its bearing on the question of immunity and the exhibition of vaccines, we

encounter great difficulties. The point that I wish to discuss more especially is this: Is the opsonic index to be regarded as being in any sense an indication of the patient's resisting power against the disease in question? That it is such an index is the assumption, as Sir Almroth Wright has told us in his opening address, that underlies the use of the opsonic index as an indicator for the dosage and spacing of vaccines. The arguments in favour of this view are: *First*, that, the activities of the leucocytes being equal in the two cases, the blood of a patient having a high opsonic index will deal more easily with an infection than will the blood of a patient in whom the index is low. This is, of course, both true and self-evident; but its value in treatment depends on a demonstration of the fact that the phagocytic value of the blood of the patient with the low index is too feeble to protect him against the disease under discussion. This, however, is not self-evident, nor do I believe it to be true. I may quote one case: a patient under Dr. Whitfield, suffering from a slight tuberculous lesion of the skin. His opsonic index was determined on many occasions, both by Dr. Whitfield and myself, and always found to be low. Sometimes it was extremely low, less than 0·1, but it was usually about 0·3 or 0·4; only on one occasion did it reach 1, and then as the result of tuberculin treatment. Yet this patient showed no evidence whatever of increased susceptibility to tubercle; his disease underwent gradual cure, and he did not develop any fresh lesions, although, like the rest of us, he must have been frequently exposed to infection during the eighteen months he was under observation. *Secondly*, the evidence that a raised opsonic index implies increased immunity depends on the fact that clinical improvement is often coincident with the elevation of the index, as brought about by an injection of a vaccine. This, of course, is also admitted, but the elevation of the index is only one of a complex series of phenomena which follow the injection. Nor is it always the case: Allen has noted that after the use of gonococcic vaccine continued improvement may accompany a prolonged negative phase, and I can corroborate the fact. I have also seen it in tuberculosis, and in one of the few cases of this disease in which I have noticed a very marked and rapid beneficial effect from tuberculin the improvement took place almost entirely during a negative phase lasting three weeks.

The evidence in favour of the opposite view may be stated as follows: *First*, a high index is not necessarily accompanied by a recovery or by a mild form of the disease. Every opsonist must have met with cases of lupus with a very high index, which nevertheless showed no sign of

improvement. I had under my care a case of lupus of the face of fifty-six years' duration associated with an index of nearly 2; it was practically stationary, and certainly showed no tendency to heal. The result of vaccine treatment was to lower the index, and there was some slight improvement in the lesions. It is the same with acute diseases. As a rule, undoubtedly, a severe attack is accompanied by a low index; but even here exceptions may be found, as, for instance, in generalized tubercle or tuberculous meningitis, in which normal or high indices are not infrequent, and the preagonal rise in other infections is well known. Sir Almroth Wright has pointed out and emphasized the fact that, admitting the rise in the opsonic index to be an indication of the immunizing response, we must not expect it to be necessarily coincident with clinical improvement. Many other factors, and notably the free access of the blood to the lesion, have to be taken into account. I have not lost sight of this, but it appears to me not to account for the development of fresh lesions in patients with a high index; for example, patients suffering from furunculosis have frequently high indices at the time fresh crops of boils are forming. Here it is difficult to see how the staphylococci can be in such a position that the blood and leucocytes have not easy access to them, and other examples might be quoted. *Secondly*, as I have pointed out already, a low index does not necessarily imply that the disease will make rapid progress. A low index may accompany a severe, a mild but progressive, or a healing lesion. We are led to believe, therefore, that the defensive powers present in blood of feeble opsonic power is adequate to the destruction of the bacteria in the body, and that, although the raising of the index may be, and probably is, an increased safeguard (especially, perhaps, against hæmic infections), this is not the mechanism by which vaccines produce their effects.

This mechanism I believe to be threefold. First, and of greatest importance, there is the local reaction; secondly, a general alterative or metabolic effect; and, thirdly, an immunizing effect. By the term "local reaction" we mean the series of changes which take place in the immediate vicinity of the lesion, and which we may describe briefly as an aseptic inflammatory process. Given a suitable dose of the vaccine, these changes are practically limited to hyperæmia of the vessels immediately surrounding the lesion, with probably a slightly increased exudation of fresh plasma and diapedesis of functionally active leucocytes. This sequence of events, of course, constitutes (in the case of tubercle) Koch's phenomenon in its slightest form. Given larger doses, more acute inflammatory processes, accompanied by obvious swelling, by stasis in the

vessels, and perhaps by necrobiosis of the lesions, will be brought about, as well as a general reaction in the form of sudden rise in temperature. These severe reactions are what we wish to avoid, and we may do so by one of two methods: we may give very small doses at long intervals, so that the immunizing effect is little or absent; or we may give larger and increasing doses at short intervals, so that the patient is immunized to tuberculin, and the effect of each dose is diminished by that which has gone before. The so-called opsonic method I believe to act in the first manner almost entirely; the doses given are those which cause a slight and beneficent local reaction, but which do not exert any appreciable immunizing influence, so that by the time the second injection is given the patient has returned to his original condition of susceptibility. In most cases these reactions are so slight as to be inappreciable clinically; they cause no rise of temperature, and are unassociated with obvious swelling of the affected region. In some cases, however, this may be seen, as, for example, in tubercle of the iris. This disease is, of course, especially favourable for observation, the actual tubercles being visible throughout the course of treatment. I have treated two cases, each being an absolute cure within two months. The doses given were $\frac{1}{5000}$ mg. of T.R., at intervals of ten to fourteen days. In each case the injections were followed by the production of a narrow, pink zone round the lesions, which developed within twenty-four hours, and lasted two or three days, and it was obvious that when the reaction had passed off that the lesions had diminished in size. I believe Mr. Mayou has made similar observations. I regret that I did not make systematic estimations of the opsonic index in these cases, and am therefore unable to say whether the reaction coincided with any of the events following the injections. Another effect of the local reaction may sometimes be seen in cases of slight phthisis, in which there is ordinarily no cough or sputum. An injection of a minute dose of tuberculin may be followed with great regularity by the expectoration of a small amount of muco-purulent sputum.

The points which I wish to emphasize in connexion with these cases of tubercle of the iris are: (1) That the reaction took place after a very small dose of T.R. (I am unable to say whether a smaller dose would have had any effect, but the fact that $\frac{1}{5000}$ mg. was definitely efficacious appears to be worthy of notice). (2) That exactly the same sequence of events followed the second injection, given ten days later, as was produced by the first; in other words, if any immunity was produced it did not last for this length of time. (3) That the reaction was mild in

the extreme, and not associated with any obvious necrobiosis, or breaking up of the nidus or dissemination of the bacilli. I mention this because in one of Sir Almroth Wright's papers, in which he discussed this local reaction in the case of staphylococci and tubercle, he remarks that the inflammatory reaction leads to a breaking-up of the nidus in which the bacteria are lodged, and a "fluttering" of the bacteria themselves. The spread and dissemination of the disease which followed large doses of tuberculin he attributed to a diminution of the patient's power of resisting the organisms thus let loose; and leads us to infer that this unfortunate occurrence might have been avoided had the patient's resisting power, as measured by the opsonic index, been raised. But the reaction after doses like this is extremely slight, and unattended by any rise of temperature, or in ordinary cases by any obvious tumefaction of the lesion. Either of these phenomena I regard as proof of an overdose, and I believe the optimum dose of tuberculin to be the largest that can be given without the occurrence of either.

How the local reaction is caused is not yet known, and this is not the occasion on which to study the numerous theories as to its causation which have been advanced. It is, however, more profitable to consider for a moment the manner in which a frequently occurring reaction may result in the cure of a lesion. The usual suggestion is that the cure is brought about in virtue of the flooding of the diseased tissue with lymph rich in bacteriotropic substances, and in particular in opsonins. But the evidence in favour of the view that a high opsonic index does facilitate the cure is not strong, and rests mainly on some observations by Dr. Bulloch on patients suffering from lupus, and treated with the Finsen light. Now this method of treatment causes a local reaction similar in all respects, as far as we can tell, to that caused by inoculation with vaccine; and Dr. Bulloch found that as a general rule those patients did best in whom there was a high index. We find, however, on looking at his tables that, although his deductions were perfectly correct, there were, as a matter of fact, numerous exceptions: thus the patient with the lowest index (0·25) of the whole series is recorded as doing well under light treatment. Here, therefore, this patient's blood was, if we accept this theory of the action, sufficient to kill the organisms if brought into contact with them by means of a reaction induced by the Finsen light. I wish to make it clear that I am not denying the value of the flushing of the tissues with healthy blood or lymph. I am trying to show that even with a low, opsonic index there is a sufficiency of defensive substances in the blood. In this connexion please notice: (1) That the

number of bacilli in the lupus lesion is very small in comparison with the number of tubercle bacilli that are taken up in an opsonic test, even with a serum of low index; (2) that the time which the plasma and leucocytes may act in the body is indefinite, in place of the fifteen minutes of an opsonin experiment; and (3) that if we accept certain recent researches there is a constant shower of bacilli from the local lesions in phthisis, yet without the development of distant tubercles, showing that, contrary to former ideas, the blood must have a very considerable bactericidal action on the tubercle bacillus. May I suggest that it is probable that apart from, and perhaps more important than, the bactericidal action of the blood thus induced to stream through the lesion, there is also a stimulating action on the tissue cells of the lesion, so that the same substance which, when present in excess, causes necrobiosis, causes, when present in extremely minute amount, an increased vitality and increased resisting power in the cells in the neighbourhood of the bacteria? I believe that this is the primary effect in a reaction, and that the hyperæmia is a consecutive effect due to the heightened functional activity of the cells in the lesion or in its immediate vicinity.

The second effect, which I have termed the "alterative action," does not always occur, but is occasionally very well marked. It consists in a general improvement in nutrition, often accompanied by a marked sense of well-being and stimulation of all the bodily faculties. It is not entirely an effect of suggestion, for it may be seen in very young children. This improvement in the general health may take place when there is no obvious improvement in the local lesion, especially in some cases of pyorrhœa alveolaris, in which a decided improvement in nutrition and the general health has occurred without being accompanied by any apparent local benefit. Of course, the general tonic effect may, and probably will, have a beneficial effect on the lesion if continued long enough. Another result frequently seen during a course of vaccine treatment, which I include under this heading, is diminution of anæmia, should this be present. Thus I have on two or three occasions seen marked temporary improvement in the anæmia of malignant cachexia treated with neoformans vaccine, an improvement which I need hardly say I do not attribute to any direct curative effect on the malignant growth. In one case of severe frontal sinusitis, due to the pneumococcus, the percentage of hæmoglobin when I started vaccine treatment was 20, and it rose to 40 in three weeks. This was practically at the rate of 1 per cent. per diem, which is about the rate at which

hæmoglobin is regenerated in a healthy person after a severe hæmorrhage. In this case there was decided local benefit, but hardly sufficient to account for the dramatic improvement in the patient's general condition. I have some reason for thinking that this tonic or alterative effect may be non-specific, and may be induced by vaccines other than that of the organism with which the patient is infected, but I do not regard this as certain. This alterative effect has been carefully studied by Campbell Bruce,[1] who found a very marked increase in the weight in patients suffering from chronic mania when treated with streptococcic vaccine; he also holds that the effect is a non-specific one, and offers a plausible and interesting suggestion, based on Ehrlich's side-chain theory, as to its causation. It is also alluded to by Dr. Courtenay McWatters,[2] who refers to the "pale, fretful, easily-tired children, with no appetite, who are never well and who are 'bad doers.' These cases often do remarkably well, gaining in colour, appetite, weight and vigour after a few doses of tuberculin." I can quite corroborate this.

The third effect of vaccine treatment is, or may be, the production of immunity. This is, of course, best seen in the use of prophylactic injections of typhoid, cholera, and plague vaccines. I believe this immunizing effect is not one to be striven for in the therapeutic uses of vaccines in ordinary diseases. Take, for instance, the organism against which vaccine therapy has won its most conspicuous triumphs—the staphylococcus. Here the curative effect is most conspicuous, but it is extremely doubtful whether it is possible to raise appreciably the resistance of the body to the organism, and certainly it is very difficult to induce the massive immunity that can be produced to typhoid or cholera in animals, whatever the size or the spacing of the injections. In the staphylo-mycoses a further demonstration of the fact that the cure of the lesion is not due to any immune substance circulating in the blood is seen in the fact that the cure of one lesion, whether natural or due to a vaccine, may coincide with the spread of another and the appearance of a third. I may remind you of the remarkable paradox that a horse with an abundant supply of antitoxin in the blood may die from a small dose of toxin, and that a patient whose blood is powerfully bactericidal to typhoid bacilli may nevertheless suffer from a relapse after typhoid fever. It appears to me that the production of a high degree of immunity may even be prejudicial to success in the treatment of an existing lesion, by raising

[1] *Brit. Med. Journ.*, 1910, i, p. 430.
[2] *Practitioner*, 1909, lxxxiii, p. 327.

the dose of vaccine necessary to cause a reaction, and that the real advantage of the small doses that have now come into use is that they avoid any cumulative immunizing effect and allow the patient to revert to his previous state of susceptibility before a second injection is made.

The conclusion I wish to draw from these theoretical considerations is that the curative action of a vaccine is due mainly to its reactive action, and to a much less extent, if at all, to an elevation of the opsonic index. As a practical result of this I do not believe it to be necessary or even advantageous to control the injections by means of the opsonic index. In giving a vaccine I am desirous of causing a local reaction, and of thereby benefiting the patient, not of raising the opsonic index. The danger of the negative phase I believe to be a fictitious one; of course it is undesirable to give too large a dose of a potent vaccine, just as it is undesirable to give too large a dose of a potent alkaloid, but I believe that by this time the doses of the two substances have been fairly ascertained, and, given accurate counting of the bacteria in the vaccine itself, the danger of an overdose is little or none. I have only seen one case in which I felt tolerably certain that an overdose had been given, and in this case I was working with an unknown organism, and one that it was impossible to count in the ordinary way. I had intended at this point to discuss the alleged dangers of the negative phase, but will content myself by referring to Major Russell's article,[1] in which the subject is dealt with at some length. Sir Almroth Wright, in his opening speech, said that the bacteriologist who abandoned the use of the opsonic index was the real foe to vaccine therapy. I must deny this in my own case most strongly. In ordinary clinical work we often meet with cases of great susceptibility to the use of certain drugs, but we do not on that account decline to use those drugs. We proceed with caution, basing our doses on previous experience; and this, I think, is the only practicable method.

Dr. DAVID LAWSON (Banchory): Presumably it is due to the not unimportant part which I took in the discussion held in 1904 under the auspices of the Medico-Chirurgical Society, a discussion which ushered in the great wave of popularity which of late has attended the employment of opsonic methods in medicine, that I have been invited to place upon record my impression of the value and limitation of serum therapy in relation to lung disease.

[1] *Bull. Johns Hopkins Hosp.*, Balt., 1910, xxi, p. 83.

The impressions which I propose to submit are, for the most part, drawn from experience gained in the continuous use of opsonic methods in association with my clinical work during the five years which have elapsed since that time. The vast majority of the cases dealt with have been cases of pulmonary phthisis, mostly at an early stage of the disease. In addition, however, I have had to deal with one case of pure streptococcal abscess of the lung; four staphylococcal infections of the same organ, an uncertain number of mixed infections of pulmonary phthisis, together with a small number of bladder, renal, and gland forms of tuberculosis, and one case of adult tuberculous meningitis. I have also come across a large number of cases of acne amongst my phthisical patients, and these have been invariably treated by vaccine methods, generally with success.

If it be objected that this experience has been gained in a somewhat narrow field of observation, it should be borne in mind that within these limits the experience has been an extensive one, not less than 600 cases having been dealt with during that time. It has been an advantage, arising out of the circumstance that I have been living under the same roof day in and day out with my patients, that I have enjoyed exceptionally favourable facilities for watching the course of the disease in these cases closely and minutely. Moreover, most careful and complete clinical records have been kept of every case throughout that time. It is convenient to submit the observations under four headings, namely :—

(1) Oral administration of vaccines.

(2) The use of T.R. tuberculin in diagnosis.

(3) The use of T.R. tuberculin in prognosis.

(4) The use of T.R. tuberculin and vaccines in the treatment of pulmonary tuberculosis.

First—Oral Administration of Vaccines.

In Copeman's work it is recorded that so far back as 1810 the administration of the contents of vesicles to a girl by the mouth was followed by the appearance of a typical variola eruption, thereby demonstrating the fact that it is possible to obtain a reaction by orally administering a vaccine. When Koch produced his tuberculin he definitely stated that administered orally it was inert, and so great was the influence wielded by that great pathologist that for many years no one appears to have questioned his assertion. In 1908 Dr. Arthur

Latham published in the *Proceedings of the Royal Society of Medicine*[1] a preliminary communication dealing with work done on the oral administration of horse serum. Again, at a later date we find in the *Proceedings*[2] of the same Society a paper by Dr. Latham, in association with Dr. Spitta and Dr. Inman, dealing with the same subject. The latter paper in particular is one of extreme value and importance, not only on account of the extensive nature of the research, but also because of the conclusions at which the observers have arrived. Amongst other conclusions arrived at by those workers is this one: "T.R. tuberculin administered orally exerts a definite influence on tuberculous disease."

With the object of testing the validity of this conclusion, along with Dr. H. S. Gettings I carried out an independent inquiry on the lines followed by those observers. We selected six cases of pulmonary tuberculosis; four of these were apyrexial, and two were pyrexial. All were cases of somewhat extensive disease, and all were more less old-standing. Full clinical records were kept embodying observations of the pulse, respiration, temperature, weight, and urine. We were not able to base any conclusions upon some of those records, and so far as they seemed to have no bearing on our ultimate conclusions they have been omitted. Only those parts of the record essential for our purpose have been retained in the charts prepared. The blood of each patient was taken every morning before breakfast and subjected to the ordinary opsonic test. The doses of tuberculin were given at 10 p.m., when presumably the patient's stomach was empty. Certain precautions were taken to eliminate the more apparent sources of error. To obviate the personal factor, measures were adopted to prevent the observer from being aware whose blood he was dealing with until the close of the experiment. To effect this the blood capsules, both of control and of the patient, were handed to him merely bearing numbers, and it was not until the completion of the counting had taken place that he was made aware of the identity of each blood.

Secondly, in regard to technique, the earlier practice of counting twenty-five or even fifty polynuclear cells is now recognized as not sufficiently reliable, and therefore throughout the inquiry a minimum of 100 cells was dealt with. In this way over 28,000 cells were examined in the course of the research. Further, it had been previously recognized that when the bloods were incubated in batches, withdrawn at the same time, filmed and stained seriatim, the cells of those last prepared showed

[1] *Proceedings*, 1908, i (Clin. Sect.), p. 100.
[2] *Proceedings*, 1908, i (Med. Sect.), p. 195.

a uniformly larger proportion of bacilli than those done first. This is due to the phagocytosis that evidently still goes on at room temperature during the interval of waiting. It occurred to one of us that probably if the leucocytes were chilled such phagocytosis might be stopped. Accordingly, on withdrawing the pipettes from the incubator, they were at once placed in a vessel of cold water and taken out one by one as required. We think that this method gave us increased accuracy in our results, and we desire to draw the attention of our fellow workers to this point.

A study of Chart I reveals the ground upon which the conclusion arrived at rests. The charts have been drawn out in such a way as to show: (1) An opsonic curve indicated by a single line; (2) the doses of tuberculin and the dates on which they were administered by mouth indicated by vertical lines. The question we set ourselves to answer was this: Can tuberculin, when given by the mouth, exert an action on the blood content as indicated in the variation of the opsonic index in a person suffering from tuberculous disease?

Out of thirty-six instances in which doses of tuberculin, varying in amount from $\frac{1}{4000}$ mg. to $\frac{1}{1000}$ mg., were administered, in twenty-three instances (or 64 per cent. of the whole number) a negative phase immediately appeared. The fact that in 64 per cent. of the occasions referred to a definite negative phase was recorded seems to support the claim made by Dr. Latham and his colleagues that tuberculin administered by the mouth in cases of tuberculosis is absorbed and modifies the blood content. Following upon this inquiry, we have gradually substituted for hypodermic medication the practice of administering tuberculin by mouth. Only on rare occasions and for special reasons is the needle employed. So far as we can see, by this change of procedure one has not in any way sacrificed or diminished the benefit which in suitable cases the administration of tuberculin undoubtedly confers.

Prognosis.

There is a practice which obtains in certain quarters amongst pathologists in this country—not confined to the lesser lights, but favoured by some in high places—of predicting to the patients or friends from the slender data which one, two, or at most three tuberculo-opsonic estimations afford, the subsequent course which the disease is likely to follow in cases of pulmonary phthisis. It seems to amount to this: that when these readings are within two points of 1, then the patient is assured that the prognosis is favourable. The patient leaves the room

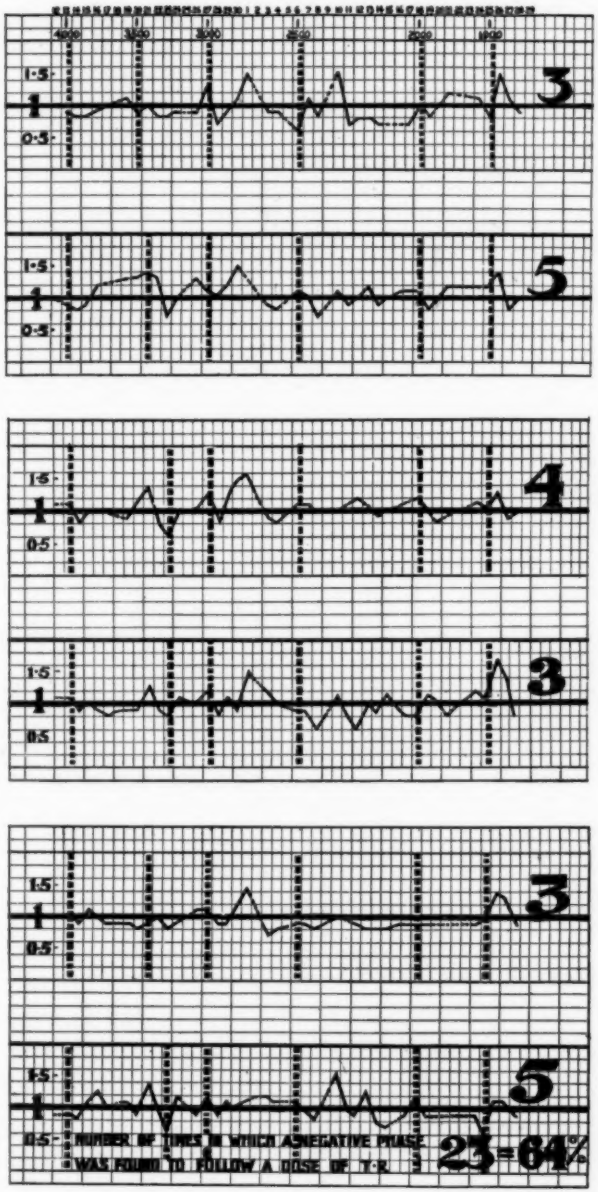

1·5
1
0·5
3
5
4
3
3
5
NUMBER OF TIMES IN WHICH A NEGATIVE PHASE WAS FOUND TO FOLLOW A DOSE OF T·R.
23=64%

CHART I.

greatly marvelling at the wonderful advance the science of pathology has made. The burden of fulfilling these predictions passes to the shoulders of the unfortunate physician to whose care the patient returns. How utterly unreliable the indication furnished by opsonic indices in relationship to prognosis may be, Charts II, III, and IV, selected from a large number of similar ones in our possession, show. Chart II is a simple one. It shows an index of 0·8 and 1·1. Chart III shows a range of indices which pass but little beyond normal limits. If we except the single fact that the physician, who referred this case (Chart II) to him for blood examination, stated that he believed it was a case of early pulmonary phthisis involving one apex, the top chart

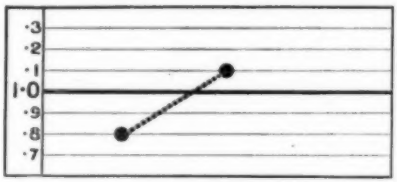


Chart II.

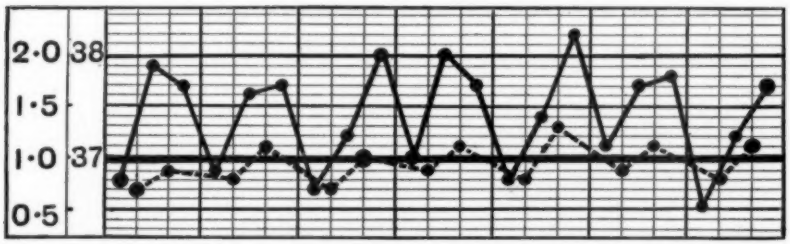


Chart III.

indicates all the facts which were available to one of the most distinguished bacteriologists of the day. On the strength of that limited knowledge supplied, by two opsonic estimations, this observer assured the patient that if he would go to a sanatorium and live there for three months he would return to town a sound man. In the case represented on Chart III a similar opinion was expressed by another pathologist on much the same data. Case No. 1 (Chart II) happened to be one of diabetic phthisis, and the patient died in four months. The urine of Case No. 2 (Chart III) was soon afterwards found to contain a large amount of albumin, and Ehrlich's diazo reaction was very well

marked indeed. Of course, here again an unfavourable termination ensued in the course of a few months. Opsonic indices entirely failed to yield any indication in either of those cases of the presence of urinary conditions so vital to the patient and so necessary to him who would prognose.

Chart IV shows opsonic curves taken from two patients suffering from terminal miliary disease. Neither furnishes any striking departure

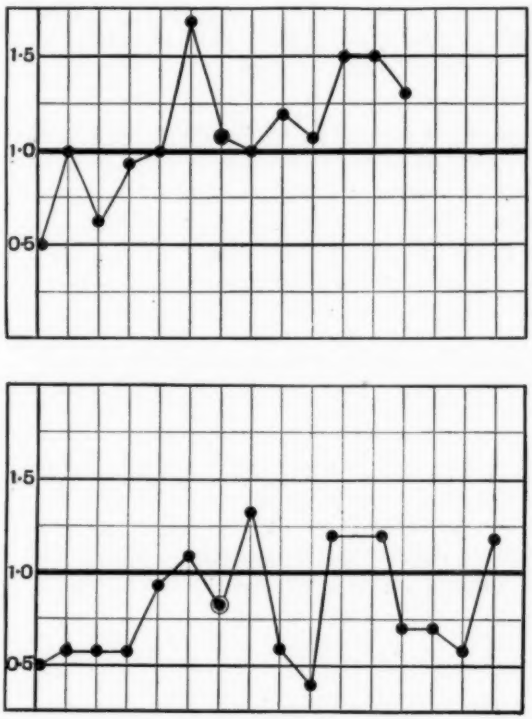


CHART IV.

Terminal opsonic index curves.

from normal. Yet one died in twenty-four hours and the other in seventy-two hours. Instances such as these can be multiplied out of our experience, but they suffice to illustrate this point. In pulmonary phthisis the tuberculo-opsonic index is quite valueless as a guide to prognosis, and such investigators would do well to discontinue a practice which in the end can only lead to disillusionment and disappointment on the part of patients and friends alike.

Vaccine Therapy and Diagnosis.

One point stands out in striking clearness in this connexion. A marked change has in recent years come over the clinical type of case which is presenting itself for treatment at the better-class sanatoria. I recall my experience in the earlier years of my work with feelings of pain. The recollection of that time is a nightmare. Cases which came were for the most part of the advanced and hopeless type. Many were almost moribund. Rarely did I see a reasonably early case. Now all that is changed, and I believe I speak for others when I state that quite 80 per cent. of the patients now arrive in a condition when there is a good chance of a recovery being achieved. For this beneficent change we are largely indebted to Sir Almroth Wright. Whilst the chest signs, or constitutional symptoms, are still little more than suggestive, it is becoming more and more the practice of physicians to obtain a series of tuberculo-opsonic observations of such patient's blood at rest, and with those showing a range which varies outside of normal limits, the physician infers the presence of tuberculosis and at once advises serious treatment. Others prefer to employ exercise with a view to inducing auto-inoculation and then observing the effect of that auto-inoculation on the patient's tuberculo-opsonic index. If the index after moderate exercise shows a marked rise in the altitude when compared with that of the blood taken before exercise, then the presence of tuberculosis may be assumed. Of the two methods, the latter has, in our experience, proved the more reliable. So frequently have I been able in such cases sent for treatment afterwards to confirm with certainty the diagnosis arrived at in this way that I have come to attach a very high value to this use of opsonic methods in the diagnosis of phthisis. It is my conviction that in the presence of indefinably suggestive symptoms and signs of pulmonary phthisis, where the microscopic examination of sputum present has failed on repeated examination to reveal tubercle bacilli, the physician has not done his duty to his patient if he gives it as his opinion, and bases advice upon that opinion, that the case is not one of phthisis, if before doing so he has not first employed one of the methods of serum diagnosis associated with the names of Wright, Calmette, or von Pirquet.

Treatment.

Whilst my experience has been chiefly confined to simple cases of tuberculous infection—by that I mean cases in which only tubercle

bacilli have been found in the sputa, or apyrexial cases with other signs of pulmonary tuberculosis but yielding no sputum suitable for examination—I have met with a number of other interesting conditions and treated them by serum methods which may be worthy of mention.

(1) *Streptococcal Infection of the Lung* (Chart V).—Two cases have been met with where the pus yielded a pure culture of streptococci. In one case complete recovery followed evacuation by the surgeon. The other was also operated upon and subsequently treated by streptococcal serum injections. The injections appeared in no way to modify the

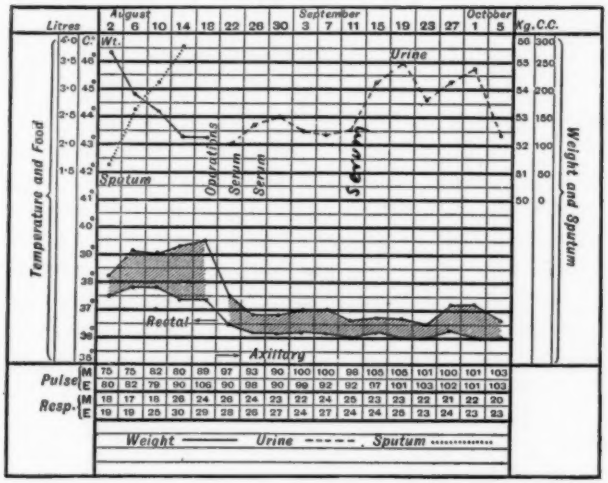


CHART V.

Chart of case of streptococcal abscess of lung (four daily records).

unfavourable course of the disease, and the patient died of exhaustion eight weeks after the operation had taken place.

(2) *Staphylococcal Infection of the Lung.*—Four cases of lung disease, at first believed to be cases of pulmonary tuberculosis but subsequently found not to be so, all yielded pure cultures of staphylococci. The tuberculo-opsonic indices of those cases taken repeatedly were always found to be within normal limits; but it was not so with the opsonic indices to staphylococci. When the initial fever had subsided these were all treated by injections of staphylococcal vaccine obtained from the patient's own brand of organism. In each instance a favourable

result was obtained and complete recovery ensued. I have recently heard, however, that one of the cases had again broken down after two years of office work.

(3) *Pneumococcal Infection of the Lung.*—My experience of treatment by serum therapy of this form is extremely limited. It is confined to three cases in which pneumonia set in in extensive cases of phthisis with mixed infection, and where, at that stage, enormous numbers of pneumococci were found present in the sputum. In one instance we recovered pneumococci from the blood. Each was treated by pneumococcal vaccine without avail. They all terminated fatally within three weeks of the onset of the pneumonic symptoms.

(4) The common form of mixed infection in which tubercle bacilli, staphylococci, and frequently streptococci are present in the expectoration will be referred to at two stages:—(*a*) *The marked pyrexial stage with "hectic" manifestations:* In those cases I have never been able to trace any influence exerted by the use of serum therapy either on the more prominent symptoms, or on the general course of the disease. (*b*) *The apyrexial stage:* In a certain number of cases one finds following a term of ordinary sanatorium life the patient's general and local condition improve and the rapid pulse and temperature disappear. A considerable number of moist sounds persist in the lung and expectoration continues to be produced, which when examined is found to contain tubercle bacilli, staphylococci, and sometimes other organisms. Two such cases after they had reached the apyrexial stage were treated by mixed vaccines for a period of close upon three years. For over a year the opsonic indices of those cases to the first two organisms were estimated daily, and the information derived from this source utilized in the administration of the vaccines. The first organism to disappear from the sputum was the tubercle bacillus, and next to that the streptococcus. The staphylococci have proved in both cases most persistent, and are still present. When I last had an opportunity of examining those patients I found slight physical signs still present at the apices. The patients are very well. One, an army officer, efficiently discharges the full duties of his profession. The other, a lady, leads a careful life in good surroundings, and is also very well. The persistence of staphylococci in those cases is at variance with the view held by some observers (amongst others by Habershon) that the transference of a case of mixed infection from town to pure country atmospheric surroundings invariably promptly changes the case from being one of mixed infection to one of simple tuberculous infection.

Tuberculous Cases.—Contrary to the experience of some, I have never found the exhibition of tuberculin in cases of pulmonary phthisis accompanied by pyrexia followed by the reduction of the fever or activity of the disease. I have seen it act in this way in gland cases, but not where the lungs were known to be involved. On the contrary, I have in more than one instance had occasion to believe that the pyrexia was unfavourably influenced by the vaccine. As a matter of practical procedure I have now abandoned the practice of administering tuberculin in lung cases where fever is present.

In treating early cases of phthisis from the commencement by a combination of tuberculin administration and sanatorium treatment, and watching recovery take place, it has been found difficult, if not impossible, to allocate to the action of tuberculin its just share in promoting recovery. The difficulty is increased when we recall the fact that for several years before we were using T.R. as a routine procedure a very large proportion of cases got quite well under sanatorium treatment alone. There remained a certain number, however, drawn from this class, and also from a class of more serious type, where the recovery advanced to a certain point and then all progress appeared to cease. Many of these cases ultimately went wrong. It is in such cases that we now recognize how great a service may be rendered by the careful, systematic, and persevering use of tuberculin. By adopting tuberculin treatment at this juncture, and continuing its use for a prolonged period —from six to twelve months—I am convinced that a large proportion of those cases, where our ultimate successes in our pre-tuberculin days were few in number or modified in completeness, may now be guided into a state of complete recovery. Moreover, I have the impression, although not in a position to submit that convincing proof which figures may yield, that the proportion of relapses is smaller in those cases than in the cases which have not been treated by tuberculin. In this connexion it is appropriate to recall the experience of so careful and laborious a worker as Trudeau, who quotes figures to show that by the employment of tuberculin in all stages of the disease he found not only that a larger proportion of recoveries was obtained, but that a smaller number of relapses occurred than took place amongst those cases in which tuberculin had not been employed. From other sources, and from what we have heard at this debate, I gather that another view is held in certain quarters upon this subject. We have heard the opinion quoted with approval by pathologists that the use of tuberculin or vaccines in pulmonary phthisis is practically worthless. After making

due allowance for the great service bacteriologists have done and are doing in medicine, we must remember there are branches of the work upon which, before accepting their opinion, it is well to examine the basis upon which that opinion rests. This is a case in point. The average pathologist's experience of the treatment of phthisis is drawn from two sources. Firstly, from the ordinary advanced and usually hopeless case which is to be found in the wards of a general hospital; secondly, from private patients, for the most part old-standing chronic cases who have placed themselves in the hands of the pathologist for vaccine treatment. Many such have eventually come under our notice who had previously undergone more or less prolonged courses of treatment under pathologists who had judged of their cases purely from information derived from systematic opsonic estimations, had never examined their chests, and who did not see their patients for weeks at a time. It is a welcome feature of the situation that this practice one understands is tending to become less common, for neither by training nor by experience is the pathologist who is not working in association with a physician qualified to advise in the best interests of the patient. The patient ultimately becomes dissatisfied, expresses his views freely on all sides on a variety of subjects in language in which such expressions as " charlatanism " is to be found, and the pathologist lays himself open to be misunderstood. An opinion on the value of vaccine therapy in pulmonary phthisis which rests upon so slender a basis as this should not be allowed to pass unchallenged by others, whose wider and more varied experience of the disease has led them to arrive at a conclusion of quite a different nature. Vaccine therapy is a valuable weapon in the physician's armamentarium in treating lung conditions, and where it is possible to employ it in association with systematic opsonic estimations, better results can be obtained than where the guide employed is of a purely clinical nature. In this branch, at least, the bacteriologist's claim to rank equally with the physician cannot be admitted. The information he supplies can be utilized by the physician in the treatment of the patient, but the part he plays must always be subordinate to that of the clinician, who should have full charge of the case.

Dr. A. C. Inman: I must thank the Council of the Society for the honour done me by asking me to take part in this most important discussion.

Those who have preceded me have not always confined their remarks to the various points of the discussion, but have merely given a series of cases treated with vaccines. These have been of value if only because of the number of branches of the medical profession represented by the speakers. I do not intend to add to the list of cases treated with vaccines, but rather to confine myself to the discussions which have arisen around the various points at issue. Firstly, I would venture, with all due deference, to criticize the attitude of the medical profession as a whole to the work of Sir Almroth Wright. If we inspect closely and seriatim the contributions of Sir Almroth Wright to the study of immunization, and to a host of other subjects, we find that they are all attempts at finding a guide or guides to medical treatment which shall make treatment more accurate, rational, and individual. When he was brought into daily contact with numbers of soldiers suffering from and dying of typhoid fever, he sought to find means of gauging the extent of the production of protective substances of the blood. He gave us a method for the quantitative estimation of the agglutinins and of the bactericidal substances. As a result of these laborious investigations he proposed the prophylactic typhoid inoculations which have been attended with such gratifying results. Now Sir William Leishman holds out the hopes to us that the inoculation of typhoid vaccines may be of value in the treatment of this grave disease.

An appointment at a large general hospital in London stimulated Sir Almroth Wright to study a larger class of bacterial infections, and, ever seeking for some guide to help him, he devised the opsonic technique which has led to the adoption of active immunization in the treatment of a large number of bacterial infections. In a few months these labours had shown that the inoculation of infinitesimal doses of tuberculin is able to call forth an immunizing response in the infected individual. This alone would have been a great triumph. A study of the elaboration of protective substances in the blood in other chronic diseases led to the exploitation of the corresponding vaccine in the treatment of such diseases. The results claimed for this method of treatment have been amply confirmed by subsequent workers: witness the parade of successfully treated cases at this meeting. Sir Almroth Wright, and the small band of workers who stood by him, continued in this way to study the various problems of immunity in a laborious, scientific, and accurate manner. One of the most momentous outcomes of these studies has been the enunciation of the principles of auto-inoculation, spontaneously occurring or artificially inducible. These

principles are so important in the study and treatment of bacterial diseases that no physician can afford to neglect them. Gentlemen, to my mind, we ought to emulate these methods of research. We, too, must study disease in this methodical way, and seek for scientific explanations of pathological phenomena, of failures and successes in the treatment of disease.

There has been a good deal of discussion as to who should and who should not administer vaccines to patients. Unhesitatingly I say that the physician must be the selector of cases for this as for any other method of treatment; he must guide the treatment and must be the judge of the results obtained. Equally unhesitatingly I affirm that the trained laboratory worker must be appealed to for help, for help in the more accurate preparation of the vaccine, for help as to the results of the inoculations—whether for good, bad, or indifferent—in the establishing and spacing of doses when necessary, and for evidence of cure or arrest of disease where such is lacking. In this connexion I would emphasize that the laboratory worker is not necessarily a laboratory hermit who experiments with small animals and then seeks to apply his knowledge thus gained to the treatment of human beings, with laughable, nay, even disastrous results. We have now at most of our hospitals clinical pathologists, men who have perforce to spend a considerable amount of time daily in the wards, at the bedside, where they are confronted with the many difficult pathological problems which are constantly before the physician. Many of these men have served a long apprenticeship in the wards, and have, in addition, undergone a long training in laboratory methods. Now, I doubt whether the ideal time will ever come when the clinical pathologist and the physician shall become one and the same person. As our knowledge steadily advances technical methods become more and more difficult and complicated, and for the sake of accuracy a man has to devote a great deal of time to perfecting his technique and keeping his fingers deft. Again, who would doubt that the making of a great physician is the study of a lifetime? I am one of those who believe that the physician, the man who, as Dr. Fowler said, always has to deal with human nature, the practitioner of that medicine to which there is no royal road, is as essential to the study of medicine as the researcher in the laboratory. There is plenty of work for the two of them, and, for the sake of advancement, these two must work together—the *chef de clinique* and the *chef de laboratoire*.

As regards the value of vaccine therapy, I associate myself with those who recognize that it is of value in certain classes of bacterial infections, whilst in others it is as yet impossible to form an opinion on the matter. But surely even the most ardent supporters of this method of treatment recognize that it is still in an experimental stage. At all events, the opinions expressed at these meetings should stimulate a wide and systematic trial of the method in the acute as well as in the chronic bacterial infections. Vaccine therapy is not, and never will be, an empirical form of treatment; it is based on scientific facts and is essentially an individual treatment. Every case approached needs investigation. In the diagnosis of the case I would insist on supplementing the clinical methods with the direct and indirect evidence derived from laboratory tests. Then, as regards treatment, we have of necessity to consider the effect of the bacterial invasion on the body, and the attitude of the body towards the infection. To help us in this respect we have three guides:—

(1) A consideration of the general condition of the patient, symptoms, and physical signs.

(2) The temperature curve, for evidence of intoxication and activity of disease.

(3) The investigation of the serum as regards its content in protective substances—evidence of response or failure of response to the infection on the part of the infected organism.

Each of these is valuable by itself, but the three together are much more valuable. All three must be considered individually and collectively; none must be ignored.

A discussion has centred round the question as to whether vaccines can be administered by the mouth. I invariably prefer the hypodermic method to the oral, and, when possible, I always employ it. But, from a consideration of the clinical results obtained by the oral administration of vaccines, from a study of the blood changes and modifications in the temperature curve which ensue, I am convinced that vaccines can be satisfactorily absorbed from the stomach; and if boils and acne pustules disappear, as I have seen them do, after doses of staphylococcus vaccine given by the mouth, the result may be considered satisfactory from the point of view of the patient. As regards ill effects following the oral administration, I have once seen vomiting and twice a generalized urticaria, but never serious complications.

The relationship between the opsonic curve and the temperature curve is a more important question. In previous communications I have

published a series of observations which show that there is a very definite relationship between the two. These results were all obtained by working with unknown samples of blood, and the two curves were superimposed some time after the experiments had been done. A number of similar experiments made since only go to confirm the results. To obtain these curves the blood and temperature must be recorded synchronously, and over a certain period. I, too, have found no relationship between tuberculo-opsonic curve and the temperature curve in several cases of pulmonary tuberculosis, but further investigation of these cases showed a very definite relationship between the opsonic curve of one or more other organisms recovered from the sputum and the temperature curve. I do not hold that the temperature can give the whole information as to dosage and spacing of vaccines, but I do think that it gives us a very good mirror of what is happening in the blood, and should in no case be neglected. Similarly, it would scarcely be upheld that the opsonic curve given is the whole story of the immunizing response on the part of the body, but it gives very valuable information, and, when practicable, should not be neglected. It is, I think, certain that in a case of active febrile tuberculosis daily, even hourly, variations occur in the blood curve just as in the temperature curve. Now we should get very erroneous information from a single temperature observation taken at, say, eleven o'clock in the morning; similarly, it is no good seeking information from single daily opsonic indices in such cases. It is scarcely practicable to institute four-hourly indices on any number of cases, and so I believe that in these cases the imperfect guide of the temperature curve must suffice, with occasional reference to the opsonic index when necessary. In this connexion it may not be out of place to emphasize the fact that it is a fallacy to presume that all cases of "tuberculous glands in the neck" are afebrile. It is only too often the case that palpable or visible glands in the neck are associated with others equally infected with tuberculosis in other parts of the body; and, on investigation, many such cases turn out to be not localized, but generalized, infections.

The last clause of the discussion is on the limitations of vaccine treatment. It is obviously a very difficult question to discuss at the present stage of our knowledge. Of first importance is the recognition of the fact that we are dealing with active immunity. By appropriate inoculations we try to stimulate the body to respond, and, aided or unaided, to bathe the infected tissues with serum rich in the necessary protective substances. If the body is already responding to its fullest

extent, or cannot be made to respond at all, and under both circumstances the lesion does not heal, we are powerless to do any more by this method of treatment. We are to a large extent at the mercy of the body.

Again, to a large extent, we are at the mercy of the patient, for whereas many of the diseases yield to treatment in the early stages of the disease, later on we are unable to permanently, or even temporarily, arrest the disease. It must be remembered in this connexion that the immunizing mechanism of the body is the last to give out both in naturally and artificially acquired infections. Even passive immunity—e.g., diphtheria antitoxin—cannot protect the body from the evil effects of bacterial toxins when actual damage has already been done to body-tissues. This is a fundamental principle of immunity which needs to be borne in mind by many who still think that a high opsonic index or a high agglutination titre of a serum means that a patient is doing all that is required of him to overcome his infection. It is also further evidence of the necessity of taking everything into account when a case of infective disease is under consideration. Therapeutic active immunization tries to aid the natural processes of healing. It must be borne in mind that the body sometimes manages to arrest and cure disease of itself without help from without. At other times the body does not respond to the infection; the stimulus does not seem to be sufficiently great. In such cases one or two appropriate inoculations are sufficient to rouse the body to rid itself of the infection. The chart (p. 214) refers to a generalized infection with the *Bacillus coli communis*, the organism having been recovered from the urine three weeks after parturition, during which instruments had to be used. It will be seen that after two doses the temperature came down, and this was concomitant with marked improvement, both local and general, which resulted in a cure. Similarly, in a case of acute staphylococcal infection following the use of a dirty needle for the self-administration of morphia in a morphomaniac, the boils which covered the face, trunk, arms, and legs entirely disappeared after two doses of an autogenous vaccine.

In the more chronic infections, such as tuberculous glands in the neck, acne, &c., a longer or even very long time is necessary before the desired result is obtained. I have seen disappearance of extensive glands of the neck after five, seven, and twelve inoculations; others, apparently very similar in extent and character, took eighteen months, and in one case two years, to disappear. In other cases the inoculations had either no effect or only produced slight diminution in size.

I agree that one of the first and most marked results of the inoculation of vaccines is the tonic effect; the circulation improves, the patient gains weight, subjective symptoms disappear, before any noticeable effect is produced on the local condition. In a certain number of cases the next stage is amelioration of the local condition, which may be complete or incomplete. As an instance may be quoted the disappearance of fever and all symptoms in cases of acute or chronic cystitis due to the tubercle bacillus, or the *Bacillus coli communis*. In some cases all the

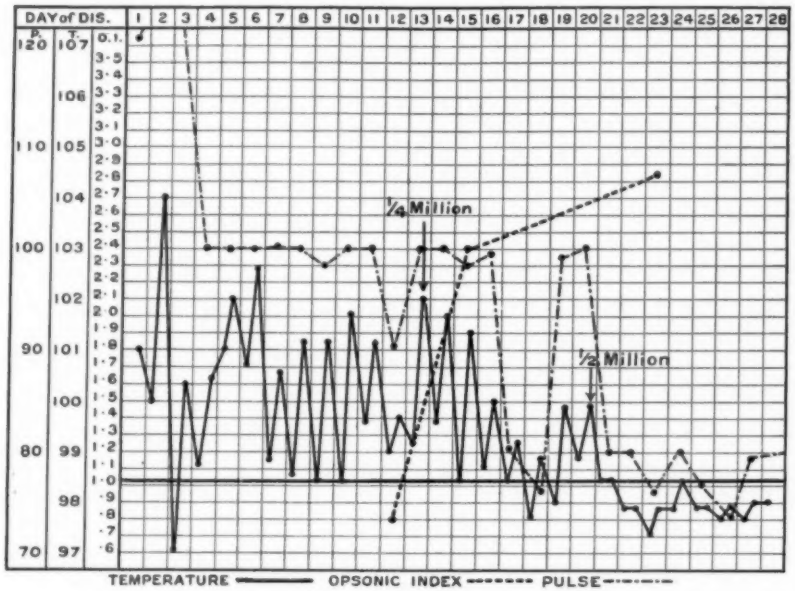


Chart (selected) to show the effect on temperature, pulse and opsonic index of two doses of *Bacillus coli communis* vaccine. The culture was obtained from the urine and vaccine prepared on the eleventh day of the disease.

bacilli disappear from the urine, but in the majority of cases which have come under my observation repeated cultures of the urine have revealed the persistence of bacilli long after all symptoms have disappeared. This must be counted one of the limitations of vaccine therapy.

I have purposely refrained from dealing minutely with the value of tuberculin, its administration and limitations in the treatment of pulmonary tuberculosis. In this discussion we have been inquiring into

the value of vaccine therapy as a whole. The discussion which centres round the tuberculin treatment of pulmonary tuberculosis is as large a subject as is the present one; for this reason I have not touched on it. I trust that this discussion will have the effect of encouraging a wider study and application of vaccine therapy, of putting an end to a useless conflict between physician and clinical pathologist, and of doing away with the prejudice which exists in some quarters for this rational and, above all, individual method of treatment.

Dr. P. Watson Williams: My experiences with the administration of vaccines in infective diseases of the nasal accessory sinuses, by which term I include non-suppurative as well as purulent sinusitis, leave me in no doubt as to the value of vaccine therapy in suitable cases. But there is another field for the useful employment of vaccines to which I desire especially to invite attention—viz., the treatment of incipient adenoid growths. Only a fuller experience than I have had up to the present time would enable me to indicate how far operative measures can usefully be replaced by vaccines, and I certainly do not suggest that, with rare exceptions, definitely established adenoid growths should not be removed by operation. But I have long held the view that[1] "adenoids are simply a lymphoid overgrowth directly resulting from the invasion of infective organisms. In some patients such invasion is productive of marked hypertrophy; these were formerly described as 'of the strumous diathesis'; in others the same measure of infection calls forth less marked hypertrophy. If the infective organisms continue to exist in the tissues and escape destruction, the patient suffers from repeated attacks of catarrh, with greater or less degree of systemic disturbance." So, too, the deafness resulting from the presence of adenoids is due to an infective catarrh spreading up the Eustachian tubes, and practically never is the deafness due to mechanical pressure on, or occlusion of, the Eustachian orifices by the adenoid mass. My colleague at the Bristol Royal Infirmary (Professor Walker Hall) has isolated for me the following organisms from the interior of adenoid growths: *Streptococcus longus* and *brevis*, *pneumo-bacillus*, *strepto-bacillus*, giving group reactions, and *Staphylococcus*, chiefly *albus*, and has prepared vaccines made up in the proportions of the organisms obtained by growing in + 10 veal broth. Children suffering from adenoids have been treated in my clinic with these vaccines, and have always improved in general health, the

[1] Williams (P. Watson), "Rhinology," 1910, p. 75.

associated nasal and nasopharyngeal catarrh has been lessened, and in some instances marked diminution in deafness has resulted without any other treatment. One of the patients now undergoing treatment was referred to me at the Royal Infirmary by Mr. MacWatters, and he writes: "June 4: I saw the child, R., yesterday, and I understand from the parents that she has far less snoring at night, and, from my own observations, I certainly think that there is less post-nasal obstruction than was present before treatment." The cases so treated could not conveniently be operated on, owing to the distance from which they came, and beds at the time were unavailable. I usually begin with doses of 125 million of the mixed vaccine, gradually increasing the dose to 500 million. I am not aware that vaccine therapy has hitherto been tried for this class of affection by other observers, but I am convinced that in suitable cases it is a method of treatment worthy of careful investigation, more especially with a view to preventing the development of incipient "adenoids," and in clearing up the catarrhal symptoms that sometimes persist after their removal, and particularly in those rare cases of adenoid growths where operative measures would probably be attended with special dangers.

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VOLUME THE THIRD

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section of Anæsthetics.

November 5, 1909.

Mr. Richard Gill, President of the Section, in the Chair.

Heart Massage in Heart Failure during Anæsthesia.

By Vivian B. Orr, M.B.

Before reading the account of the case which suggested this paper, it is proposed to touch on some of the points in connexion with the history, mode of action, technique, and indications of heart massage that a search of the literature and personal inquiries have revealed. For much of the matter in the paper the writer is deeply indebted to the works of Lenormant, Mocquot, Crile, Keen, and others, while professional friends have supplied many facts and opinions.

Historically, it is generally conceded that Schiff's experiments in 1874 first proved the value of rhythmic compression of the bare heart in animals; and he explained its action as effecting an artificial circulation. Hake, about the same time, published a work on the subject; no doubt inspired by Schiff's researches. From 1874 to 1877 Boehm and his pupils reported a series of researches on the reanimation of animals killed by potassium salts, chloroform, and asphyxiation, his method of reviving the heart being rhythmic compression of the thorax without opening it. Between 1887 and 1898 the hearts, first of cold and then of warm-blooded animals, were isolated by physiologists in endeavours to fix the conditions of myocardial automatic function. The work of Tuffier and Hallion in France and McWilliam in this country revived interest in 1898, and their experiments were controlled and expanded by Prévost and Batelli.

The theory that fibrillary tremor ("delirium cordis" of McWilliam) was the chief bar to successful reanimation was advanced by them.

Later on Prévost and Batelli showed that during digestion animals were more successfully revived than when fasting. Arabian, after a parallel series of experiments, insisted on the importance of maintaining an adequate body temperature, and stated that if this were done the heart-beats may be restored after thirty minutes' cessation. Prus emphasized especially the necessity for both the respiration and circulation to be artificially reproduced by direct insufflation and cardiac massage, and he asserted that in asphyxial and chloroform deaths in dogs life may be restored as long as an hour after the cessation of the heart.

In 1903 came Lane and Starling's successful case of subdiaphragmatic massage for collapse during an operation in the appendix region under ether. In this case the patient survived. This appears to have been the first reported success; yet there is good reason to believe that both successful and unsuccessful cases of this kind have been not infrequent, but from their very nature few have been published to the world. In 1903 appeared two other suggested methods for dealing with heart-failure. Crile, with the idea that vaso-dilatation in the splanchnic area with fall of pressure was the chief cause of the heart's stoppage, raised that pressure by compression of the lower limbs and the abdomen and by adrenalin injections. Spina injected fluids into arteries afferent to the heart so as to close the aortic cusps and induce the fluid to enter the coronaries. Employing this method of Spina in isolated hearts, Kuliabko proved the extreme vitality of the myocardium and succeeded in restoring rhythmic beats in hearts two days after their isolation, after freezing and after various intoxications.

Recent work by Mummery and others has added pituitary-gland extract to the list of pressure-raising drugs—one more rapid, powerful, and persistent than adrenalin. But it is in the direction of heart massage that we must turn for a practical means of restarting a heart, and the conditions for its successful application have been laid down by Keen in America, Zesas in Germany, and Gross and Sencert in France. They arrived at the conclusion that intravascular and intracardiac injections employed by themselves remain uncertain remedies, that electricity is ineffectual and dangerous, and so we are left with cardiac massage to take its place beside the customary procedures in the treatment of heart failure during anæsthesia. The great difficulty is to define the exact moment when massage should be employed. In abdominal operations nothing should prevent the surgeon from performing subdiaphragmatic massage when heart failure, accompanied by pallor, occurs; while when cyanosis is present they suggest from five

to six minutes of the usual procedures before a resort to massage. This is an abbreviated and much-condensed sketch of the evolution of massage, which leads us on to consider the principles which guide us in applying massage.

The experiments of Kuliabko on the frozen and poisoned isolated hearts of animals show that the muscular mechanism of the heart has a vitality that may be latent even for hours. And clinically we recognize that under certain conditions both circulation and respiration may be suspended and then completely recover; in other words, that there may be an interval between apparent and systemic death. Now, in several of the reported cases there has been a return of both spontaneous breathing and heart-beats for some hours, but death ultimately occurred without any return of consciousness. This points to a greater vulnerability and more rapid death of the delicate tissues of the higher nerve-centres; and it is this condition which demarcates the interval between apparent and systemic death. Assuming that no condition incompatible with life, such as profound injury, hæmorrhage, disease, or poisoning, be present, the most hopeful cases of heart failure are those that occur during anæsthesia.

In the chloroform accidents it is essential to distinguish between the various degrees of anæsthesia and between the different exciting causes that may stop the heart. Most of the dangers of anæsthesia are no doubt avoidable, but there remain a certain number of cases which in our present knowledge baffle the experienced anæsthetist; such as, for example, the so-called "status lymphaticus" and those early fatal cases of heart failure, with pallor, in apparently healthy subjects. The great variation in susceptibility to chloroform of individuals of the same type is common knowledge, and we have only to go a theoretical step further to reach a hypothetical person whose resistance to the toxic action is too feeble to permit of anæsthesia. Given such a person, the question arises whether it is possible to restore animation by efforts to eliminate the toxic dose from the blood. Unfortunately it is difficult to say, when successful, whether the efforts were the essential factors in the reanimation or whether the dose would in any case have been insufficient to kill. The earlier the attempt is made, the greater the chance of success: hence the temptation to a possibly too precipitate resort to heroic measures. Apart from the restoration of the heart-beats, blood coagulation from circulatory arrest must be a necessary and variable barrier to the return of vital functions.

Experiments on animals with a view to determining the length of time that the nervous system will survive after apparent death show varying results. Batelli has shown that recovery of the nervous system may occur after ten minutes of anæmia from electric stimulation; after fifteen minutes recovery is not constant, and it becomes impossible after twenty minutes. On the other hand, after twenty minutes of cessation due to electrical stimulation the heart can recover, but not after thirty minutes; while heart arrest due to chloroform or asphyxia can only be restored up to ten minutes. Prus is the only observer who has been able to restart the heart after as long as an hour's arrest from chloroform and asphyxiation. Arabian asserts that he has restored both heart and nervous functions after thirty minutes' stoppage, and d'Halluin, in his more recent work, has shown that the heart may be revived by massage one hour fifteen minutes to one hour fifty minutes after stopping, and that the nervous system can recover after one hour fifteen minutes; but care was taken to prevent blood coagulation by the use of extract of leech-heads.

In man the experimental work is necessarily limited, but recorded cases proved the possibility of a return of life to the lower centres and the heart without any recovery of the more vulnerable higher nervous centres. Kummell by massage obtained contractions of the heart two hours after death in a man who had been hanged. Sick massaged the heart of a man by the thoracic route, and after three-quarters of an hour of arrest and a quarter of an hour of massage he obtained a survival for twenty-four hours. Riedel could feel the heart-beats after half an hour of massage in a man whose heart had ceased for three-quarters of an hour. This patient survived twelve hours. Delbet has had a good result in cases after ten to fifteen minutes of arrest, and he maintains that after this lapse of time in chloroform cases restoration of function is problematical. Ramsay's case in Launceston, Tasmania, proved the possibility of recovery after five minutes' cessation of the heart, treated by massage through an epigastric incision.

An essential factor in the successful treatment of circulatory failure is the continuous perfusion of oxygenated blood. To ensure this, most authors have employed the method of direct insufflation of air or oxygen. This is not the place to discuss the relative efficiency of the different methods of artificial respiration, but it may be submitted that whatever method be employed some direct intermittent pressure should be brought to bear on the heart, by means of which an artificial circulation is induced or encouraged. In animals, as Boehm showed, this

compression of the thorax acts, as does heart massage, in restoring the beats of an arrested heart, and the analogy in man is borne out clinically in some cases of primary heart failure under chloroform. The maintenance of an adequate body temperature is an important condition in the restoration of the vital functions, which experiments on animals and clinical experience have placed beyond doubt. Bearing on this are the experiments of Prévost and Batelli, who found that when an animal was digesting a meal of albuminoids and carbohydrates its resistance to the action of chloroform was much greater than when fasting.

The practical suggestion of these experiments seems to be that we should minimize as far as possible in man any abstention from food before operations. Another point bearing on this is the fact that it has been long recognized that the inhibitory action of the vagus is intensified in the fasting animal. We know from experimental as well as clinical evidence that the response of the heart to stimulation or massage is influenced by the depth of the intoxication, by the resistive power of its nerve centres, as well as by the means employed to restore animation. In favourable conditions, then, when cessation of the heart-beat has not persisted too long, and when the manœuvres to effect reanimation are adequate, the paralysed, flabby heart will regain its tone and more or less rapidly its regular rhythm. The condition of fibrillary tremor of the heart-muscle (the "delirium cordis" of McWilliam), which has been observed in animals and rarely in man, seems to indicate a condition from which recovery of normal action is problematical. The longer the duration of the anæmia, the more surely will fibrillary tremors supervene.

Some methods that have been employed for reviving a heart that has ceased to beat are as follow :—

Aspiration, to empty a distended right heart, is a method that may do good in cases where the stoppage is caused by the over-distension ; but in the failure that may occur under chloroform, where no overfilling but an acute paralytic dilatation is present, the method of aspiration must necessarily fail. In cases of air-entry through the veins this method has been recommended by Bégouin and Mocquot.

Electric Stimulation.—Lepage used an apparatus whose interruptions were isochronic with the heart-beats, and had a successful case in a condition of traumatic shock. Others have tried various kinds of electric stimulation, but without success in man.

Simple Puncture and Electric Puncture of the Heart are procedures which up to now do not show any record of success.

Intra-arterial, Intravenous, or Intracardiac Injections and Infusions.—These may be employed as adjuvants to other methods, but for practical purposes in sudden heart failure they are too slow and complicated.

The combined procedures advocated by Crile and Dolley may here be referred to. In a series of experiments on sixty dogs killed by chloroform, ether, and asphyxiation these observers obtained the best results from a complicated method that must be difficult to carry out clinically. While doing artificial respiration they massage the heart, directly or indirectly, by pressure on the thorax; they inject Ringer's solution into an artery afferent to the heart, and they add to this solution a therapeutic dose of adrenalin (10 c.c. of 1—1,000). They tried leech extract to prevent clotting, but it seemed to alter the viscosity of the blood in such a way as to cause fatal œdema of the lungs. They found that the longer the delay in resorting to action, the less likely was recovery to take place; and their experiments showed also cases where recovery was maintained for some hours, and then occurred a sudden failure of heart and respiration—a sequence of events that has been observed in man in several instances. Any trauma to the heart predisposed to clotting, and this seems to suggest the necessity for gentle massage in man. Crile reports a case of collapse that occurred at the end of an operation under ether for exophthalmic goitre, in which, after five to six minutes of heart arrest, rhythmic pressure was made on the thorax, and an indiarubber pneumatic suit applied to the lower limbs and abdomen. Just as adrenalin was going to be injected the heart-beats returned, and the patient recovered completely.

Massage of the Heart.—Of the several methods of restoring the beats of a stopped heart, massage alone appears to be justified by the results of its employment in arrest occurring under the influence of anæsthetics. The route by which access to the heart is obtained is either the thoracic, the subdiaphragmatic, or the transdiaphragmatic. By the thoracic route life has been restored in two instances out of a considerable number of cases. The time taken in opening the chest and pericardium, the danger of causing a pneumothorax, and other injuries are sufficient objections to the employment of this route, except where the heart is already exposed in the course of the original operation. The transdiaphragmatic route, from the abdominal cavity through the fibres of the diaphragm into the pericardial sac, has up to now been unsuccessful. There remains the subdiaphragmatic method—where the hand in the abdominal cavity strokes or compresses the heart with the

diaphragm intervening. There have been at least ten successes by this method out of a considerable number of cases, and it may be submitted that this route is the only justifiable one. Where an operation wound already exists in the abdomen the course is clear, unless sepsis or firm adhesions bar the way. Where there is heart failure before the operation has begun, or in the course of an operation elsewhere than in the abdominal cavity, an abdominal incision should be made in the epigastric region, either median or in the "linea semilunaris," either of these being preferable to the curved incision along the left costal margin. The use of indiarubber gloves, or their removal if already soiled, and the rapid disinfection of the skin with some such antiseptic as tincture of iodine are details which make for the successful issue of the operation. Having introduced the hand between the left lobe of the liver and the diaphragm, pressure or stroking movements may be made with the palmar aspect of the fingers over the abdominal aspect of the pericardial attachment to the diaphragm, while some counter-pressure may be made by the other hand outside the parietes. If these manœuvres be done rapidly and the reaction of the heart be prompt, there need be but momentary cessation of artificial respiration. In the case now brought forward this was so, but in more protracted stoppages tracheotomy or intubation, with insufflation of air or oxygen, is necessary to ensure blood oxygenation without interference with the massage. The rate at which the heart expands and contracts is usually slow at first, and the pressure or stroking movements should aim at synchronizing with the systole. When the beats become established there may be a period of tachycardia before the normal rate returns.

The mode of action of this massage is no doubt complex. There seems to be no doubt that mechanical stimulation plays some part in exciting heart contractions. Mechanical stimulation of the diaphragm, too, may cause a contraction of that muscle. This may induce an inspiration, and this in turn may initiate spontaneous respiration. Another suggested effect of massage is the emptying of an over-distended heart, but in the observed cases of heart failure under anæsthetics this condition did not appear to be present.

"It has been suggested, and experiments have been made to prove, that by massage an artificial circulation is established. But this can hardly be the case, since with an inactive heart and no venous pressure the only effective factors in filling its cavities are thoracic aspiration and the pressure on the diaphragm from below." It is possible that massage tends to drive some blood into the coronary arteries, and it is known that

by an adequate fluid pressure in these arteries, even of metallic mercury, the heart-beats may be re-established (Sollman).

Statistics of results give far too high a percentage of favourable cases, as the records of the failures are not published.

Dr. Waldo has reported sixty-three inquests in six and a half years on people who had died under anæsthetics, and Dr. Freyberger seventy-four cases of inquests for sudden death under anæsthesia. These facts are mentioned to account for some at least of the unrecorded failures. This study of the recorded cases should, however, enable us to formulate the conditions of success, and as in the following instance most of those conditions seem to have co-existed, it is narrated as completely as possible under the circumstances.

The patient was a woman, aged between 30 and 40, who was in Westminster Hospital suffering from gastric trouble. Mr. Charles Stonham, whom I have to thank for permission to report the case, proposed to explore the abdomen in the epigastric region. The patient in other respects appeared to be healthy. Chloroform was administered on an ordinary open mask, covered with a single layer of lint. Up to the stage of regular stertorous breathing, with a corneal reflex dulled but obtainable, the course of the anæsthesia gave no cause for anxiety. The patient was wheeled on the operating table from the adjoining anæsthetising room into the theatre, and while the bandages and guard were being removed from the area of operation the breathing suddenly failed, the pupils dilated fully with relaxed insensitive corneæ, and the face and lips became deathly pale. No pulse and no cardiac impulse could be felt. Tongue traction, a good squeeze of the thorax over the diaphragm, and then artificial respiration were tried, without inducing any spontaneous breathing or any return of pulsation in the heart or arteries. Approximately three or four minutes after the heart stopped Mr. Stonham opened the abdomen in the left "linea semilunaris," by an incision commencing just beneath the costal margin and extending 4 in. downwards. There was no hæmorrhage from the margins of the incision. While artificial respiration by Silvester's method was continued, so far as possible without interfering with the operation, Mr. Stonham, standing on the left side of the patient, introduced his right hand into the abdomen until he felt with the tips of his fingers the relaxed diaphragm, and through this a flabby heart that did not pulsate. Gently stroking the diaphragm below the attachment of the pericardium, and pressing towards the anterior thoracic wall, he felt an indescribable sensation of

movement in the heart, and then soon he felt it contract and harden. The normal rhythm of the heart was slowly regained, and spontaneous respiration, contraction of the pupils, and corneal reflex were established almost immediately after the return of the heart-beats. Under ether on an open mask the stomach was examined, but no further operative measures were indicated, so the wound was sewn up and the patient made an uninterrupted recovery.

A careful search through the recorded cases has failed to discover any other instance where massage has been successful in a case of primary anæsthetic heart failure, uncomplicated by any possible reflex shock or inhibition.

Mr. Stonham informs me that during an abdominal operation in the country, the patient's heart stopped while pulling on the mesentery, and that he more than once restored it by subdiaphragmatic massage. The patient recovered. He also has twice unsuccessfully opened the thorax and done direct massage of the heart. He holds very strongly the opinion that in cases of heart failure sub-diaphragmatic massage should be done without delay, and if this paper has made that point clear, it will have achieved its purpose.

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DISCUSSION.

The President (Mr. Gill) said the section was very much indebted to Dr. Orr for his learned and excellent paper. It was a subject of all-absorbing importance in cases of crisis and gravity which from time to time occurred in the course of anæsthetics. He had no first-hand knowledge of that mode of treatment, but he had read accounts of it, and had derived as much knowledge as possible under the circumstances. But in many of the reports all the conditions were not stated. What he was about to say he did not adduce as an analogous example, or as detracting from the excellence of Dr. Orr's remarks. He supposed all present knew that in cases of submersion in water the longest period after withdrawal of the body that the heart's action could be restored was nine minutes. A case was on record in which a man fell from the side of a barge head downwards and, being caught either by a rope or by one of his comrades, was prevented from sinking. He believed nine minutes elapsed before he was hauled on deck, but the man recovered. Nine minutes was a very long time, and perhaps the same effects, which were taking place in the body in that case, might be at work in those cases which were said to revive after an incision had been made in the abdomen and the heart massaged so as to cause it to contract.

Dr. Dudley Buxton said Dr. Orr was to be congratulated upon bringing before the Section a most interesting case of heart massage and upon the lucid manner in which he had summarized our present knowledge of the subject. Although the treatment of dangers under anæsthetics which were due to respiratory failure was well understood and was sufficiently precise to insure prompt measures being adopted, the same was not true when the dangers which threatened the patient's life were due mainly or entirely to circulatory

difficulties. The very fact that circulatory failure commonly ended fatally had led to a feeling of almost hopeless incredulity as to all measures the aim of which was to tide over a crisis caused by circulatory difficulties. As had been pointed out by Dr. Orr, cardiac percussion by Maas' method, acupuncture, and even aspiration of the ventricle, as well as rapidly adopting Nélaton's method, by which the feet were raised above the level of the medulla oblongata, had all been advocated with more or less enthusiasm. The older plans of counter-irritation over the precordium, stimulating the mucous membrane of the nose, anal dilatation and intramuscular or subdermic injections of ether, alcohol, strychnine, and so on, were now recognized as of little value except in slight circulatory complications, since to be of any value it was necessary that some blood movement in the arteries, veins, and capillaries should persist. At one time it was thought necessary to keep a battery in the room when an anæsthetic was given, with the view of being prepared to stimulate the heart by an electric current if cardiac failure occurred. At present heart massage practised by various methods was the plan which many thought held out the best means whereby a circulatory failure could be combated. Probably all the more rational plans of treatment—by which he (Dr. Dudley Buxton) meant heart massage, inversion, cardiac aspiration, rectal clysters, and free bleeding from veins—had failed because no very clear picture of the actual causation of the danger which threatened the patient's life had been in the mind of the person giving the anæsthetic at the moment when collapse occurred. It was certainly impossible to separate respiratory and circulatory phenomena, since respiration depended upon the efficient performance of circulation and vice versa. But for the purposes of discussion it was worth while to regard these cases of collapse as falling into the two categories of white syncope, when the blood had passed into the large venous sinuses of the abdomen, and when the heart and arteries were depleted, while the nerve centres were rendered anæmic; and the blue syncope, when the right ventricle was overfull, the pulmonary circulation was brought to a standstill because the *vis a tergo* was unable to overcome the obstruction in front. The two main factors in causing circulatory failure were fall of blood-pressure and acute distension with dilatation of the heart. Fall of blood-pressure must ensue whenever chloroform and many other anæsthetics were introduced into the circulation. It was not necessary to inquire how far each of the factors, the enfeeblement of the heart muscle, the paretic state of the musculature of the arteries, the vasomotor paralysis with failure of the capillaries to maintain due tension of blood in the arteries, the interference with the thoracic suction exerting its influence upon the large veins, was responsible for the result. The main point was that fall of blood-pressure occurred, and occurred directly in proportion to the extent to which the tissues were subjected to the physiological action of the anæsthetic. It was a mathematical certainty that so much chloroform vapour in so much air when inspired into the lungs and thence absorbed into the blood stream and tissues produced so much fall of blood-pressure. There were many other factors no doubt operative in bringing about the result: the vitality of the tissues, the

quality of the blood, its richness or poverty in blood cells and hæmoglobin, the initial blood-pressure and the possibly peculiar irritability of the nervous system, which rendered it a more ready prey to vagal effects and vagal inhibition of respiration and circulation. But it was not necessary to go into detail in dealing with the subject in hand, since one or all of the subsidiary circumstances mentioned would only act in the direction of making a smaller percentage of chloroform vapour in the inspired air bring about a greater fall of blood-pressure. The objects of heart massage must be to initiate cardiac rhythm, and in the case of the distended heart to assist the organ to empty itself and resume work. The causes of sudden cessation of the action of the heart and of its fibres passing into the abortive condition of delirium cordis, when its power of emptying itself was lost, were a reflex inhibition conveyed through the vagus and initiated in the lungs or through a peripheral nerve, direct effect of chloroform conveyed to the myocardium by the coronary circulation, and the increased peripheral resistance which prevented the heart emptying itself into the arteries. In asphyxia, as in chloroform toxæmia, this condition is present. If we accept the evidence adduced by McWilliam that in chloroform inhalation some degree of cardiac dilatation occurs, the amount varying directly with the strength of the vapour, it is clear that a rapid induction of anæsthesia by a comparatively strong vapour commences a condition which may culminate in paralytic dilatation. We know that strong vapours, vapours greater than we wittingly employ in therapeusis, will bring about an instant cardiac standstill, and that in such cases, as the protoplasm of the heart is irretrievably damaged, no massage or mechanical stimulation can effect recovery. Some cases of accidental overdosing with breath-holding and struggling which occur in the hands of the inexperienced or careless no doubt get near this condition and cause catastrophe, even though artificial respiration and heart massage are employed. In less dangerous, in even comparatively safe, strengths of vapour, as safety is often accepted by persons giving chloroform, concurrent asphyxial complications or what not may cause this heart standstill, and it seems a justifiable and correct treatment to employ heart massage in such cases, both because it may assist in overcoming distension and in bringing about cardio-contraction by mechanical stimulation. But to be of any use it is necessary to adopt the measure promptly and remove all causes which militate against the due performance of circulation: bleeding when the pulmonary circulation is overfull and raising the patient with avoidance of pressure upon the abdomen, since such pressure can only increase the danger of the case by forcing more blood into the overfilled heart. When the cardiac cavities are not distended, and the respiration fails because the medullary centres are empty or are deprived of oxygen, massage may overcome the inhibition if it is present, but inversion and pressure upon the abdominal veins must be practised to fill the depleted ventricles and get blood pumped to the nervous system. It was not known, he believed, whether a heart standstill due to inhibition could be restarted by mechanical stimulation. It was known, however, through the work of Embley and Martin that whether or not the heart would escape from

inhibition must depend upon the actual depth of the chloroform toxæmia. No doubt in the case of persons with asthenic hearts even comparatively tenuous chloroform vapours would produce standstill, and in these hearts if distension occurred, as it did very readily, the heart was incapable of emptying itself, although its muscle was not damaged by the amount of chloroform in the coronary circulation. It was very doubtful whether artificial respiration or bleeding would bring about resuscitation in such cases, and, as heart massage offered the best chance of overcoming the difficulties of the heart, it would appear far more rational to adopt it than to temporize until the distended heart, being deprived of oxygenated blood through cessation of the coronary circulation, passed into a condition of exhaustion and delirium. Considering the small amount of traumatism involved in securing the abdominal route, especially if the transdiaphragmatic method were avoided, it seemed more than expedient to pursue it in every case in which heart massage was practised. To sum up, it was then absolutely essential to carry in the mind the symptoms which would enable the anæsthetist to recognize the various conditions which led to circulatory failure and to separate them from that due to respiratory collapse, and to be prepared to adopt different measures suitable to the different cases. Subsidiary measures were, as a rule, important and helpful, but one formerly advocated—viz., electric stimulation of the heart—was to be deprecated, as it was open to grave dangers. The insertion of the needle into the wall of the heart might very readily produce inhibition by damaging the ganglia, and the passage of a current along it would certainly result in cardiospasm and inhibition, from which there would be no escape.

Dr. A. E. RUSSELL said he had been greatly interested in the paper, and especially as regarded the length of time which intervened between the arrest of the action of the heart and the restoration of life. Three or four minutes was mentioned as the time, and Dr. Russell thought that five minutes was about the outside limit of time that the human brain could withstand the complete cessation of the circulation. But in chloroform arrest of the heart there was a complete arrest of the whole circulation, cerebral as well as bodily, and though animals could withstand a fairly long interval, he doubted whether that was so in man. Dr. Orr spoke of the nervous system withstanding one hour or one-and-a-half-hour arrest of the heart in animals. That was a very long time. An exhaustive series of experiments on cats were done by Stewart, Guthrie, Burns and Pike, in which it was shown that complete arrest of the cerebral circulation only was in no case followed by a complete recovery after a longer period than sixteen and a half minutes. It was also found that the chances of the animal recovering if the spinal cord was transected in the upper dorsal region were greatly diminished. In chloroform syncope the condition was somewhat comparable to transection of the spinal cord, because the cord had been submitted to the same arrest of the circulation as the brain. So that in chloroform syncope there was complete arrest of circulation in body, brain, and spinal cord. There were certain cases of cardiac massage after chloroform syncope, intermediate between

those of immediate death and those of complete recovery; and their subsequent history was very interesting. A few had survived for so long as from twenty to twenty-four hours, and their symptoms during that time had been complete unconsciousness, with spasms and convulsions, unilateral twitchings, dilated pupils, loss of sphincter control, and ultimate death, the heart beating fairly well the whole time. T. A. Green reported such a case in 1906 of chloroform syncope, in which a boy lived twenty hours after restoration of the heart by cardiac massage. The pathology of these cases was obviously obscure. It was possible that irremediable degeneration occurred in the grey matter of the brain; but it was also possible that the symptoms might be due to œdema of the brain. He thought lumbar puncture might offer a prospect of recovery in these cases. He also suggested that in cases of chloroform syncope it was extremely urgent that very little time should be lost in starting the cardiac massage. He thought five minutes was the outside time that the brain could withstand the loss of its circulation, so that if two or three minutes of the ordinary methods of restoration did not restore the heart's activity, the abdominal operation and cardiac massage should be commenced. As Dr. Dudley Buxton said, there could be no doubt but that the massage, by forcing the blood round, in conjunction with the artificial respiration, would restore the coronary circulation, and everything must depend upon that.

Dr. E. A. STARLING desired to join in the expression of thanks to Dr. Orr which had been so eloquently voiced by Dr. Buxton for his interesting and complete account. He wished to relate a case which was not treated by direct massage of the heart in the way which had been described, but what he regarded as massage of the heart through the thoracic and abdominal parietes. The case was that of a child, 11 months old, which in 1906 required an anæsthetic to be administered. The operation was the needling of a congenital cataract. On two previous occasions the same anæsthetic had been given for a similar operation on the other eye, and on neither of those occasions did anything untoward happen. The child was weakly and underfed, and, like so many of those cases, almost imbecile. He gave chloroform on a Junker's inhaler—not the ordinary black vulcanite, but a celluloid mask, which, in so young a child, left considerable space for the access of air. The anæsthetic was taken very well, and when the child was sufficiently under for that purpose, the head was slightly raised to permit a towel being pinned round, the child having been flat on the table. As soon as the head was raised the face became pale, the breathing stopped, and the pulse could not be felt. He pulled the child to the edge and hung the head over the end of the table, felt that the throat was clear, and drew the tongue forward, and started artificial respiration by compression of the chest. No beating of the heart could be heard with the stethoscope. Ether and strychnine were injected subcutaneously, though he had always regarded this as very feeble treatment. The child lay flaccid, with a cyanosed face, the cyanosis, he thought, being due to the head hanging down; and there was no sign of entry of air into the lungs. Warm saline was injected *per rectum*, but was returned,

and the child appeared to be dead. He did not refer to the condition of the pupils, because for that operation atropine had been freely used. He passed the fingers of his left hand well under the ribs of the left hypochondrium, and compressed all the structures between his fingers and the ribs supported by the palm of his hand. After doing that five or six times there was a gasping inspiration, and then, with the stethoscope, the heart was heard to be beating irregularly. Artificial respiration was continued for some time, warmth was applied, and an enema of hot water and brandy was given and retained. Gradually the breathing became regular, and the colour of the patient and the pulse improved. The child was put to bed with hot-water bottles, and in an hour was able to take warm milk and water. The amount of chloroform used was between ½ dr. and 1 dr. A fortnight later he administered chloroform to the child without trouble, and once more after that. One remark he wished to make was on the necessity of maintaining artificial respiration for some time after such an event. The initial pulsations of the heart were not sufficiently strong to do without the aid of artificial respiration. The other remark he wished to make had some historical interest—a by-product of the case referred to by Dr. Orr, in which Mr. Lane massaged the heart through the diaphragm. He talked the matter over with Mr. Lane at the time, and he said he gained the idea from a visit to the laboratory of Professor Starling, where, when an animal was under anæsthesia and the heart stopped beating, the routine procedure was massage of the heart to restart it, and that was nearly always successful.

Dr. ORR, in reply, thanked the speakers who had been kind enough to express appreciation of his paper, the compilation of which had afforded him much pleasure, and taught him many things which he had not previously known. Many interesting details he had not been able to include in his paper. In answer to the President, there were distinct conditions: the heart failure resulting from asphyxiation, from electrical stimulation, from chloroform, and from other intoxications. So he did not think it was quite fair to argue from analogy with regard to the time after which asphyxiation caused a fatal result. Dr. Buxton's criticism amounted to a filling in of details which he had himself omitted. With regard to the distinction between white and blue syncope, the case reported in his paper was white syncope, while Dr. Ramsay's case was an excellent example of blue syncope. In both there was success by means of massage. The prompt action in carrying out the massage was the crux of the whole matter; time should not be wasted over abortive measures, such as tongue traction, intravenous or hypodermic injections, puncture of the heart and so on. He maintained strongly that a thoroughly prompt resort to epigastric incision with massage through the diaphragm was indicated as soon as one was satisfied that heart failure existed. Dr. Russell had spoken of the great vulnerability of the higher nervous centres, a point which the paper brought out. The suggestion with regard to lumbar puncture in the cases where recovery had apparently taken place, except in the highest centres, where

the patient had restarted respiration and circulation and the higher centres remained in abeyance, seemed to be valuable, and it could certainly do no harm. If anybody should do it, and it were successful, it would be due to Dr. Russell for his suggestion. It was true that the heart could be got at without opening the abdomen in a small child. A friend of his told him of a case at a fever hospital. The child was unconscious, and the doctor did tracheotomy for diphtheria. The baby was apparently dead. He tickled the heart by pressure under the ribs on the left side, and found distinct evidence of the heart beating again. After it had been beating for a short time he desisted in his efforts, and it stopped; on recommencing the stimulus, the heart beat again. The child, however, did not eventually recover.

Notes on an Apparatus for the Administration of Ether by the purely Open Method.

By R. H. Hodgson, M.D.

By your kind permission I have the pleasure of showing an apparatus which, I believe, is as effective as it is simple in the administration of ether by the open method, by which I mean that the patient is anæsthetized by ether and air without breathing a second time the same air or ether. For I feel that the disfavour in which ether anæsthesia is held by many is almost wholly due to the want of recognition of the objectionable rebreathing. There can be little doubt but that patients who are anæsthetized by the closed or partially-closed methods are more or less poisoned by their own breath, and that the deaths attributed to ether should be in reality assigned to auto-genetic narcotic poisoning which arrests the circulation in the capillaries of the lungs, whereas ether stimulates that circulation. We take great care that our ether shall be pure, yet when we give it by any other method than the absolutely open method we mix it with most irrespirable gases. In support of this opinion, I may point out that with the strictly open method I experienced none of the causes for anxiety inseparable from the administration of ether by the closed or partially-closed method, such as bronchitis, pleurisy, nephritis, &c., nor was there any struggling or excitement whilst going under or coming to. Ether, given in this manner, seems to me to be free from danger, no matter what the patient's condition; and, furthermore, by making ether the vehicle I have been able to give by

inhalation the following drugs: Camphor, hexamethylene tetramin, carbolic acid, red iodide of mercury, iodide of arsenic, formalin, hamamelis, creosote, perchloride of mercury, guaiacol and iodine, the fumes from many of which are ordinarily most deadly for inhalation purposes, but which may be inhaled with impunity through an apparatus such as I now show. Thus it will be seen that pulmonary tuberculosis may be directly attacked by almost any drugs in the very seat of its origin; and it is therefore only reasonable to conclude that, with the help of the anæsthetist, pulmonary tuberculosis may be arrested and destroyed with great certainty.

Although I have so far limited the value of inhalation to the cure of tubercle of the lungs, I do not mean that the treatment is useless in tubercle of other parts of the body, but that the lungs afford the most convenient evidence of its benefits. I have with this apparatus administered ether alone or mixed with other drugs in extreme cases of pulmonary phthisis with cavity and without, in general tuberculosis, in tubercle of the lung and kidney, in tubercular pleurisy, and in advanced organic heart disease with dilatation and impending death. In all cases rapid improvement has followed and no danger has been apparent. Consumptives taking ether this way show rapid alleviation of their symptoms, the expectoration is reduced in quantity in a few hours, the cough is lessened, the temperature lowered, the sweats reduced in number and severity, the appetite marvellously improved, the desire for and assimilation of fatty foods correspondingly increased, and the area of dullness perceptibly diminished. Another great advantage of treating pulmonary phthisis through the medium of ether inhalation is that it leaves the healthy parts of the lungs in a state of hypertrophic compensation without producing emphysema. The fact that I have administered ether by itself or in conjunction with other drugs by this apparatus on upwards of 450 occasions leads me to think that in bringing this method before you I am not only showing a useful instrument, but am opening up a large field of work for anæsthetists, whereby pulmonary consumption can be rapidly and effectually dealt with and anæsthetists become the lung operators of the day, and their instruments will be made of the finest tempered drugs.

The apparatus, though simple, has the following advantages over any other that I have seen: (1) All the air breathed must first pass through the drugs; no second breathing of the same air can take place. (2) The quantity of ether necessary for a long operation is small—2 or 3 oz. (3) Freezing of the ether may partially take place in

exceptional circumstances. (4) The patient feels but little sensation of suffocation. (5) There is practically no struggling going under or coming to, and no fear of hæmorrhage. (6) The lungs and bronchial tubes are not choked by viscid mucus. (7) The after-complications of ether administration do not arise. (8) There are no sponges or other media liable to become foul or to accelerate freezing.

The enormously increased power of assimilation of fatty foods, and the rapid relief of congestion of the lungs by those subjected to ether, suggest the treatment in cases of diseases of the pancreas and in pneumonia.

The PRESIDENT said the Section was very much obliged to Dr. Hodgson for the trouble he had taken in coming to the meeting to describe his very interesting apparatus. No doubt there was a field open for instruments, especially in the case of ether administration, and he had no doubt that Dr. Hodgson's instrument would be tried and would be proved to be as successful as he stated it to be.

Section of Anæsthetics.

December 3, 1909.

Mr. Richard Gill, President of the Section, in the Chair.

"Lymphatism."

By H. Bellamy Gardner.

WITH PATHOLOGICAL REPORT

By R. Salusbury Trevor, M.B.

Thomas Carlyle, in his lecture on "The Hero as a Man of Letters," says: "So soon as men get to discern the importance of a thing, they do infallibly set about arranging it, facilitating, forwarding it, and rest not till, in some approximate degree, they have accomplished that."

To-night I venture to draw your attention to a pathological condition of the human body which has been found associated with a number of cases of sudden death—death which is so dramatic in its unexpectedness that an apparently healthy child, sitting up in bed playing with its toys, has two minutes afterwards been found dead, without uttering a sound or giving warning of illness of any kind. This pathological condition is "Lymphatism," also named "Status lymphaticus" and "Status thymicus." I will endeavour to relate certain facts with regard to it which seem to be of such practical importance as to call for the very earnest co-operation of several branches of the medical profession in their elucidation.

Two years ago Dr. W. J. McCardie published an exhaustive paper on "Status lymphaticus in relation to General Anæsthesia" [7]. In

the course of it he says: "The merit of having directed into the right path the investigation of many hitherto inexplicable cases of sudden death is undoubtedly due to Paltauf [8], who in 1889 and 1890 found in them, post-mortem, an enlargement of the tonsils, of the lymphatic glands, of the follicles at the base of the tongue, of the spleen, an enlarged thymus gland, and in most cases also a narrowing of the aorta, sometimes called hypoplasia or under-development." Dr. McCardie mentions, also, swelling of solitary follicles and Peyer's patches in the intestine, a dilated heart, and extremely flaccid cardiac muscle. To these may be added some less constant features—great pallor of the skin, enlargement of the whole tongue, of the thyroid gland, of certain superficial glands.

The mode of death in many cases of infants and young children has been either by increasing attacks of dyspnœa, proving fatal within a few hours, preceded sometimes by fits of stridor occurring during a few weeks; or simply by sudden cessation of respiration and pulse, without obstructive asphyxial phenomena. In older children and adults death has been of the latter character.

Enlargement of the thymus gland is found especially in association with rachitis, exophthalmic goitre, myasthenia gravis and leukæmia. Dr. Leonard S. Dudgeon, in an excellent monograph, "A Contribution to the Pathology of the Thymus Gland" [2], records the examination of the thymus gland in 111 cases. In 95 of these the thymus weighed less than 7 grm., or about $\frac{1}{4}$ oz. In the remaining 16 cases, which died suddenly, the average weight was 25 grm. He states that the growth of the thymus gland proceeds during the first two years of life, when it reaches its maximum period of development. From the end of the second year until puberty it remains somewhat stationary in size; after this a rapid degeneration commences, and in adults of all ages up to 70 it usually appears to consist of little more than fibrous tissue. A few islets of thymic tissue are to be found in this by careful search. The average weight of the gland from birth up to the age of 2 years is roughly about 7 grm. to 10 grm. All cases in which the thymus retains its infantile size beyond the normal period of retrogression must be regarded as instances of relative hyperplasia or hypertrophy. Dr. A. S. Warthin [11] says: "From the surface of the fresh thymus a thick fluid may be expressed. It is normally sterile, and possesses an acid reaction. This fluid is probably, for the greater part, the result of a post-mortem change or digestion. Inasmuch as this does not occur in the lymphoid tissue in other parts of the body, it may be assumed that

there is some substance within the thymus responsible for this liquefaction (proteolytic enzymes)." Several writers have maintained the possibility of an enlarged thymus producing dyspnœa through tracheal compression, especially in infants and young children. To illustrate this view I now exhibit photographs of "Frozen Sections of the Newborn Normal Infant" from an article by Dr. Jacobi, of New York [6]. He states that the distance from the vertebræ behind to the manubrium sterni in front in an infant aged 8 months has been found to be only 2 cm. Dr. A. S. Warthin relates that sudden death from fright or intense emotional excitement, or while bathing or swimming, has been found to be associated with enlarged thymus gland, and in one case he saw the fatal attack of thymic asthma was apparently induced by a slight burn from a hot-water bag. Many deaths from "overlying" also belong to this class.

To illustrate the circumstances and mode of death I have chosen certain clear and definite cases where this pathological condition, and this alone, has been found post mortem in the absence of any other lesions sufficient *per se* to account for dissolution.

Case I.

Dr. Hugh Thursfield [9] relates four marked cases of sudden death, associated with lymphatism, which he observed at Great Ormond Street Hospital. Of one he says (Case 2): A boy, aged 13 months, was admitted to the wards with rickets and tetany. This last was very slight; a "fit" was said to have occurred on the previous day. While I was standing at the side of the cot the child suddenly sat up, its eyes became fixed, it ceased to breathe, became very slightly blue and then quite white, and fell back dead. The whole series of events occupied less than thirty seconds. There was certainly no question of asphyxia; it appeared rather as if both circulatory and respiratory centres struck work simultaneously.

Post mortem the rickety lesions were found to be slight. There was a small patch of lung with broncho-pneumonic consolidation. The heart was dilated, the ventricles both containing fluid blood and no clot. The thymus gland weighed 16 grm. and was bulky; the lymphatic glands in the cervical, bronchial, and mesenteric groups were enlarged. Peyer's patches and the solitary follicles of the large intestine were unusually prominent.

Case II.

Dr. Paul Herbier [4] records that a pretty plump child, aged 9 months, came under the care of M. Haushalter for eczema extending freely over the face and hairy scalp. The secretions of this eczema had dried up in an unusual manner two days before this. The child presented very great dyspnœa, with retraction of the ribs, some cyanosis, and then some convulsive phenomena. He died less than an hour after the onset of the dyspnœa.

At the autopsy no visceral lesion which could explain his death was found, excepting a hypertrophied thymus weighing 50 grm.; its surface presented several petechial patches and the bases of the lungs were congested. The liver was a little enlarged, with a slight degree of fatty degeneration.

Case III.

Mr. Ernest Humphry [5] relates six definite cases he has met in the past two and a half years. One patient (his fourth case) was a male aged 29, a congenital imbecile who was well nourished, well developed, of good physique and general health. He got into a bath, with 5 in. or 6 in. of water at 95° F. in it, at midday. An attendant was present. After two minutes he suddenly slid down from the sitting position, and, although nothing abnormal had been noticed previously to this, it was quite obvious that he was dead. The eyes remained open, and the pupils were dilated, so that this, together with the sudden cessation of all movements and sudden pallor, alone constituted the dying phase.

Post mortem: The thymus weighed 28 grm. There were large lymphoid nodules at the back of the tongue. There were some slight adenoids. The mucous membrane of the stomach presented a complete chain network of lymphatic nodules, especially at the fundus. Peyer's patches and solitary follicles were thickened. Mesenteric glands were considerably enlarged; the majority of them were quite red. There were groups also which were quite white in colour. The impression conveyed was that the white ones had been enlarged for many years, but that the red were affected by a superadded inflammation of quite recent date. The heart muscle broke down very readily on pressure; valves normal. The brain was normal in appearance.

CASE IV.

Dr. McCardie states that two deaths have been recorded under local anæsthesia; one reported by Horoszkiewicz was a woman, aged 30, who was operated upon for a small cyst in the neck; altogether 0·075 grm. of tropacocaine was injected locally. At first the patient laughed at her fears, then suddenly became pallid and convulsed: the operation was stopped until the patient was better, then the neck was extended and the operation begun again. Respiration then stopped, her pulse became small, and death quickly followed. Post mortem was found a typical condition of lymphatism.

A number of patients have also died under general anæsthetics in whom the condition of general lymphatic hyperplasia has been found to exist. Dr. McCardie has examined the records of thirty cases in which the anæsthetic used was in seventeen chloroform, in six ether, in five a mixture of chloroform and ether. Two were doubtful cases in which the anæsthetic was nitrous oxide, also two deaths occurred after anæsthesia.

CASE V.

Another case from Mr. Ernest Humphry's record is this: The patient was a boy aged 14, apparently normal in health on examination excepting for slight deafness, enlarged tonsils, and adenoid growths in the naso-pharynx. Properly prepared and quite cheerful, he was given 2 c.c. of ethyl-chloride from a closed rubber bag by an experienced anæsthetist. After three inspirations the corneal reflex suddenly disappeared; the mask was removed, the breathing stopped, and the face became cyanosed. The operation was hurriedly proceeded with, in the hope that the consequent bleeding would help to relieve the condition. This not being so, every means of resuscitation was employed. Artificial respiration was performed unremittingly for three hours. The heart-sounds, though at first audible, were rather distant and irregular, and the colour, general appearance, and expression of the face were continually changing. As late as two and a half hours from the beginning of artificial respiration, a particularly gross change was noted. The face changed from a blotchy purple and white to a uniform pink colour. The lips lost their lividity, and the flabby, purple tongue became red, regained tone, and started bleeding, a bright-red blood. Natural respiration was, however, not re-established.

Post mortem: Enlarged thymus was present, which weighed 28·3 grm. Both lungs were rather congested. The spleen was very slightly enlarged, and Malpighian corpuscles were prominent. Possibly there was some thickening of Peyer's patches in the intestine. The mesentery was studded with numerous pink glands, nearly all of which were about the size of a sweet-pea.

I will now relate the case upon which my immediate interest in this subject is founded:—

Case VI.

The patient was a boy aged 13, rather big for his age and of somewhat flabby build. The hair and complexion were fair; his face did not present the adenoid appearance, and he was not an habitual mouth-breather. He was apparently in ordinary health, with a regular, soft pulse of about 80 per minute. His heart-sounds were normal; his chest was rather long and flat, but its elasticity and expansion were quite up to the average. It had been decided to enucleate his tonsils, which were much enlarged and deeply seated, and to remove a very large mass of adenoid growth from the nasopharynx. I was told that he had suffered from eczema, but this had all disappeared excepting the slightest traces of epithelial scaling near the angles of the mouth. He had never had an anæsthetic before; he was quite cheerful, and lay down supine on the operating table with complete confidence. A nurse, whom he knew well, stood at his side and held his hand. He was told to slowly breathe upon a large Skinner's mask covered with one layer of flannel, and this he did regularly and well. It was then slowly moistened with a mixture of one part of chloroform and two parts of ether (by volume). I kept talking to him until he fell asleep, and the induction was quite uneventful. He breathed regularly and remained of a good colour. He moved his hands very slightly, but did not struggle at all. In about six or seven minutes from the beginning of the administration, when 4½ dr. of the mixture had been used, the breathing was automatic and slightly snoring in character, and the corneal reflex was present and moderately active. His head was drawn a little over the pillow, still resting on the table and *not* extended backwards. I opened his mouth by means of a Mason's gag between the left molar teeth and inserted the tube terminal of a Junker's chloroform bottle in the left angle of the mouth. I again tested the corneal reflex, which was

moderately active, as before. This was evidence of the presence of exactly the same degree of anæsthesia which I have previously produced in several hundreds of cases for this operation with the same anæsthetic mixture. This degree retains the coughing reflex, so that the throat cannot be silently invaded by effused blood, and, as a rule, is attended only by the slightest reflex movements of the limbs during the surgeon's manipulations. The patient was lying quite flat on his back, with the face in the mid-line looking upwards towards the ceiling.

The operator inserted a caged Gottstein's curette into the naso-pharynx, and with one sweep removed the large adenoid growth in one piece. There was no reflex movement by the patient. The operator sponged the pharynx and I gently pumped a little chloroform vapour into the mouth; the operator curetted the left portion of the nasopharynx, and then the right side. At this third sweep of the curette the patient made a very slight retching movement, and a little bluish blood appeared in the pharynx; his head and body were rotated to the right side for drainage, and a teaspoonful of bilious fluid was ejected from the right corner of the mouth, and he stopped breathing. The throat was sponged. I squeezed his chest; air escaped and entered audibly and freely. I looked into the pharynx and felt behind the tongue; there was no blood nor other obstruction there. Suddenly a deep navy-blue coloration appeared on the forehead and temples, slanting away towards the ears, with a definite border along the zygomatic arch. The operator afterwards likened this band of colour to the ashy blue of the epileptic's face during a fit. The lips, ears, and face were at the same time almost normal in colour. The corneal reflex now disappeared, and the pupil, which had been about $2\frac{1}{2}$ mm. in diameter, enlarged to 4 mm. or $4\frac{1}{2}$ mm.

From the moment of respiratory paralysis not one single natural muscular or respiratory movement ever took place. Still continuing to squeeze the chest, I put a clip on the tongue and drew it forward, inserted an oxygen tube in the mouth instead of the Junker's tube, and performed artificial respiration, with the aid of the surgeon and of another doctor who came to help us, for four and a half hours. A few seconds after the respiratory paralysis the pulse could not be felt, but the colour was only a little purplish and did not become white unless we ceased squeezing the chest momentarily. The head was lowered to fill the cerebral centres, and the face then became cyanosed; the head and body were raised in order to relieve a possibly dilated right cardiac ventricle. Pallor then appeared, so he was laid down again. The corneal reflex was absent and the pupil remained moderately large, but

the colour now improved and became pink at times, though no pulses could be felt. I squeezed the heart and ventricles from the flaccid epigastric region gently between my fingers and the chest-wall; there was no movement of the heart, which was flaccid to the touch. The patient's body was kept warm with hot blankets, the feet were raised, and hot brandy and beef tea injected into the rectum. A specialist who had been sent for faradized the phrenic nerves, but there was no response from the respiratory muscles. After an hour and a half of artificial respiration the patient's face became the colour of pink blotting-paper, the body was warm and dry, and I thought that the pupils were growing a little smaller. I could hardly believe the boy was dead, and at this time a little bright-red blood appeared in the pharynx, slowly oozing from the operation area. I felt almost inclined to send for his relatives to reassure them, as I was still hopeful; but I did not do so, as the pink colour gradually faded, the face became mottled blue and white, and the extremities grew cold as time passed on. As no voluntary breathing had taken place since 10.15 a.m., artificial respiration was stopped at 2.45 p.m. because it had no effect. When this was done the face at last became completely white. I had only given two or three gentle puffs of chloroform during the operation, with the object of maintaining the existing stage of anæsthesia during the enucleation of the partly buried tonsils which was to follow. This might take five to ten minutes, or longer, and the anæsthetist has always to be prepared beforehand for the longer operation.

With an experience of fifteen years devoted entirely to the study and practice of anæsthetic administration, I am of opinion that these symptoms are not those of anæsthetic poisoning in the normal human subject. First, because the corneal reflex was present when respiratory cessation occurred. This I have never seen before without explanatory pathological cause, such as cerebral abscess or sudden profuse hæmorrhage. Secondly, because there was no syncopal pallor before or at the time the breathing ceased, which is an invariable accompaniment of failing circulation. Thirdly, there was, at no time, either obstruction of the airways or any laryngeal or tracheal sound, while air entered and left the chest freely all the time of artificial respiration. I found the most efficient method of performing artificial respiration, and that which was also least exhausting, was effected while standing behind the patient as he lay on his left side. Raising his right arm with my own caused a deep inspiration, then bringing his arm down upon his right ribs, pressure of my own weight against his back rotated him on to his

left side semiprone and caused audible expiration. I am satisfied, by my observation of this case, by the colour and warmth of the face and body and slight bleeding of bright blood after an hour and a half, that with a fairly elastic chest-wall this method of artificial respiration also causes an artificial circulation, at any rate, in the trunk and head. This is borne out by the observation of the electro-therapeutist, who says in his report to me: Faradaic reaction (while the colour in the face was good) was found to be wanting in the extremities, though this reaction was quite brisk in the case of the muscles of the trunk (loss of faradaic reaction in the muscles is generally supposed to be a sign of death).

Report of the Necropsy of the Body of a Male aged 13 (*Case VI*).

By Dr. Salusbury Trevor.

The body is that of a well-nourished boy, 5 ft. 4 in. in height. There are slight traces of eczema about the mouth. Enlarged glands can be felt in both groins, but not in the axillæ; the enlargement is not great. Dried blood present in both nostrils and in the corners of the mouth. Thorax: Pleuræ normal. Lungs: Right 14 oz., left 12 oz.; both are congested, but otherwise healthy. Larynx (fig. 1): The epiglottis is studded on its laryngeal surface with small white dots of lymphoid tissue; there is an increase of the lymphoid tissue in the folds outside the arytæno-epiglottic folds. Trachea contains a small quantity of blood intimately admixed with air which has entered the trachea during artificial respiration; infra-tracheal glands are enlarged. The thymus (fig. 2) is very large, normal in colour and in consistency, and weighs 2 oz. (= 56·6 grm.).

Histological examination: The thymus gland (fig. 3) shows diffuse hypertrophy. There is no evidence of fatty change. Corpuscles of Hassall are plentiful, some being of large size and showing degenerative changes. In these the typical concentric arrangement is lost, and is replaced by a granular debris in which polymorphonuclear leucocytes and lymphocytes can be seen. In some instances these white cells have been carried into the granular areas in the process of cutting the sections.

Pericardium normal. Heart 10 oz. The muscle is soft and watery from œdema; no obvious fatty change; on the mitral valve are two small glistening and fibrous vegetations of old date; the valve does not appear to be incompetent; the other valves are normal; the aorta is

of normal size and not hypoplastic; it shows some early fatty change; the coronary arteries are healthy. Abdomen: Peritoneum natural. Glands (fig. 4): The mesenteric glands are much enlarged; one of them shows old caseating tubercle.

Histological examination: The glands examined were from the groin (fig. 5) and from the mesentery. In both cases the sections show slight endothelial proliferation. The absence of any marked "large-celled" hyperplasia is noteworthy. The germinal centres in both glands are normal. No evidence of tuberculosis was found in any of the sections examined.



FIG. 1.

The larynx (half actual size).

Liver, 3 lb. 12 oz., is pale, greasy and fatty; gall-bladder normal; portal vein and ducts normal. Spleen 8 oz.: The spleen is enlarged, and on section the lymphoid tissue (Malpighian bodies) is very prominent; the consistence of the spleen is about normal.

Histological examination: The spleen (figs. 6 and 7) shows a marked grade of endothelial proliferation of the type met with in cases of toxæmia. The Malpighian follicles are very prominent, but show no variation from the normal.

Pancreas normal. Kidneys, 6 oz. each, are congested; both have a faint smell of chloroform. Urinary tract normal. Alimentary tract: An operation for the removal of adenoids has been performed. At the



FIG. 2.

Hypertrophied thymus gland (half actual size).



FIG. 3.

The thymus gland, under microscope.

base of the tongue is a very marked development of lymphoid tissue, which is far in excess of the normal. The tonsils on either side are very large and deeply buried. There is some swelling and a slight scratch on the uvula as the result of sponging. The œsophagus and stomach contain traces of blood and mucus. Throughout the intestinal tract there is an extraordinary development of lymphoid tissue, so that the solitary follicles and Peyer's patches throughout the small (fig. 9) and large (fig. 11) intestines are raised and nodular, giving a gritty feel to the bowel-wall. Cranium: Skull normal. Brain, 3 lb. 4 oz., is normal; there is no increase of fluid; there is no evidence of œdema around the region of the respiratory centre.

A physician who had been consulted with regard to this patient said that he saw him first four years ago. There was a parental history of gout. The boy had suffered from severe eczema from infancy, of a weeping or suppurating character, distributed all over the chest and back, face and fingers, which made his life a misery. There were traces of albumin in the urine at first. He treated this eczema by diet and tonics, and to these it gradually yielded and disappeared. He found the heart's second sound to be reduplicated, and thought the spleen was enlarged, as he could feel it by palpation. He thought the boy might have lardaceous disease, as he was also always somewhat anæmic. He was aware of the presence of the adenoids and tonsils; but when, two years ago, an attack of acute inflammation and suppuration in the middle ear occurred, with a temperature of 104° F for four days, he incised the drum without an anæsthetic, as he considered the boy's general condition negatived it and any further operation. On being shown the condition of the intestine recently, he suggested that there might be some analogy between the effect of continued eczema on the alimentary canal and that of burns of the skin in children, which often produced gastro-intestinal inflammation. Another doctor said that he attended him with measles; when he was very ill his tonsils and adenoids embarrassed his breathing very much, and his rash was delayed until the sixth day, when it covered his face and body without any intervening areas of skin, and then his pulse was feeble and irregular.

With regard to the causation of this disease, Mr. Thomson Walker, at a discussion on "Sudden Death in Children" [10], said: "The trachea has only in the rarest instances been found compressed post mortem. The analogy with other lymphoid hyperplasias leads us to consider the pathological condition as a reaction to some chronic



FIG. 4.

Enlarged glands in mesentery.

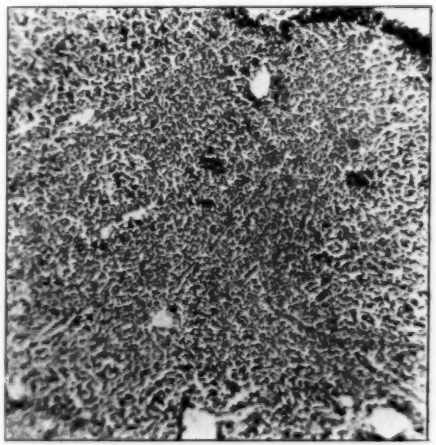


FIG. 5.

A lymphatic gland (inguinal).

toxæmia." M. Paul Gallois [3], in an article entitled "The Treatment of Lymphatism," says adenoid subjects often have a dilated stomach, and their gastric troubles are attributed to the irritation produced by the pharyngeal muco-pus which is swallowed. This may be sufficient to set up a mild state of septicæmia.

According to Dr. Farquhar Buzzard [1], in myasthenia gravis, a disease which ends in attacks of dyspnœa and respiratory failure, the central nervous system is free from discoverable lesions. Enlarged hyperplastic thymus gland was present in three out of the five cases recorded. In one case masses of lymphoid tissue resembling islets of thymus were discovered in the loose areolar tissue surrounding the pancreas, and in all cases cell-clumps of lymphocytes, designated "lymphorrhages," were visible in the muscle fibres. The apparent connexion between thymic lymphoid hyperplasia and death through the respiratory system cannot fail to arrest attention. Professor Warthin [12] relates five cases in which surgical relief and cure have been afforded in thymic stridor by partial or total thymectomy, from the episternal notch. Dr. Dudley Buxton, in a comprehensive lecture on "The Status Lymphaticus in its Relation to the Administration of Anæsthetics," delivered at University College Hospital on November 3, 1909, advised that, when operation is imperative in subjects suspected of thymic enlargement or general lymphatism, a low-strength vapour of chloroform in the presence of oxygen should be employed in preference to ether, by which all obstructive conditions may be intensified.

The vital importance of making a diagnosis of this pathological condition, especially before the administration of any anæsthetic, is shown by the events which I have described. The public has been alarmed by learning of this disease, because it has been stated that it cannot be discovered during life; and where formerly I was asked before an operation whether I would sound the heart to see if it were in a fit condition for an anæsthetic, several nervous patients have recently asked whether they might have status lymphaticus, and, if they had, would it be possible to find out?

The absence of any symptoms of internal disease during life in several of the cases described, and the difficulty of diagnosis in the present state of medical knowledge, present a problem which is worthy of the most active research; and I cannot yet feel that the existing methods of diagnosis will lead to the discovery of lymphatism in any but the most marked cases. According to the above authorities, in patients with enlarged thymus and lymphatism the skin may be pale,

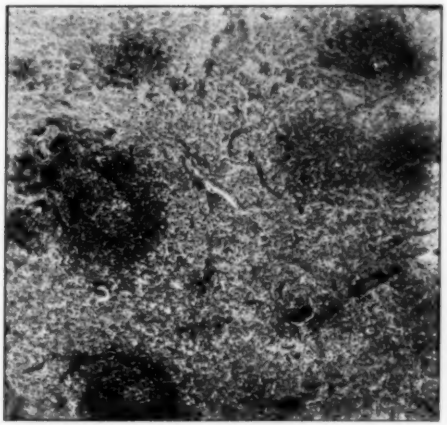


FIG. 6.

The spleen. (Low power.)

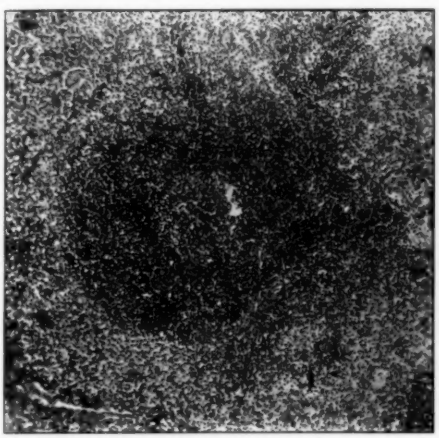


FIG. 7.

The spleen. (High power.)

pasty, or muddy in colour. There may be a history of asthmatic attacks or laryngismus. If there be stridor, not due to adenoids, it persists on closing the nares. The enlarged thymus may present as a distinct tumour in the suprasternal fossa, especially during expiration or when the head is thrown back, and may be palpated in that situation. The blood may show anæmia and relative lymphocytosis. Evidences of rachitis are present in many cases. Gastro-intestinal disturbances are not infrequent. In older patients attacks of dizziness and syncope are not uncommon. Adenoids of the nasopharynx and enlarged tonsils have been associated with a number of the cases in which enlarged thymus gland and general lymphatism have been found post mortem. The spleen may be felt below the edge of the ribs on deep inspiration. The enlarged mesenteric glands may be palpable, or, as in my case, the enlarged inguinal glands may be distinctly felt in either groin. In other cases enlarged axillary glands have been detected.

Percussion of the thymus: An area of dullness triangular in shape, the base at the level of the sternoclavicular articulation and the blunt apex behind the level of the third rib represents the thymic area; but if this extend more than 1 cm. beyond the sternal lines, it may be taken as evidence of an enlarged thymus gland in a child, or of persistent hyperplastic thymus in an adult. Radiographic examination offers the most certain method of diagnosis of thymic enlargement [11]. (Two lantern radiographs were shown.)

I have endeavoured to avoid theories and speculation, and to give you only known facts.

We desire to discover: (1) The cause of sudden death in lymphatism; (2) In what subjects to suspect lymphatic hyperplasia; (3) How to diagnose it; (4) How to treat it; (5) How to conduct anæsthesia safely in patients who suffer from it.



FIG. 8.

A normal portion of small intestine.



FIG. 9.

Small intestine from Case VI.



FIG. 10.

A normal portion of large intestine.

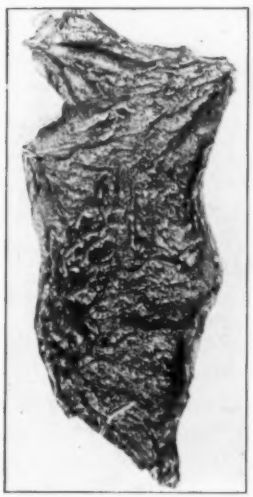


FIG. 11.

Large intestine from Case VI.

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DISCUSSION.

Dr. DUDLEY BUXTON thanked Mr. Bellamy Gardner for the extremely able manner in which he presented the details of the very sad case, as well as for the admirable way in which he had summed up the knowledge on the subject. The difficulty with which anæsthetists were faced was due to the fact that nearly all the authorities who had attempted to give a picture of lymphatism, and who had collated the pathological facts, as well as those who had described the clinical details, differed so much one from another that the mental picture obtained was one almost amounting to confusion. And that must be so, because there was not yet any clear idea as to the relation of the various pathological facts to the clinical symptoms revealed by the cases. There were the conditions known as thymic death and thymic asthma and that called status lymphaticus, and these presented different causes and different mechanisms of fatality. Many infants had very extensive hyperplasia of the thymus, and yet did not die as the result of it, and a number of people had died suddenly, presumably from status lymphaticus, in whom the thymus had not been very much enlarged. On the other hand, it had been stated that thymic death and thymic asthma had been proved to be due to pressure, because when an operation for removal of the thymus had been carried out the symptoms had been relieved. In the cases dying under anæsthetics nothing of the sort was present. Any intelligent anæsthetist would at once recognize such a degree of dyspnœa as would be caused by so enlarged a thymus. But there were also certain symptoms affecting the circulation and affecting the lymphatic system throughout the body. Hyperplasia of the lymphatic system might be dismissed, remembering that the enlarged thymus probably represented the result of some cause, and it was probably this cause which produced the sudden death. In many such cases there was a depressed circulation arising through a more or less feeble heart, frequently due to a thin wall and want of muscular tonicity, giving the clinical signs of weak, distant sounds of the heart and a lack of that peculiar click which it was customary to hear on the closure of the valves. There was also a narrowing or some other alteration in the aorta and very low blood pressure, so that the pulse was liable to collapse and disappear as the result of some slight cause, such as fright. Presumably also there were certain nervous lesions, and sometimes there was associated epilepsy, chorea, and general nervous deficiency. He believed that a condition closely associated with lymphatism was seen in Graves' disease; in these cases there was a tendency to sudden death, even when it was certain that an overdose of the anæsthetic had not been taken. The thymus often persisted in these patients, but he, or more often she, suddenly died with the celerity which was associated with death from the status lymphaticus. None of the conditions he had mentioned caused sudden death, except in so far as they might depress the general vitality of the individual. It had been suggested, and, he thought,

reasonably, that the presence of hyperplasia throughout the lymphatic system was associated, as in kindred states, with a liability to the production of toxins. It seemed most consistent to believe that a toxæmia existed in such people, producing anæmia, a difference in blood pressure, enfeeblement of the heart, and a tendency to quasi-degeneration in the myocardium, leading to that peculiar feebleness of the nervous system, laying it open to attacks from peripheral causes, such as slight pain and fright, causing inhibition, and the inhibition was fatal owing to the inability of the tissues to recover themselves. It had been shown many times that an anæsthetic by itself was not competent, even in the lymphatic state, to produce death. Within the year the case was examined in Brompton Hospital of a boy of tender age who died under chloroform, and the lesions found were those of the status lymphaticus. It was shown that the boy had been subjected to nine or ten operations previously to the one during which he had died, so that clearly in all cases the anæsthetic was not the predominant factor, though possibly a determining one. With regard to how those people should be dealt with, Mr. Gardner had pointed out the difficulty of arriving at an absolute diagnosis, but he (Dr. Dudley Buxton) thought one could arrive at a knowledge of the general vitality of the individual; and, given a tonsillar growth and enlargement of the glands, one was justified in presuming the presence of some grave dyscrasia, probably status lymphaticus, and one was bound to give the patient the benefit of the doubt and treat him as if he were subject to that condition. As he said in the lecture to which Mr. Gardner had alluded, he thought the danger in those cases arose when the anæsthetic was the determining factor in the death, from the inability of persons the subjects of lymphatism to take with impunity the ordinary doses of the anæsthetic. It had been said that such persons went under the influence of the anæsthetic with great celerity, and required very little to maintain anæsthesia. Dr. Munro was opposed to that view, for he had stated that in the case of nitrous oxide he believed that people who had the status lymphaticus were very insusceptible to the influence of the gas; they would occasionally take enormous quantities without becoming anæsthetized. He (the speaker) did not know whether that was true, but it was not in accord with his own experience. But as far as chloroform was concerned, the question of dosage was a most important matter for them. He believed children in the status lymphaticus were less liable to dangerous consequences when they had a smaller dose given them than was required by the ordinary patient. He recognized it might be said that the danger in those cases arose in light anæsthesia at the commencement of the induction period. He drew a distinction between "incomplete" and "light" anæsthesia. He did not advocate incomplete anæsthesia in those cases, for he regarded that as at all times very dangerous, while light anæsthesia was not dangerous; indeed it was safer for these people than the ordinary range of anæsthesia. He dissociated that remark from the case which Mr. Gardner had brought forward, because he felt no doubt that the anæsthetic was given on that occasion with great skill and judgment, and probably the boy could not have had a better chance than

was given to him by Mr. Gardner. He (Dr. Dudley Buxton) believed that deaths occurring from status lymphaticus under an anæsthetic were really deaths due to natural causes, probably accelerated by the anæsthetic. The subject was of such grave importance and far-reaching interest that the Section would do well to form a sub-committee whose duty it should be to collect all published cases, and the many cases which had been recorded but not published, which were germane to the present discussion, and also to collect pathological findings. The committee in due course should present a report to the Section. And if the report were found to be of sufficient value, it should be tendered to the Royal Society of Medicine for inclusion in their *Proceedings*. He would, with the President's permission, bring that forward as a resolution at the close of the present discussion.

Dr. SPILSBURY said that the admirable paper and specimens made it scarcely necessary for any remarks from him from the pathological point of view. But as he had had unusual opportunities of investigating cases of death under anæsthetics, and as he had met with cases of death from the status lymphaticus other than those under anæsthetics, he would emphasize certain points which had been brought forward. He laid great stress on the need for taking all the changes together. Too much had been made of the fact of the thymus being enlarged; sometimes that organ had not been present; what had been regarded as thymus had been fatty tissue replacing the thymus. In one case which came under his notice subsequently, tuberculous enlargement of the lymphatic glands was present, and this condition had been mistaken for the status lymphaticus. The necessity for a thorough microscopical examination of the tissues in each case was very great. A skilled pathologist could recognize the changes by a naked-eye examination, but those who had only seen a few such cases could scarcely be expected to do so. He was certain that many cases of so-called status lymphaticus were not that condition. It was very difficult to say how much thymus-tissue was present, and therefore the records as to the measurements or weight of that tissue should be placed side by side with the record of the microscopical examination. A very important point in the diagnosis of the condition was the change which took place in the heart muscle. In all cases he had found a certain degree of degeneration; in the well-marked cases he had seen a surprising degree of fatty degeneration or brown atrophy, and frequently a combination of the two. This would be found much more often associated with the status lymphaticus than had previously been recorded. He had made post-mortem investigations in four cases in which exophthalmic goitre with enlarged thymus was present. But all the four cases in which those conditions were present were on the borderline of status lymphaticus. The diagnosis was a matter of extreme difficulty, though he could not speak from the clinical standpoint. It had recently been said that where there was obvious lymphoid hyperplasia in the tonsils and tongue the case must be regarded as status lymphaticus. In that regard he was sure there had been exaggeration. In the post-mortem room cases were seen which died

of other diseases in which tonsillar enlargements and adenoids were present, with excess of lymphoid tissue at the base of the tongue. It was only in a small proportion of such cases that the status lymphaticus existed, and therefore the very large proportion of cases which were successfully anæsthetized could not be congratulated on having had a very narrow escape. All that could be said about the essential pathology of the condition must be a matter of speculation. It was certainly a form of toxæmia, and apparently represented a continuance of the condition present in early life. He was not sure that in some the status lymphaticus did not develop subsequently to childhood. In some of the specimens he had examined he found the original thymus tissue somewhat atrophied, side by side with growths of lymphoid tissue of more recent date. He specially noted in a man aged 24 who died under an anæsthetic that that change was very marked. In the central part of the gland Hassall's corpuscles showed degenerative changes, but in the outer parts of the gland there was a complete absence of the corpuscles, and the tissue showed only hyperplastic lymphoid structures. In the lymph-glands there was much more difficulty in obtaining evidence of subsequent enlargement. Of course everybody, including anæsthetists, must realize that the status lymphaticus was not confined to cases of deaths under anæsthetics. Those who were constantly making post-mortem examinations found it not infrequently in other conditions; he had seen it in deaths from shock following slight injuries and in deaths following comparatively trivial infections, in none of which could the death be attributed to the injury or infection. Deaths under anæsthetics also occurred where the status lymphaticus was not present. Seventy-five per cent. of deaths investigated by him had belonged to that class, and in 25 per cent. status lymphaticus had not been present. In only one case of death under an anæsthetic had he found that there was no disease and no status lymphaticus. It was therefore important to remember that status lymphaticus was not a condition which had been invented to cover the misdeeds of erring anæsthetists, but was a pathological entity, and played some obscure but important rôle in the causation of death—a much more important one than the anæsthetic itself.

Dr. HEWITT said he came with the idea of learning from the discussion. He congratulated the Section on the very valuable and almost classical address which Mr. Bellamy Gardner had delivered. All would benefit largely by the contribution. He agreed with Dr. Buxton that it fully deserved the attention of a special sub-committee. Medical men seemed to be face to face with something new, and it was for them to ascertain whether the new factor was of itself responsible for sudden death. Without throwing out the slightest hint or suggestion, he thought that before admitting that that pathological entity could cause death under anæsthetics, they should try to satisfy themselves that the deaths which occurred could not be explained on any other ground. He did not say that they could, but there were certain considerations worth discussing. It seemed to be generally recognized that there were certain

anæsthetics under which that accidental death was exceedingly uncommon. If that were so, one might say that certain anæsthetics predisposed to sudden death when the particular pathological condition was present. Then he thought they should ascertain—and that was a point for the pathologists—what proportion of the cases examined post mortem had evidences of lymphatism, but in which death had taken place from causes altogether apart from the presence of that lymphatism. He referred to cases in which patients had died from accidental or other causes in which that lymphatic condition obviously played no part. So far as he knew, there seemed to have been no attempt in that direction, but it would be a very important channel of inquiry. Another fact which required attention was that in many of the cases in which patients had died suddenly an asphyxial condition had immediately preceded death. There could be no doubt that one met with cases in which a very slight auto-asphyxial factor was sufficient to cause the most dangerous and apparently syncopal states. Certainly many of the cases which had the typical pathological conditions, which had been so admirably described by Mr. Gardner and Dr. Trevor, were cases in which anæsthetics were especially likely to give trouble from the respiratory side. The conditions in the upper air-tract, in the nasopharynx, tonsils, thymus and bronchial glands, predisposed to alterations and modifications of respiration which might quickly, in certain types of patients whose vitality had been lowered and who were susceptible to anæsthetics, lead to a fatal issue. Moreover, the stomach of a patient with lymphoid states about the upper air-tract almost invariably contained swallowed mucus, and this was liable to set up reflex circulatory depression quite early in anæsthetization and so to complicate anæsthesia. But he was absolutely opposed to the view that the heart, *per se*, had any important share in deaths from anæsthetics, whether those deaths came about in the way which had been described or in any other way. No doubt the heart did become affected by anæsthetics in the final stages of anæsthesia, but patients with all sorts of degenerative and valvular heart affections, and even those with hardly any pulse at the wrist, usually passed through anæsthesia if properly conducted. That fact made one hesitate to attribute any large share to the state of the heart muscle. He was quite unbiased and open-minded in the matter, but his view was rather that, provided the respiration were kept absolutely free, that the anæsthesia were not too profound, and that the influences of posture were fully understood, even though the status lymphaticus were present, it was possible, in the vast majority of cases, to avert accident. It would be very unfortunate for the progress of surgery if the public had the idea that they were liable suddenly to die, either at the dentist's or elsewhere, because they might be the subjects of that condition. He hoped that we might be able to see our way to the proposition that, with due care and recognition of the dangers, the anæsthetic could be so given as to make the status lymphaticus negligible. It was, of course, conceivable that they were face to face with an altogether new factor, but no conclusion should be come to until the ordinary factors of death had been excluded.

Dr. HORACE MANDERS said that at the East London Hospital for Children there was, last week, a case of the disease, and he had brought the notes to show the result of a careful post-mortem examination. There was an advantage in investigating the condition anew, without preconceived ideas or being biased by anything which had been written years before. This case presented unusual features, because the child did not die until thirty-six hours after the administration of the chloroform. The boy was aged $4\frac{1}{2}$, and was admitted on November 25 last. On the next day chloroform was given on lint. He was soon sick, and defæcated on the table, in spite of the usual precautions. (In Mr. Gardner's case, just before death, some fluid oozed from the mouth; cyanosis occurred, and the respiration ceased.) The operation had not been commenced. The administration of ether and brandy, and carrying out artificial respiration for several minutes, brought him round. Hot fomentation at the precordium caused a burn. He was put back to bed and saline administered *per rectum;* consciousness was restored. He talked to his parents and seemed comfortable, but he suffered greatly from thirst. The pulse was feeble, but caused no anxiety. The evening temperature was 102° F., pulse 140, respirations 32. On November 27, in the afternoon, the pulse gradually failed, and death occurred. The breath did not smell sweet. The boy had an attack of whooping-cough six weeks ago, beyond which the medical history was healthy. At the post-mortem the body was found to be well nourished, but a burn was found on the left pectoral region. Superficial glands not enlarged; liver not palpable. The slide shown represented the thymus, which was enlarged and extended over the pericardium, down to the diaphragm. It weighed $12\frac{1}{2}$ grm.; no tracheal compression. It had not yet been examined microscopically, so he could not say how much was gland-substance and how much was fibrous and fatty. The tonsils were not unusually large; lungs were normal; aorta small, with patches of atheroma which existed also below the aortic valves in the heart. The spleen was not enlarged; mesenteric glands large and discrete; gall-bladder distended; pancreas normal. The follicles of the duodenum were definitely enlarged, also those in the small intestines. Altogether there was tremendous hyperplasia of the glandular structure. It was very strange that the death did not occur until thirty-six hours afterwards. Had the burn anything to do with that? In two cases mentioned by Mr. Gardner eczema occurred. He (Dr. Manders) believed that lymphatism was a toxæmia, but it was not known to what toxin it was due. The coccus which produces the toxæmia to which rheumatism is due was not known until a few years ago, and some were not certain of it even now. The toxæmia did not merely affect the heart, but also the respiratory centre in the medulla. Electro-therapeutists examined many children for physicians where there were enlarged bronchial glands, but only in very few cases was an enlarged thymus seen. With the new methods of taking flashes and fixing one's attention on what was expected to be seen, more could sometimes be seen than even with a prolonged exposure. Of course in many cases there were large bronchial glands in plenty. Where a child had some specific

history, or had suffered from chronic eczema, it should be subjected to X-rays before operation, to see if possibly there was any sign of enlarged thymus. That was the only way, so far as he could see, in which any diagnosis could be made.

Dr. LEONARD WILLIAMS desired to add his word of admiration for the paper, and said the matter was one of very considerable interest to him. He thought the condition called status lymphaticus was obviously due to some want of balance between the internal secretory glands. The normal condition was maintained in a state of equilibrium by the antagonism which existed between certain of those glands. It was known that the ovary and suprarenal, the ovary and the thyroid, the thyroid and the pituitary, the thyroid and the pancreas, exercised certain definite physiological antagonisms to one another. And it was to be supposed that when one of those major glands was thrown out of gear or was undeveloped, the work of that gland was taken up, so far as it could be, by certain minor glands. In the status lymphaticus those minor glands were enlarged, doubtless because they had to do some extra work. In status lymphaticus he believed the major gland chiefly at fault was the thyroid, and that lymphatism was due to want of competency on the part of this gland. Several descriptions of the condition under discussion had appeared in American and French literature, and the meeting had just heard much that had been written on it in English literature. Although there were certain points of disagreement, there were many points of agreement in these descriptions. The points of agreement were such as to constitute a very serious indictment against the thyroid gland. A recent writer in the *Lancet* (Dr. Munro)[1] instanced what he considered were the conditions which constituted the status lymphaticus clinically, and every one who had studied the question would agree with Dr. Munro's views. In the time at his disposal the speaker could refer to a few points only. The first was a slow and unexcitable pulse, seldom reaching 70, frequently only 60, and unresponsive to the ordinary heart stimuli. The next was an obstinately subnormal temperature—i.e., 97° F., or 96° F.—which would often refuse to rise even under the influence of acute specific diseases such as the exanthemata. Those two symptoms were present in myxœdema and cretinism. Next was the condition of the thyroid gland as found post mortem. In some cases the gland was shown to be enlarged, and in others it was either normal or atrophied. When enlarged it was generally a pseudo-hypertrophy, having been composed largely of fibrous tissue. But whether the gland was enlarged or atrophied was of no importance to the argument, because simple goitre, which was the result of an increased call by the organism, was often curable only by the exhibition of thyroid extract. Another question of importance, more especially from the anæsthetist's point of view, was the question of chloroform. The cases of lymphatism which had died had had only small doses of chloroform or other anæsthetic. Alcohol, chloroform, and

[1] *Lancet*, 1909, ii, p. 1468.

ether had many features in common, and Dr. Buxton had instanced in his speech the case of people who were supposed to be affected by very small doses of drugs, especially anæsthetics. Of a large number of people who were intemperate in the use of alcohol it was said that very little affected them. Such people were always sub-thyroidic, and if anyone doubted the power of the thyroid in dealing with poisons, whether microbic or other—and the Section was at present concerned with others—he invited him to experiment upon his own person, by taking ½ gr. of thyroid extract three times a day for three or four days. At the end of the third day he would find himself able to take a quantity of alcohol which, under ordinary circumstances, would surely intoxicate him.

There was only time for a brief reference to one other point—namely, the question of adenoids. It was for the removal of adenoids that the fatal operation, when it had been fatal, had usually been undertaken. It seemed necessary to insist that adenoids did not really represent a condition of mere "cussedness" on the part of the organism, but rather an enlargement of purely normal structures. Adenoids, when not too large, could be made to regress by means of thyroid extract. No doubt when they were large enough and diseased enough to cause concomitant symptoms, they should be removed surgically. That brought him to the point upon which he wished to insist, which was that, if it were a rule of practice that adenoids should not be removed until the patient had been treated with suitable—that is, with very moderate—doses of thyroid extract for at least a month, and if anæsthetists would take upon themselves the responsibility of refusing to give an anæsthetic to such cases unless this rule had been complied with, he believed the vast majority of cases of lymphatism could be subjected to an anæsthetic not only without a fatal result, but without giving rise to any cause for anxiety during its administration.

Section of Anæsthetics.

February 4, 1910.

Mr. RICHARD GILL, President of the Section, in the Chair.

Discussion on "Lymphatism."[1]

Mr. CARTER BRAINE said he had been requested to read the following communication, because it seemed to bear on the subject of the discussion. It was not status lymphaticus. Though he had been giving anæsthetics for twenty-four years he had never come across one of those cases.

Cardiac Failure, Respiratory Failure, associated with Engorgement of the Thyroid Gland.

Patient, female, aged 24; very frail, mere skin and bone; extremely nervous, with a rapid pulse; had been under the care of many gynæcologists. Operation for removal of an abdominal tumour. The administration was commenced with 4 c.c. ethyl-chloride and continued with ether on an Ormsby inhaler. The induction was quite normal, and the patient was wheeled into the theatre and placed in the Trendelenburg position at once, prior to cleaning up, &c. The abdomen was opened; the operator then complained that the recti were a little rigid. The pupils at this period were dilated and a light-reflex present. Whilst endeavouring to obtain abdominal relaxation by means of a deeper anæsthesia I noticed the pulse was becoming feeble, the pupils still very large, and a doubtful corneal reflex present. There had been no traction on the abdominal contents. The condition proved to be an enlarged ovary, fixed down by a broad adhesion to the pelvic wall—a perfectly straightforward and easy case. Respiration became very shallow, the anæsthetic was stopped, and chest compressions employed to assist respiration. Breathing, however, ceased; there had been no cyanosis, no stertor; the colour was ashy-grey, and the pulse imperceptible at the wrist. Artificial respiration with patient on the table was not satisfactory, so the abdomen was covered with cloths and the patient lifted on to the floor. Pupils were now enormous in size and there was no corneal reflex. Haward's method of artificial respiration was employed, with little benefit; oxygen with artificial respiration was tried; artificial respiration was persevered

[1] Adjourned from December 3, 1909.

with, although the patient was considered by all present to be dead; the oxygen was discarded, as it did not appear to be of any use; the tongue was well forward and had given no trouble.

I now noticed what I am absolutely certain was not present either when the anæsthetic was commenced or when the patient was on the table—a swelling of the thyroid gland, the two lateral lobes being well marked and the isthmus, about two fingers broad, connecting the lobes over the front of the trachea, was also conspicuous. Considering this enlargement of the thyroid was in some way connected with the inability to restore natural respiration, the head was raised from the floor and slightly flexed on the sternum, in order to relax the sterno-mastoids and so relieve any pressure on the enlarged thyroid, and artificial respiration continued in this position. In a few minutes the pupils commenced to contract, a thready pulse returned, and respiration, with a little assistance every now and then, became re-established. Strychnine was injected hypodermically, the patient placed on the table, and the operation completed, under light anæsthesia, in the horizontal position. The thyroid swelling soon disappeared, and when the patient had returned to her room the colour was good, pulse good, and no trace of the thyroid could be discovered, the rings in front of the trachea being felt with ease.

The entire proceedings occupied thirty-five minutes, most of which was spent on the floor resuscitating the patient. There was never any cyanosis. The cardiac failure commenced fifteen minutes after induction of anæsthesia.

This case is, I consider, conclusive evidence that the thyroid gland, under certain conditions during an operation, can become so engorged as to interfere with respiration—an extremely rare incident, I admit, but yet it leads one to suppose that in status lymphaticus—a condition in which the closely-allied thymus gland is enlarged — it may also become engorged under similar conditions, and from its position behind the sternum, overlapping important structures at the root of the neck, be more likely to give rise to reflex cardiac inhibition or respiratory stasis, the cause of which would probably be overlooked.

In answer to Dr. Dudley Buxton, Mr. BRAINE said there was no prominence of the eyes, the pulse came down to normal, and there was no tachycardia afterwards.

Dr. FARQUHAR BUZZARD: When Mr. Bellamy Gardner called upon me a few days before reading his paper on "Lymphatism," with a request that I should tell him all I knew about the enlargement of the thymus gland in "myasthenia gravis," he only reawakened an interest which I had often felt in the possible connexion or relationship between these two morbid conditions. This is the only excuse I can offer for my appearance before you this evening and for my temerity in attempting to open the adjourned discussion of Mr. Gardner's paper. Before entering upon my task may I add my tribute of unstinted admiration for the way in which Mr. Gardner introduced his subject? He realized that the value of a discussion such as this would lie in the opportunity it afforded for collecting reliable facts and records of varying experiences,

and he refused to be tempted into airing obvious theories which most of us can propound and which none of us can properly substantiate. I shall endeavour to follow his most excellent example.

The first point at which the two diseases—lymphatism and myasthenia—appear to touch is a clinical one. I refer to the mode of death. Cases of myasthenia terminate fatally in various ways: some die of asphyxia due to the obstruction of food; some die of an intercurrent disease; some die, after many attacks of dyspnœa, from gradual failure of the respiratory musculature; finally, there are those who die suddenly. It is with the latter we are most concerned for the moment. Mailhouse records the case of a child, aged $2\frac{3}{4}$, who presented all the signs of myasthenia, but who had had no attacks of dyspnœa. One evening, after being up and about, he had a little difficulty in drinking some coffee from a cup; his head dropped and he was dead. Clifford Allbutt mentions the case of a girl, aged 18, whose symptoms were regarded as hysterical. One morning she went into her mother's room, fell to the ground, was convulsed, turned blue, and died. Such cases could easily be multiplied if it were necessary, but we shall all agree in regarding them as showing a strong superficial resemblance to many of the recorded cases of fatal lymphatism. After a careful study of the accounts given in the literature, I feel sure that there is evidence to show that, although many of these myasthenic patients die from sudden cessation of respiration only, there are others in whom the heart fails at the same time and with equal abruptness. We are confronted, therefore, in both diseases with instances of sudden respiratory, or cardiac and respiratory, failure, either quite spontaneous or dependent upon some exciting cause which is too trivial to explain their origin. In the one disease there are generally previous symptoms which, from accumulated experience, warn us of the possible occurrence of sudden death; in the other disease we have not as yet learned to recognize the signs by which such a calamity can be foreseen. Under these circumstances, it is not surprising that an excessively common event, such as the induction of anæsthesia, may be occasionally, among other events, the trivial exciting cause of a fatality. A more careful routine examination of patients before operation, and perhaps a more general use of radiography for that purpose, may afford us in the future those clues for the detection of lymphatism which we still lack.

Let me now refer to the second point at which the two diseases come into contact. For twenty years after the clinical symptoms of myasthenia had been recognized the disease was without any defined morbid anatomy, owing to the natural tendency of investigators to direct their attention almost entirely to the nervous system. During the last ten years it has been shown that myasthenic symptoms are constantly associated with certain morbid changes chiefly in tissues outside the brain, spinal cord, and peripheral nerves. These may be briefly summarized in the following way: In the first place, scattered through many of the tissues and organs of the body, and especially in the muscles, are to be found small clumps of lymphoid cells, which I have termed "lymphorrhages," and which have the appearance of having escaped from the lymphatic

circulation. In the second place, a careful examination of skeletal muscles reveals a varying number of fibres undergoing degeneration. This degeneration may be slight, amounting only to slight hyaline or granular change, or may be more marked, resembling the atrophy of the early myopathies. In the third place, there is present, in probably over 50 per cent. of cases, a thymus gland developed out of all proportion to the age of the patient. Some of these thymus glands are merely hyperplastic, but others have been described in which there is evidence of neoplastic changes, lympho-sarcomata and other tumours having been recorded. I have never maintained that any one of these changes are characteristic of myasthenia, or that they yield a definite clue to the pathogenesis of the disease; but, taken together, they are sufficient to confirm a diagnosis based on clinical symptoms. I am not even disposed to affirm with complete confidence that their absence would invalidate a diagnosis made from observations during life. On the other hand, their discovery emphasizes the importance of a minute microscopical examination of unsuspected tissues, and suggests the possibility that in other obscure diseases, such as lymphatism, there may yet be found important evidence of the essential morbid process at work.

The anatomical analogies between lymphatism and myasthenia are not really very striking if we remember that an enlarged thymus is not a constant feature of the latter, and if other observers have found as little evidence of general lymphatic hyperplasia as I have in carrying out the examination of nine or ten cases. It is interesting, however, to note exceptions to this experience, and of these the case recorded by Hödlmoser is one of the most remarkable. He describes a girl patient with typical myasthenic symptoms who died from respiratory troubles and who was found post mortem to present the features of status lymphaticus—a large thymus, prominent follicles at the base of the tongue, and hyperplasia of the lymphatic tissues of the intestines. There are instances of abnormalities of the spleen, the thyroid, the pituitary, and even the liver, but the indictment of these organs has never been so severe as that of the thymus. I am sorry that I cannot offer you to-night any reliable information on the question as to whether there is any relationship between the presence of an enlarged thymus and the particular mode of death in myasthenic patients. An inquiry of that sort should certainly be conducted by any committee appointed to report on the subject of lymphatism.

At the end of his paper Mr. Bellamy enumerated five questions for which we are seeking answers: (1) The cause of sudden death in lymphatism; (2) In what subjects to suspect lymphatic hyperplasia; (3) How to diagnose it; (4) How to treat it; (5) How to conduct anæsthesia safely in patients who suffer from it. I would suggest some amendment of the order and, to some extent, of the substance of the questions: Is there a pathological condition to which the name lymphatism may reasonably be applied? How can it be recognized during life and after death? What is the cause of the sudden death? I put the question of the cause of sudden death *last* instead of first, because I venture to think it is one of the biggest questions which can be asked in the whole of pathology, and one which concerns many other morbid states besides

that of lymphatism. Sudden, unexpected death which cannot be correlated with any definite cause in the post-mortem room is not an uncommon event in patients suffering from diverse conditions. We can recognize the anatomical characters of their diseases and of their diseased organs, but we are sometimes apt to forget that we are quite unable to give any satisfactory reason why at one moment they were walking about little different from what they had been any time during weeks or months, and the next moment they were dead.

We must first of all establish a recognizable symptomatology and morbid anatomy for lymphatism, if it deserves them, and leave the mystery of sudden death, whether it occurs in lymphatism, myasthenia, Graves's disease, or malignant growths of any part of the body—to mention only a few of the conditions which occur to me—to be solved by that clearer insight into pathological processes which we may reasonably look forward to in the not very distant future.

Dr. BLUMFELD said it would be very difficult for anyone, and impossible for him, to add any facts to those which had been brought forward. That discussion would, it was hoped, help them to arrive at one of the desiderata mentioned by Dr. Buzzard. It had formed a better standard of what should be shown post mortem by any case which had been christened status lymphaticus. Those who read the account of deaths during anæsthesia which hitherto had been classed as due to status lymphaticus must have been impressed by the poor way in which many would coincide with such a standard as was now being set up. Many of the cases had no more met the pathological standard of Mr. Bellamy Gardner's case than had many cases described as deaths due to fatty heart met the microscopist's demands. Speaking purely as an anæsthetist, he thought they would make a considerable step if they became stricter in what they regarded as the status lymphaticus. Another point concerned the part which the anæsthetic played. There were two legitimate causes of death before them when the fatality occurred during the performance of only a slight operation: one, a condition which had been shown even when unconnected with anæsthesia to be able to produce death; and the other which, as anæsthetists, they knew could cause death—namely, chloroform inhalation. When cases did not meet the demands of the status-lymphaticus test, they were driven to regard them as chloroform deaths. That led one to question the part which the anæsthetic played in such cases, and whether there was any difference in the anæsthetics. Mr. Gardner had favoured him with the references to those six ether cases to which he referred, and on reading those cases one was driven to the conclusion that there was not a *bona-fide* case of status-lymphaticus death under ether recorded. Probably but few cases of real status lymphaticus had been operated upon under ether, so that we could not infer its greater safety. The best recorded one he could find was one of exophthalmic goitre and status lymphaticus. The former condition, of course, had tendencies of its own, apart from the status lymphaticus, to lead to a fatal issue.

Dr. RALPH VINCENT said that he had been greatly interested by the admirable paper read by Mr. Bellamy Gardner. He was surprised that in the

course of the discussion so much stress had been laid on the coincidence of death with the administration of an anæsthetic. As Dr. Farquhar Buzzard had pointed out, the factor that determined the moment of death was but little known, and, in this case, he (Dr. Vincent) would attribute the immediate causation of death to the operative procedure rather than to the anæsthetic. In these cases shock appeared to be the critical factor, and a probable cause of this was the surgical interference with the sensitive region of the nasopharynx. So far from making the anæsthetic responsible for the fatal issue, he was disposed to think that this protected the patient rather than otherwise; for chloroform certainly tended to minimise surgical shock. One of Dr. Thursfield's cases was especially interesting in this respect, for the child suddenly sat up and died within thirty seconds. If an anæsthetic had been administered in this case, no doubt the administration of an anæsthetic would have been considered as in some way responsible for the death. Certainly, in an infant in whom convulsions were imminent a digital examination of the nasopharynx would be quite likely to provoke an attack. Sudden deaths in infants and young children raised some extremely important questions. He would refer briefly to one case of death from convulsions. The infant at birth was quite healthy, and for the first two months did exceedingly well. At the end of that time the mother adopted methods of feeding which were very inadequate, and he pointed out to the relatives that if this course were persisted in, death from convulsions was likely to ensue. But the mother was very satisfied with the "progress" of the infant and would not listen to any remonstrances. When the infant was eleven months old it was seized with convulsions and died in the course of an hour. Many such cases were seen, and they were of great interest from the chemical and physiological point of view, because they showed clearly that sudden deaths could be caused solely by the toxic conditions arising from improper feeding. Another example of toxic conditions was acute fat intoxications. This was particularly liable to occur in infants recovering from atrophy. The symptoms were quite characteristic and were well known at the Infants' Hospital. In a typical case the infant suddenly showed a marked distaste for food, the temperature rapidly rose to 103° or 104° F., vomiting was generally present, and constipation was marked. The infant was generally extremely drowsy or comatose, and the general appearance and symptoms suggested meningitis. If the symptoms were left untreated for twelve hours the infant would probably die. Both these toxæmic condition showed clearly the perversion of metabolic processes arising from dietetic errors.

In regard to the case under discussion, he had no hesitation in saying that rachitis was the essential feature of the structural alterations. The adenoids, the enlarged tonsils, the enlarged spleen, and the general overgrowth of the lymphatic structures all pointed strongly in this direction. The essential feature of rachitis was that it was a disease of growth. It was not caused merely by an insufficiency of diet. A mere deficiency would probably result in atrophy. But if an infant were fed with a food deficient in fat and proteins, but containing a large quantity of carbohydrates, then all the essentials for the

creation of rachitic structure were present. The whole case spoke to him of malnutrition in infancy and early childhood. The chronic eczema was additional evidence of this. He did not think that the patient had been fed during infancy for any considerable period on pure breast milk of good quantity. Probably within three or four months of birth the infant was fed on boiled milk, with carbohydrates, foreign to milk, added to it. At any rate, that was the typical kind of food likely to produce the pathological conditions that had been described. In searching for the cause of structural alteration it was essential that the fullest consideration should be given to the materials from which that structure was created.

In reply to the President, Dr. VINCENT stated that the diagnosis of fat intoxication was made from the striking combination of symptoms which were quite characteristic and were corroborated by the results of treatment. The extremely urgent symptoms were removed by a dose of castor oil and extremely dilute food (chiefly water) for the next twenty-four hours.

Dr. A. G. LEVY said he had been asked to communicate to the Section an account of a few experiments he had made bearing on the subject under discussion. The condition of the thymus gland was so prominent a feature of status lymphaticus that the possible relation of a thymus secretion to sudden death under chloroform had to be considered. A fresh extract of sheep's thymus gland was made and injected both into the peritoneal cavity and subcutaneous tissue of cats under chloroform. Beyond a slight fall of blood-pressure these animals did not react to the anæsthetic in any unusual way, and stimulation of the central and peripheral ends of the cut vagus nerve did not produce any greater effect, direct or reflex, after the injection than before. He had also fed a number of animals both with the fresh gland and with the commercial dried extract of gland, making observations in those cases more especially in relation to the induction period of anæsthesia, comparing results obtained before feeding with those obtained in the same animal after feeding. That series of experiments also failed to afford any conclusive evidence that the contents, or the hypothetical secretion, of the thymus gland was operative in the production of syncope under chloroform anæsthesia.

Mr. R. E. HUMPHRY read the following communication, and, in reply to the President, he said he meant, by saying that death might be explained on psychical grounds, that slight afferent impulses or shocks, caused either by painful or mental impressions, might, in the case of all the nerve centres in the medulla being in a devitalized and moribund state—and there seemed reason to think that they were functionally most imperfect—be all that is necessary, while in such a vulnerable condition, to give rise to a sudden cessation of the functions of the contained cardiac and respiratory centres, with the result sudden death :—

On the evening of Friday, December 3, I was greatly interested in listening to the very able and comprehensive paper read on this subject by Mr. Bellamy Gardner. I am afraid I must plead guilty to having written several articles wholly in favour of lymphatism as a "pathological entity," and when doing so I

have been cognizant of the fact that there are medical men of much repute who hold different views. I have to plead that it is my convictions that have urged me to do so, and which constitute my only excuse. Moreover, unfortunately, I have not been able to learn their views, and my humble contributions have ostensibly been set forward as suggestions. I have not been so sanguine as to expect them to be accepted as established facts, but I have felt that lymphatism veritably presents a most serious and important problem, and I had at least hoped to meet with some opposition, and to stir up a discussion on the subject on the part of men who were better able to deal with it than I am. I have heard it stated that a disease that cannot be recognized during life, and only with difficulty after death, cannot be accepted as a cause of sudden death until it has been made much more clear, and that one is particularly sceptical when it is given as a cause of death under an anæsthetic. Some are inclined to think that it would be an easy matter to draw many difficulties into the status-lymphaticus net, and from such an opinion I am bound to dissent. Personally, I consider that the error would be more often on the side of not attaching enough significance to the disease. Concerning the diagnosis, some twelve months or more ago I wrote a paper to the *Lancet*[1] endeavouring to point out that all the signs and symptoms existing then were practically valueless. Now, after having physically examined further cases during life, and subsequently made post-mortem examinations on the same after death, I think, under certain circumstances, a positive diagnosis may be arrived at clinically, even without the very valuable additional aid of radiography. I would base my diagnosis, personally, on the following five physical signs in order of importance :—

(1) Signs of a persistent thymus gland, as evidenced by a lowering of the upper border of superficial cardiac dullness in the absence of emphysema of the lungs, or old tubercular cavities at its bases, with or without fullness of the episternal or interclavicular notch.

(2) A uniform prominence of all the papillæ of the tongue, especially the circumvallate papillæ, between which and the epiglottis posteriorly there is frequently present a great multiplication, hypertrophy, and hyperplasia of all the lymphatic nodules, giving rise to a very characteristic picture, some of the sessile nodules measuring as much as $\frac{1}{8}$ in. to $\frac{3}{16}$ in. at their bases. The condition cannot be seen without the aid of forehead and laryngoscopic mirrors. In addition, the tongue is often unusually large, flabby, and tooth indented.

(3) A symmetrical enlargement of the thyroid gland.

(4) Distant and muffled heart-sounds with absence of the sharply defined characteristic click caused by closure of the valves associated with a soft, weak and ill-sustained radial pulse.

(5) A greater or less amount of hyperplasia of any or all of the faucial, pharyngeal, and lingual tonsils, also of the uvula, and the presence of adenoid growths in either very small or large quantities.

The co-existence of (1) and (2) is, in my opinion, sufficient evidence of the presence of the disease. I have to confess that the first is an original physical

[1] *Lancet*, 1908, ii, p. 1870.

sign of my own coining, but, despite this, I feel certain that if any of you here present would be kind enough to remember and test it, you will find it a reliable one. I am sure that the presence of the thymus gland does not give rise to the dullness one would expect it to do. This region may even be a little hyper-resonant, and if dullness is present it is probably due to other causes, possibly an aneurysm of the arch of the aorta, a gumma, a tubercular abscess, or one of the rare malignant tumours of the thymus, but not to a simple hyperplasia of the gland substance. The thymus, although a solid organ, is of much less solidity than that of the heart, and, by interposing itself between it and the anterior chest wall, it pushes the heart further away from the manubrium, depressing the upper level of absolute cardiac dullness. The "large" size of the thymus is not an essential part of lymphatism, but in instances where the dimensions of the gland have been extraordinary, and have, during life, given rise to pressure on the trachea and big blood-vessels, it is probable that a certain degree of dullness to percussion may have been caused by it; but here even I should expect to place a greater reliance on the still further depressed limit of superficial heart dullness. In the remarkably resonant chests of infants at the end of the second year of life, when the gland should have attained about its maximum size and development, no really appreciable dullness is present, and after sudden and unexpected deaths from lymphatism have occurred in adults, post mortem I have not not been able to detect any. Age will not help one much in the diagnosis, since fatalities have been known as young as one year, or even younger, and as old as 50, though probably they are commonest between 12 and 22. The presence of anæmia is common enough apart from the condition, while an "apparent" degree of the same is not always concomitant. Some observers have noted a diminution in the number of lymphocytes, but add that the change is neither great nor constant; they note also a reduced percentage of hæmoglobin. Perhaps males are affected rather more than females, and the subjects of the disease are said to be peculiarly liable to fright. True, fright may result in sudden death, but, as a phlegmatic disposition is much more common than a neurotic one in such cases, I do not think they can be said to be peculiarly liable to "take fright." Signs of a former attack of rickets have been found in many instances, and most frequently the disease is found in offsprings of a neuropathic stock. Its association with epilepsy has been definitely established. Dr. W. J. McCardie lays a certain amount of stress on dilatation of the pupils of the eyes. It has been said that the mesenteric glands are sometimes palpable, but, as these glands are invariably quite small, and not very hard, and as also the subjects of the disease are usually well nourished, the occasions when they can be felt must be very uncommon. The palpability, or non-palpability, of the cervical, axillary, and inguinal glands may easily be misleading.

Mr. Bellamy Gardner and Dr. Horace Manders rightly emphasize the great value of radiography. At the last meeting Dr. Bernard Spilsbury pointed out that adenoids were not necessarily an indication. For my own part I would have said that the presence of adenoids, *per se*, was never a trustworthy

indication. (True, a number of cases of lymphatism are only brought to light by the occurrence of a fatality while undergoing an operation for the removal of post-nasal growths and enlarged tonsils, but this is not to be wondered at considering the enormous number of such operations that are performed yearly, and also that this huge percentage of operations is performed at a period of life when deaths from lymphatism are commonest, and the very condition for which relief is sought by operative measures is the one with which lymphatism is more frequently concomitant. The only surprising thing is that more fatalities are not met with in this particular branch of surgery.) Further, in some instances of the disease, adenoid growths and enlarged tonsils are not present. Either or both the liver and spleen may, or may not, be enlarged, and to such a variable degree that no reliance can be placed on the absence of physical signs of enlargement, and very little on the presence of the same. One would expect to find some constant change in the blood, though as yet no such phenomenon has been discovered. Post-mortem, the pathological changes are gross and widely disseminated, but unless one is cognizant of the picture of the disease, or on the look-out for it, or methodically considering every organ seriatim, it is quite easy to miss. It seems a paradox to say that the changes are gross and ubiquitous and in the same breath to say that they are so easily missed, but on further consideration this is not so. The thymus can be missed because it is seldom so large as to obtrusively present itself, and if the incision is only commenced at the root of the neck it tends to remain in large part hidden beneath the sternoclavicular articulation. The tongue may not be observed, likewise the lymphatic structures in the mouth. The œsophagus may not be opened, nor the stomach, though the former may contain a few much-enlarged miliary lymphatics, and the latter, perhaps, a complete chain network of them. The small and large intestines, too, ought to be examined in their entire length, though unfortunately these searches are not always made in country places. The mesenteric glands may be obscured by an excess of subperitoneal fat. It is these glands that are chiefly affected. They may be all rather large, and pale pink in colour, or all quite small, and bright pink. Sometimes certain groups of glands are more picked out than others. A uniform enlargement of all the peripheral glands is, as a rule, conspicuous, and this in the absence of any apparent infective disorder of the intestines or evidence of tuberculosis. The bronchial, tracheal, and mediastinal glands are often not much affected. The changes in the liver can be discounted as, although pathological, one might not know how to account for them, and the changes, in themselves, are not sufficient to lead to death. Much the same can be said of the spleen. The heart may show a variable amount of degeneration of the myocardium. When this is marked, the death certificate is probably signed "fatty degeneration of the heart"; and when it is not marked, and the result of the autopsy has been unsatisfactory, the death certificate is probably signed "heart failure." It is in these ways that many cases of lymphatism are undoubtedly not recognized.

Concerning the kidneys, in two of my cases reported in the *Lancet* of

December 26, 1908, there was present an acute congestion of both organs. In one of these cases the notes recorded at the autopsy were: "Both kidneys were dark red in colour; on section, there was present a marked uniform injection; the cortex was not abnormal in thickness, and both cortex and pyramids were dark red; the capsules stripped readily, leaving a very finely granular surface. The condition of both kidneys resembled that of an acute scarlatinal nephritis, and a post-mortem catheter specimen of the urine gave a neutral reaction to litmus and showed a considerable cloud of albumin as being present." The possibility of a previous attack of scarlet fever was, of course, definitely eliminated. I do not think an acute nephritis has been observed in cases of lymphatism before, but I felt sure at the time that it was part and parcel of the disease, and merely another indication of a general toxæmia that I am convinced exists. The pathology of the disease, in our present limited state of knowledge, is necessarily bound to be obscure, and one is only able to make the few merest suggestions. It has been maintained that the heart is affected first at the fatal termination, but with this I do not agree. It is the respiration that is affected first in the majority of cases, although in some instances both cardiac and respiratory functions appear to be seized simultaneously. Apparently the disease is one that has been insidiously at work for an unknown time until some either exceedingly serious or most trivial firing-off cause inexplicably gives rise to a fulminating termination, resulting in more or less sudden death. In five out of the six cases I have reported the firing-off cause was exceedingly trivial, viz.:—

(1) Boy, aged 14, after three inspirations only of ethyl-chloride.

(2) Male, aged 17, rising from customary midday meal.

(3) Male, aged 29, while taking a tepid bath. The temperature of the bath was 95° F., previously tested by thermometer; the bath was taken during the summer months, and the patient was accustomed to having such baths.

(4) Female, aged 28, after a single epileptic fit.

(5) Female, aged 32, the only assignable exciting cause being the act of acquiescing to accompany her sister against her inclination.

In all these cases death was sudden, and the cardiac and respiratory functions seemed to fail almost simultaneously.

In the sixth case the exciting cause was a very serious one, despite which death was not nearly as sudden, and respiration was affected an appreciable time before the heart. This patient was a male, aged 21, who died *somewhat* suddenly after eleven violent epileptic fits. Six months previously, also, this subject had survived a serious attack of acute lobar pneumonia, when, for two days at least, it did not seem at all likely he could recover.

At the meeting here on December 3 I understood Dr. Hewitt to say that he would like to hear of some cases where other organic pathological changes, besides those indicative of lymphatism, were present at the autopsy. In this connexion I have seen several autopsies where pathological changes consistent with lymphatism and other diseases were concomitant, cases in which it was not possible to say what predisposing or exciting contributory factor lymphatism

formed in the causation of the fatal termination. Although the lymphatism element in these instances was left out of consideration altogether, nevertheless it probably played a very important part. The sixth case was very instructive, and formed an excellent example of the very serious nature of the firing-off cause that is sometimes necessary to determine the more or less sudden ending in the less marked instances of the disease. This patient had definitely not passed into the so-called "status epilepticus," in which it is recognized that death can take place, and his sudden death came quite unexpectedly. It also instances a case in point where a patient with chronic lymphatism could only, six months previously, survive a bad attack of acute lobar pneumonia. Notes recorded at the time were as follows: "The patient, a male, aged 21, was well nourished, of excellent physique, and his general health was good. He was an epileptic, however, and the subject of both frequent and strong fits, which occurred more or less in bouts. Four days before he died he had fourteen fits in the day, three days before he had five in the day, then no more till the day that he died, when he had eleven in the morning; death occurring at 1.50 p.m. On the morning of the day he died his general condition was good, and his pulse was full, strong, well sustained, regular in force and rhythm, and not greatly accelerated. However, about half an hour before he died, a sudden change for the worse was noticed. The colour of the face changed, the expression became anxious, and the breathing laboured, shallow, irregular, and hurried, but not stridulous, and there was no question of any obstructive dyspnœa. At first the pulse was still good, and, though scarcely so strong as previously, nevertheless it did not indicate that there was any cause for alarm, as did his breathing and general countenance. Within ten minutes of this observation, however, death unexpectedly took place." At the autopsy the pathologist who made the examination could not find any adequate cause of death. I was present at the latter part of the autopsy, but the signs of lymphatism escaped my notice, and it did not occur to me to look for them. It was only on subsequently discussing the unsatisfactory result of the examination that the possibility of lymphatism occurred to me, and we had the body reopened and found the thymus present, and other signs compatible with the disease in a milder form than usual. It was this example of the disease that led me to provisionally divide the cases of lymphatism into quasi-acute and chronic cases. By quasi-acute cases I meant to denote those instances in which, although the exciting cause might have been absurdly insignificant, death had been very sudden, and the cessation of both respiratory and heart functions had taken place simultaneously. The autopsies on these cases reveal a larger thymus than usual, and more gross changes in the tongue, œsophagus, heart, stomach, small and large intestines, liver and spleen. The enlarged mesenteric glands also are more numerous, but uniformly smaller, and always a bright pink, or even red, in colour, and there may be present in addition an acute nephritis, with albuminuria. By the chronic cases I meant those in which, although the exciting cause might have been a very grave one, death was not nearly as sudden, and the respiratory centre was affected an appreciable time before and to a greater extent than the

cardiac centre. The autopsies on these cases reveal a relatively smaller thymus, possibly a slight uniform congestion of the lungs, and no nephritis. Also the morbid changes in all the organs are less marked, and the enlarged mesenteric glands are fewer in number, uniformly larger, much paler pink in colour, and never red. Sometimes in these cases some groups of glands are more affected than others. Having constantly considered the disease now for over four years, I am convinced that a general toxæmia exists. The changes found in the heart, lymphatic system, liver, spleen, and kidneys are all compatible with such a theory. I have known of one case, too, where the temperature before death rose to 102° F. The manner in which death takes place seems comparable to that of a poison acting on the central nervous system, and affecting the respiratory centre in the medulla to a rather greater extent than the cardiac centre. In the quasi-acute cases both centres seem to be seized practically simultaneously as if from some huge overdose of a narcotic, while in the chronic cases the respiratory centre is affected an appreciable time before, and to a greater extent than, the cardiac centre, as if from a much smaller overdose of a narcotic. The concurrence of lymphatism and diphtheria is said to be particularly fatal, and theoretically one cannot be surprised that this is so. Both diseases give rise to a general toxæmia, both diseases affect chiefly the lymphatic system, and both diseases can produce an acute fatty degeneration of the myocardium.

Svehla found that the intravenous injection of thymus extract produces in dogs a great fall in blood-pressure and acceleration of the heart, and, in lethal doses, dyspnœa and collapse. When deaths from lymphatism occur in children of about 2 years of age—that is, at the time the thymus should have attained, roughly, about its maximum development—the gland, post mortem, is found to be much larger than it usually is at such an age. Concerning the prognosis, I think one might venture to conjecture that the disease is *not* uncommon, and that many subjects of the disease *do* recover. I think one also has enough evidence to assume that victims of the disease have *certain critical periods*, that it depends on the *gravity* of these certain critical periods, and the *severity* of any shocks, or the *nature and severity* of any intercurrent maladies occurring at such times, to determine or not a fatal issue. Thus the reason why sudden deaths from lymphatism under general anæsthesia are not more common is probably to be explained in this way, if I may be allowed to make use of a hypothetical example. For sake of argument, let it be granted that lymphatism occurs once in about 600 persons, which means that the chances are roughly 600 to 1 against coming across a case at all. Let it further be granted that its victims have a certain critical period (? more than one) of, say, two to three weeks' duration, during which trivial and insignificant exciting causes may be sufficient to give rise to sudden death. Having picked from the 600 persons the one with lymphatism, it will be seen that the odds are still enormous that you do not also strike that certain critical period, but, should anyone have the "misfortune" to strike both the subject of the disease and, coincidentally, his, or her, critical period, then in very many, but not all, instances I maintain

that no anæsthetist could avert an inevitable fatality. Further, of the cases which might possibly have been saved, the majority are those in which anæsthesia was "incomplete," while perhaps a quite small minority of the same group might also have been saved in more experienced and competent hands. To further exemplify my meaning, had any surgeon or anæsthetist been unfortunate enough, coincidentally, to have been performing even a minor operation, or administering *any* anæsthetic, respectively, in any of the five cases I have before referred to on the day that they died suddenly, it is only perfectly reasonable to predict that disaster would have befallen them.

On December 3 Dr. Leonard Williams suggested that the previous administration of thyroid extract in suitable doses might be beneficial. This may be so, and I am not in a position to speak either one way or the other; but I would like to point out that in Graves's disease, where there is supposed to be a hyperactivity of the thyroid gland, cases which have died suddenly and unexpectedly have been found post mortem to have an enlarged thymus gland also. *Re* the question of anæsthetics, I have several times drawn attention to the fact that the fatalities that occur from lymphatism while under a general anæsthetic invariably appear to take place during "incomplete" anæsthesia, either before reaching, or on recovery from, the third stage. Dr. Dudley Buxton carefully discriminates between "light" and "incomplete" anæsthesia, and consequently when, before, I have said light anæsthesia I have meant, and should more correctly have said, incomplete anæsthesia. Personally, I feel convinced that the effect of shock during incomplete anæsthesia is far more important than the effect of whatever anæsthetic may be employed. If I were cognizant of the fact that I was dealing with a subject of lymphatism in which it was exigent that a general anæsthetic should be given, if time allowed, I would give a small dose of strychnine by the mouth three times a day for a week or fortnight preceding the administration, and in any case I would employ a suitable hypodermic injection of morphine a quarter of an hour before commencing. With these precautions, and having also forewarned the parents of a "certain amount" of danger of an unusual character, I would give for preference a mixture of two parts of chloroform to one part of ether, on a Schimmelbusch's mask or by means of a Junker apparatus. Under no circumstances would I accede to a request to commence the operation until the patient was in the third stage of narcosis, with the eyes fixed and the pupils contracted to their smallest limit. I would also maintain complete anæsthesia until the operation was entirely finished.

I remember once giving a mixture of two parts of chloroform and one part of ether to a little boy aged between 10 and 12. Before commencing, both his pupils were equal but unusually large, and I found it impossible to get them to contract even to a small extent. Although complete anæsthesia had been maintained for a quarter of an hour or longer, still each time I slightly pushed the chloroform the pupils did not contract, but the respiration became shallow, irregular, and then stopped. I have many times since thought that this was most probably an instance of lymphatism. I have here quite a short paragraph taken only a few weeks ago from the *British Medical Journal* of

December 25.[1] I would like to read this, as it is more or less typical of a number of such notices that I have seen from time to time either in medical journals or even in the daily Press. It was in the medico-legal column and headed "Death during Operation":—

> At an inquest in Gibraltar on December 7 on the body of a man aged 18, the evidence given was to the effect that the deceased, when being prepared for operation, had at first taken chloroform well and naturally, but before anæsthesia was sufficiently profound, respiration became intermittent and the face pale. Administration was then stopped until both became normal, after which the desired incision was made. Forthwith respiration and the heart's action both ceased, and neither could be restored despite efforts, including cold affusion, injection with strychnine, and artificial respiration, carried on for an hour. The jury returned a verdict of death from surgical shock while under the influence of chloroform.

Now there can be very little doubt that this was a case of lymphatism. The exciting cause of the sudden death was shock, and the verdict of the jury was in this case correct—viz: "Death from surgical shock while under the influence of chloroform." But such shock is not sufficient to account for sudden death without some predisposing factor. I do not, of course, say that chloroform cannot cause death in cases of lymphatism, but I do emphatically say that it very rarely, if ever, does account for the fatalities. Had more chloroform been given here it is morally certain that death would have resulted, but who could, or would, attempt to give more when confronted with such alarming symptoms —viz., intermittent respiration and pallor? Moreover in cases of lymphatism it is almost the exception for such premonitory symptoms to manifest themselves, and the disasters occur either from commencing the operation before complete anæsthesia is established, or from stopping the administration too soon and allowing the last few skin sutures to be inserted after general anæsthesia has again become incomplete. Since there is reasonable evidence to show that cases of lymphatism do recover, it is fair to presume that such cases as those just mentioned might at least have stood a chance of recovery had the operations been commenced and finished during "complete" anæsthesia, and of course the lighter the better. The reason why I advocate chloroform in preference to ether is twofold: (1) One is better able to discriminate as to the completeness or otherwise of the anæsthesia, or, at all events, the non-expert is. (2) The heart in such cases is invariably degenerated, and its action consequently enfeebled, and it is better to temporarily relieve it of some of its work by a drug which lowers the general blood-pressure, than to stimulate it by ether without lessening the work it has to perform. At the commencement of the administration a dilute vapour of chloroform is more advisable than a powerful one, but there is no evidence to prove that chloroform is more dangerous than either ether or ethyl chloride in subjects of lymphatism. To me it seems that the sudden deaths might be explained on purely psychical grounds in this way. There can be no doubt of the presence of a toxæmia—and I have sometimes wondered whether it is possible for such a toxin to be manufactured by the breaking down and decomposition of the nucleo-proteids contained in the thymus. It is conceivable, moreover, for such a toxin to so affect the medulla as to give rise to a devitalized, or even moribund, condition of all its nerve centres, and with such an increased vulnerability of the cardiac and respiratory centres

[1] *Brit. Med. Journ.*, 1909, ii, p. 1829.

more or less trivial afferent impulses or shocks might be sufficient to give rise to their cessation of function, resulting in sudden deaths. Further, if this were so one would not be surprised that in the more chronic examples of the disease this afferent impulse required to be a much more formidable one, and that in these instances, too, the respiratory centre was affected before, and to a greater extent than the cardiac centre. There is evidence to show that both the cardiac and respiratory centres are functionally abnormal in such cases, and the short paragraph I have just read illustrates this where it says "before anæsthesia was sufficiently profound respiration became intermittent and the face pale."

Lastly, I have never heard of any cases of lymphatism occurring in either sex who had children, and, since virility and maturity give rise to atrophy of the lymphatic tissues and thymus gland, I have some slight grounds for thinking that subjects of lymphatism are sexually sterile.

Dr. A. S. CURRIE said he was not a professed anæsthetist, but some years ago he conducted a long series of experiments, when he was assistant to the Professor of Physiology in Glasgow University, into the cause of death under various anæsthetics; and he had had a large hospital experience in the administration of anæsthetics and an exceptionally large experience in practice. With regard to the question of the poisoning, which was raised by Dr. Dudley Buxton in the November *Phonographic Medical Journal*, there was recorded a striking instance of sudden failure of respiration in the case of a young woman who was supposed to be healthy, whose sister had had an attack of diphtheria, and in whom an antitoxin had been administered as a preventative. Two or three minutes after the administration breathing ceased, the face became pale, and, in spite of artificial respiration, the patient died. Post mortem nothing was found to account for the fatality, which, according to the reports, seemed to be due to poisoning of the respiratory centres. The question of inhibition of respiration arose, as anæsthetists well knew, at certain periods during the administration. When he was conducting his experiments single-handed, if the animal was partially roused from the anæsthetic, and he stopped and rapidly anæsthetized the animal, he almost invariably found that the animal died almost as if it had been shot. He was much puzzled by that until he found a paper in the *Anatomical and Physiological Journal* by Professor Rutherford, in which he said he had noticed a similar inhibition of respiration, and that it was entirely due to an irritant effect on the nasal branches of the fifth nerve, for he found that if he destroyed those nasal branches by the cautery, and chloroformed the animal, that inhibition did not take place; and if he tracheotomized the animal there was no inhibition. He (Dr. Currie) thought that in the operations for adenoids, the patient having been anæsthetized to the point of abolishing the throat reflex, after the adenoids had been removed, if there was a further operation for the removal of tonsils, the anæsthetic having been suspended after the adenoid operation and being again resumed, inhibition might readily occur. The mask had to be removed in order to avoid an unfortunate result. Removal of adenoids was the operation he most dreaded in connexion with anæsthesia. Fortunately, he had had no unhappy results, but

some years ago he had a fright. Adenoids were being removed when the pulse and respiration ceased, artificial respiration was performed, and he was almost giving up when there was a deep gasp and breathing was resumed. From the appearance of the patient it was likely he had had an epileptiform attack. He went from the operation to the house where the boy lived, and saw his guardian, who told him a brother of the patient was in an epileptic colony in Germany, and that another brother was an epileptic and was under medical treatment. He felt deeply grateful for Mr. Gardner's able paper.

Mr. BELLAMY GARDNER, in reply, said he would make his remarks brief owing to the lateness of the hour. Since the reading of the paper on "Lymphatism" on December 3, 1909, a case had been reported to him by the courtesy of a medical man at Ramsgate, in which a male patient, aged 10, died during anæsthesia for removal of enlarged tonsils. After the operation had been accomplished the patient's head was drawn over the end of the table, and during a digital examination for adenoids, which were not found present, cyanosis and respiratory failure occurred and death resulted. Post mortem, the only diseased organs found were enlarged glands at the roots of the lungs and lying on the vertebræ behind the intestines. Some were the size of hazel-nuts; the thymus gland measured 2¼ in. long and 1 in. broad, exactly resembling that of an infant; an enlarged gland was also present in the right axilla and one in the neck.

During the discussion on the paper Dr. Dudley Buxton had made a most welcome suggestion that a committee should be formed to investigate the disease of lymphatism, and he hoped this would be done forthwith.

Dr. Spilsbury had given the salient features of the disease presented in a considerable number of cases he had investigated, which had occurred both in anæsthesia and also after trivial injuries, and rightly laid stress upon the necessity for a thorough microscopical examination of the tissues in each case. He regarded this as essential, because it was not possible to discover the cause of the disease until there was ample recorded material, with detailed clinical signs, to the utmost post-mortem findings.

Dr. Hewitt, whom he thanked for his kind remarks upon the paper, did not believe that the heart, *per se*, had any important share in deaths from anæsthetics. And with this the speaker agreed, as the most serious cases of heart disease had passed through anæsthesia in numbers of instances without displaying alarming symptoms, or even divergence from the normal.

Dr. Horace Manders had given them excellent assistance in furnishing an account of one important case of death from lymphatism to the discussion, which was most ably and clearly demonstrated. He hoped this would stimulate others to do the same in sending similar reports to the new committee on lymphatism.

Dr. Leonard Williams had been much impressed with a letter which appeared in the *Lancet* from Dr. Munro[1] on status lymphaticus, but he (the speaker) was inclined to disagree with some of the statements in that letter. When studying this subject thoroughly for many months, he had not found

[1] *Lancet*, 1909, ii, p. 1468.

any description of a slow and inexcitable pulse in any of the recorded cases, nor did he find that the temperature had been described as subnormal. He thought Dr. Munro had been led into a side-track in some way by the analogy of persons of a lymphatic temperament, in his opinion a totally different nervous class. He thanked Dr. Leonard Williams warmly for his valuable hints with regard to the relation of thymic with thyroid diseases.

Mr. Carter Braine's casual communication to-night certainly bore on the discussion. Mr. Braine cited a case in which the thyroid gland, which was not visible before, at a moment of a respiratory crisis in anæsthesia was found swelled up to a very noticeable size, and when the crisis had passed the gland returned to normal. The inference as to what might happen in the case of the thymus gland was so obvious that the communication was one of the most valuable yet contributed.

He was more than interested in Dr. Farquhar Buzzard's kind contribution to the discussion. He knew him to be the leading authority on myasthenia gravis, and the study of that condition would surely throw much light upon the present discussion. Light was wanted on diseases which from above and below touched lymphatism, and especially where there was a co-existence of lymphatism with other diseases in the same patient.

He did not quite see, with Dr. Blumfeld, that future cases of lymphatism must necessarily be gross instances of the disease before death could properly be ascribed to its presence, because many cases of slight shock without anæsthesia had died of lymphatism not so well marked as that of Case VI.

Dr. Vincent seemed to think the case was full of rachitis. He (Mr. Gardner) certainly thought that if they could be on the track of the disease in very early life, where the thymus gland was enlarged, they might, by experiment with diet and drugs, get a treatment which would cure the disease.

Dr. Levy's results of careful laboratory experiment were negative, but he had actually done some valuable work which bore on the subject. He (the speaker) suggested that injection from a hyperplastic gland would also be useful.

Dr. Humphry, who had had a large experience in hospitals and asylums, had pointed out that only very slight causes might cause death in status lymphaticus. He believed that gentleman knew as much about the disease, clinically, as did anybody, and he had certainly made important contributions to its literature. He believed the interest of the subject would cause contributions to be made dealing with its medico-legal aspects, such as in railway accidents, in which shock without definite injuries had been sustained. In such people, possibly, experts would be able to say there were certain conditions present showing that the person was suffering from lymphatism and that sudden death might occur at any time.

Dr. Currie's experience and experiments were useful as suggesting a danger in re-inducing anæsthesia during the recovery stage.

He was very grateful for the scientific spirit in which his paper had been received, and expressed his cordial thanks to each speaker in the discussion for the valuable information which had been afforded in throwing much new light on an obscure disease.

Section of Anæsthetics.

March 4, 1910.

Mr. RICHARD GILL, President of the Section, in the Chair.

Appointment of a Committee on "Lymphatism."

The PRESIDENT said that as the outcome of the discussion which was held on the subject of lymphatism, it had been suggested that a committee be formed, with power to add to their number, to prosecute research on the subject. The names of the proposed committee were as follows: The President, Dr. Dudley Buxton, Mr. Bellamy Gardner, Dr. McCardie, Dr. Spilsbury, and Dr. Manders. It was hoped that they would do all they could to increase the knowledge on that interesting subject.

Dr. DUDLEY BUXTON proposed, and Mr. BELLAMY GARDNER seconded, a resolution that the committee be formed, and it was unanimously carried.

The Choice of the Anæsthetic.

An Address Introductory to a Discussion on the Subject.

By DUDLEY W. BUXTON, M.D.

I CANNOT help feeling that the subject which has been committed to me to open covers a very wide range. It comprises perhaps the most important part of our practice, and it would be impossible for me to deal adequately with it in the time at my disposal, so that I shall indicate

the lines which experience has taught me should be our guide. But I wish it to be clearly understood that I in no way deprecate the choice made by others. No doubt in the hands of another man the choice of another anæsthetic might be, in many cases, quite as satisfactory as that which has appealed to me. A very important proviso is that which David made before he set out to meet Goliath—namely, that he should "prove his armour" before he entered upon the fight: in other words, the anæsthetist should have experience with the particular anæsthetic which he adopts. And method also is frequently of tantamount importance. In many cases I shall suggest the employment of chloroform; but if that anæsthetic were used by certain methods, I should deprecate its application, while approving its employment by methods which I regard as being safe and sure.

The choice of an anæsthetic depends on at least three important points: the condition of the patient, the necessities of the operation—which should include the region of the operation—and the duration of the operation. The safety of the patient must be paramount in the whole problem; not only during the period of induction and the period of maintenance of anæsthesia, but also after the patient has come forth from the anæsthetic and has to face the after-effects alike of the operation and the anæsthetic, since these may be disadvantageous to him. For, instance, in operations on the chest or on the abdomen it might be justifiable to employ ether; and yet if, after the operation, the patient is going to have such tight bandaging as to restrict the respiratory movements, and prevent the ejection of any accumulated fluid in the lungs, the pulmonary after-effects might show the choice of the anæsthetic to have been faulty. Then we have to consider the patient from the point of view of normality and the degree of his departure from the normal. Many of us will feel that some of the old rules in vogue when we were commencing our work have now fallen into desuetude—such rules as those with regard to age, and the rules concerning method. We used to be told that ether should never be used below 6 years of age, and never after 60. We were told that children took chloroform like mother's milk, and that they enjoyed a special dispensation granted by Providence which relieved them from the occurrence of dissolution whilst inhaling that drug. All belief in these things is now wiped away. We know that age is no absolute criterion in the choice of an anæsthetic; we have to decide by individual physique and individual peculiarity rather than by age. Take, for example, an elderly man who, by the old rule, will be debarred from ether after the age of

60. Some men of 40 should be debarred from ether, because their arteries are degenerate, and they suffer from bronchial trouble, whereas many men over 60 are, *qua* their arteries, under 40. The main point with regard to young children is this: that they lose heat very rapidly, so that under an anæsthetic, shock, which is practically loss of heat and failure of blood pressure, is the chief peril. That is why it is inexpedient to give them ether unless precautions are taken to avoid loss of body heat. Ether is taken by young children very well for short operations if it is carefully given, but as the body temperature may be lowered from ½° F. to 3° F. very quickly, and that loss is very serious, we shall, I think, hesitate before giving ether to a very young child. Some years ago I tested the effects of giving ether to young children, and the main points which struck me were the liability to salivation and bronchorrhœa, which I regarded as not only distressing, but injurious to the children. Of course for very brief operations one would employ nitrous oxide or ethyl chloride. Dr. Flora Murray, a lady who has had a considerable experience with children, has published a paper citing a considerable number of cases. She states that ethyl chloride, given in small doses, is very valuable for small operations. In dental work, especially for fidgety children, chloride of ethyl is valuable, although it is liable to cause nausea and even sickness.

In medium-aged children it is better to continue the anæsthetization with chloroform when surgical operations are in progress. For those who maintain anæsthesia right through with one anæsthetic, I should advocate a mixture which I now employ, rather than the A. C. E. or C. E.—viz., 1 in 10 alcohol and chloroform (Schäfer) by the open method. I should employ chloroform *ab initio* when a regulating inhaler is used. It is very important in children, whose protoplasm is very susceptible to all drugs, to insist upon the use of low percentages, at all events during the period of maintenance, and avoid anything like asphyxiating complications. This, of course, would be effected by the employment of a regulating inhaler, especially if you employ oxygen *pari passu* with your chloroform. One cannot help being struck by the terrible mortality in young children during the operation of circumcision. Chloroform is commonly given, and children frequently die, and I think they die because the reflexes are so very active in that operation that perhaps those who are less experienced than the members of this Section push the anæsthetic with the idea of abrogating all the reflexes, which, of course, is an impossibility. Therefore, with the exception of extremely young children—babes a few weeks old—I

should always employ, for the induction of anæsthesia, ether in these cases, and go on afterwards with chloroform.

The routine anæsthetic for youth and middle life is recognized as being nitrous-oxide gas, followed by ether. When we remember the very potent after-effects of ether, even if there are not nausea and vomiting, I think we have forced upon us the fact that the majority of people have more discomfort after ether than is at all necessary; this arises, I am sure, because in the hands of the inexperienced too much ether is given. It is said that the employment of the open method will prevent this excessive dosage; but that is not the experience abroad, where the open method has been largely used and considerable quantities of ether are used, and where the remote after-effects are frequently prejudicial. So now my practice is to confine ether to brief operations, even in middle life, and I follow it with chloroform; and I practically always give chloroform in association with oxygen so as to avoid all asphyxial complications.

We will now pass on to abnormal types; but perhaps I should mention old people—old by the standard of the condition of their tissues and their physique. In the case of such a person we have to treat him as a pathological specimen. I deprecate the employment of chloroform for old people simply because they have passed the meridian of life. The types to which I shall draw your attention are the asthenics, the neurasthenics, the alcoholics, the marasmics, and those who suffer from renal inadequacy, and those who have distinct renal trouble associated with albuminuria; those who suffer from glycosuria, those who are cyanotic, anæmic, as well as the condition termed "lymphatism."

In the case of the asthenic—that is, either those who have suffered from serious and prolonged illness, or who have never attained to robustness of frame—I should certainly employ chloride of ethyl, followed by ether, associated with oxygen, because I am sure that in these people the initial stimulation of the circulation which the ether gives is of great value. If there be a tendency to catarrh or lung trouble I should rapidly, according to the condition, change from ether to chloroform. I am convinced that if you have once obtained anæsthesia, the maintenance of it can be managed with an extremely small quantity of the anæsthetic, and you may employ chloroform in practically all cases without lowering the blood-pressure, provided you will employ a small enough quantity and avoid any intercurrent asphyxial condition. This can be done by a regulating inhaler, giving oxygen *pari passu* with the chloroform.

The neurasthenics, although allied to the asthenics, introduce another and somewhat perplexing condition. These patients are sometimes absolutely paralysed with fear; are extremely jumpy and anæmic, and they have only a very small respiratory equation. You notice their breathing is feeble, and the thoracic excursion is small, so that they seldom, if ever, get full pulmonary ventilation: hence all the conditions are adverse to successful anæsthetization. If these patients are given scopolamine and morphine before the operation, they are certainly better able to face the ordeal than if brought in cold blood to the operating theatre; in the former case they are contented and sleepy, and a very small quantity of chloroform will see them through the operation. Of course there is no reason why the ordinary sequence of ethyl chloride and ether and chloroform should not be carried out in these cases, although the sequence of scopolamine with morphine and chloroform is so successful for these persons that I usually adopt it in preference to other methods. Some people advocate giving strychnine together with or subsequently to scopolamine and morphine, but that is a practice which I deprecate, first because I think that strychnine given in small doses is of little value, and given in large doses, I think, it increases the tendency to shock; and, further, because strychnine and morphine act antagonistically upon the cord, and you are taking away with one hand what you are giving with the other.

When we pass to consider the alcoholic, we have to face one of the most difficult problems that falls to the lot of the anæsthetist. Not only do such persons require very large quantities of the anæsthetic, but they are peculiarly liable to undesirable after-effects. I can recall in my practice several cases of patients who had gone successfully through an anæsthesia with gas and ether, but when they came out of it have become acutely maniacal, have fought and struggled, and had to be controlled by force, even for hours after the operation was completed, and this even though, considering the requirements of the case, no unduly large amount of the anæsthetic had been given. I need hardly point out that the violence of mania and the perturbation of the cerebral circulation must act injuriously upon the patient, and are liable, of course, to cause harm at the seat of the operation. It appears to me the great point in dealing with alcoholics is to overpower them rapidly; you require for them a brief and rapid induction, such as can only be attained by a powerful and rapidly-acting drug, so as to tide them over the dangerous period of induction. You will probably do this best by means of chloride of ethyl, or, if you employ gas, you must give a small

amount of oxygen with it; because gas, plus struggling, frequently leads to cyanosis, and this is most prejudicial to the patient in every way, and intensifies all the bad symptoms to which he is particularly prone. Although I say you want to induce anæsthesia rapidly, I do not wish to imply that the operation should be commenced as soon as the patient has become apparently anæsthetic, because I believe it is impossible to do this with any degree of satisfaction. In order to obtain a tranquil anæsthesia with alcoholics—and in some cases it is very difficult to achieve this—it is necessary to attain a high degree of saturation of the blood, higher than is essential under ordinary conditions. This is only accomplished after a more prolonged inhalation than is commonly adopted, therefore I stipulate that a comparatively long time shall be allowed to keep on with the anæsthetic before the operation is commenced; otherwise the surgeon will be annoyed by constant reflex movements, not only superficial but deep, and these will militate against harmony among those engaged in the operation. These reflexes are very dangerous when they involve the deeper nerve chains. The vagal reflexes in alcoholics are very lively; they are prone to cause interference with respiration and with circulation when parts are dealt with which involve stimulation of the vagi and the nerves communicating with the vagi. I have indicated that alcoholics are rendered worse by intercurrent asphyxia, and I am convinced that it is a matter of the utmost importance in these cases to avoid any asphyxial complications, so that I use a large quantity of oxygen when dealing with these people. Further, alcoholics have a proneness to develop leg tremor, especially when the operation area is the foot or the leg—a condition which is very inconvenient and difficult to control. That is why I urge that the tendency to all these reflexes should be abolished before the operation is commenced; the subjecting the patient a little longer to the anæsthetic before the commencement of the operation will render these reflexes much less likely to occur. For a short operation we are faced with the difficulty that alcoholics often take nitrous oxide badly, and even if you give oxygen with it you are liable to get a period of excitement which is frequently very embarrassing. Ethyl chloride, although it answers extremely well in some cases, will fail in others. I think one will have to decide each case upon merits, and take all the various other conditions environing the patient into consideration in dental work.

A type of alcoholic who makes the question of anæsthesia very difficult is the thick-necked, obese individual, whose head cannot be readily turned to one side. I once had an alarming experience with

such a man to whom I had given gas and ether. The patient developed a violent spasm of the larynx; he was so bad that one hesitated whether it would not be necessary to open the air passages to restore breathing. The mechanism of this is simple. You cannot turn the head to one side; the saliva, in which a certain amount of ether vapour has become condensed, gravitates to the back of the pharynx, and impinges on the epiglottis and excites spasm; and in many cases where ether is pushed unduly rapidly you possibly get a similar spasm throughout the large bronchi, and probably a like condition pervades all the bronchi in the lungs. Also these people are frequently heavy smokers, and the pushing of the ether commonly produces pharyngeal cough, and the cough is extremely liable to set up spasm and cause serious complications; hence if these conditions appear imminent, it is wise to relinquish ether and to replace it by chloroform associated with oxygen. Again, when these people are obese, certain postures are obnoxious to them, such as the lithotomy position, especially if the knees are flexed very strongly upon the abdomen, as this interferes much with respiration; the inverted or semi-inverted, or the Trendelenburg posture, will cause a considerable degree of cyanosis, and this renders the ether undesirable. Marasmus in children is the indication for the employment of ether, and probably by the open method.

Let us now pass to consider renal conditions. I have referred to renal inadequacy. Some of us have gone somewhat off the track when we have laid down a hard-and-fast rule with regard to the employment of anæsthetics, taking albuminuria as our sole guide. The condition should be carefully studied in each case, and its source tracked out. The main fact to be ascertained is whether the kidneys are doing their work, and doing it adequately. When they are not, the employment of ether is dangerous; although chloroform produces a more complete destruction of the epithelium of the tubules in the kidneys than ether, yet it is evident, I think, as was shown in a research which I undertook in conjunction with Dr. Levy, that the danger in these cases depends rather upon the amount of the anæsthetic which has to pass through the kidneys, and has to be eliminated, than upon the other causes; and since the amount which has to pass through in the case of ether is very much greater than that in the case of chloroform, especially if you restrict it by the employment of a regulating inhaler, it is therefore safer to employ chloroform in cases of renal inadequacy and in cases where albuminuria is well marked than it is to employ ether all through. I have little or no doubt that when it is desirable the employment of the

ordinary routine of ethyl chloride or nitrous-oxide gas, followed by ether and rapidly succeeded by chloroform, is quite safe in most renal conditions, provided inadequacy is not very well marked; but I am sure that since the after-effects are the great point in the situation, having once obtained anæsthesia, one should maintain narcosis by means of a very small quantity of whatever anæsthetic is being used.

With regard to glycosuria, the subject is too large to enter into at all fully now. I will merely say, in passing, that, having employed most forms of general anæsthetics, and also spinal injection in these cases, I believe the main danger lies rather in the quantity employed than in the kind of anæsthetic you may use, although from my own experience I should say less trouble follows chloroform, if asphyxia is avoided, than follows ether.

Cyanosis: We have congenital cyanosis and we have cyanosis arising from some disease. It has been my lot to give anæsthetics to several people in whom the congenital condition of the heart produced the cyanosis. I have given gas, ether, and chloroform, and, although one must admit that these cases cause great anxiety, I have never been able to determine that one form of anæsthetic gave greater promise of evil than another; although in the case of nitrous-oxide gas I should deprecate its employment unless the possible asphyxial complication were avoided by associating the gas with oxygen. With acquired cyanosis, I have no doubt that any form of ether is highly prejudicial, and should not under any circumstances be employed.

Anæmia is a condition which I have regarded for many years as one of the most dangerous we have to deal with, whether that anæmia is due to asthenic conditions or to blood disease. We know that the heart of the extremely anæmic has undergone some form of degeneration, not exactly fatty, but a degeneration which renders it extremely liable to fail, even under ether. These people have a very low hæmoglobin index, and have very little power of conveying the anæsthetic to the tissues and associating it in the tissues. I believe, as a rule, anæmics take ether following chloride of ethyl well, but they are extremely obnoxious to all asphyxial complications.

As to the status lymphaticus, I think those who suffer from it are unable to carry any large amount of any kind of anæsthetic. I am not convinced that any one anæsthetic is more dangerous than another, except in so far as some anæsthetics are more powerful protoplasmic poisons than are others, and so their prejudicial results are likely to culminate in death more than is the case with the rest. Indeed, I

believe that in all people who suffer from lymphatism the main indication is to limit the exact amount of the anæsthetic which is given to them, and permit no margin beyond that, since the excess may attack and affect the tissues injuriously.

Further, in speaking of special pathological conditions, I would advert first to the obstructive breathing when there is obstruction in the upper air passages, the most common forms of which are œdema of one of the structures in the mouth, or fauces, or some swelling about the upper air passages, such as goitre, enlarged glands, inflammatory swelling, such as is met with in angina Ludovici. These are conditions where the circulation is rendered tardy through a tendency to venous congestion; and if nitrous-oxide gas or ether is given in any of these conditions, the patient, I think, is placed in grave danger. I should regard all such patients as subjects for chloroform rather than for those anæsthetics which are liable to cause congestive or asphyxial conditions. For cases in which pulmonary, bronchial, or pleural disease exists, I should say that chloroform should invariably be employed. But probably everyone will agree that asthmatics frequently take ether very well for a short time, and are even rendered better by the employment of the purer forms of that drug. But, speaking generally, bronchial conditions and emphysematous conditions, unless extremely well marked, contra-indicate the use of ether for more than a very short time. I know that those who have emphysema, and who take chloroform, are peculiarly liable to fatality from that drug, because in giving chloroform you are pouring a heavy vapour into lungs which are practically a large bottle, and the only expiration possible in these patients is carried out by the movements of the diaphragm. For bronchitis and emphysema, chloroform given by what I regard as the safest method, the regulating inhaler, is the best method to adopt. If you employ the open method you must use chloroform diluted with alcohol. For lung conditions I have adopted the method of rectal etherization, and the results arrived at are very satisfactory. But there are circumstances associated with it that render its general adoption difficult. Still, there is that method to fall back upon when the conditions seem to absolutely exclude the use of chloroform.

Pathological conditions of the heart now cause me far less anxiety than they did in former years. I believe that the asthenic and the fatty heart are more or less bogies, and that, if the anæsthetic is carefully given, patients with cardiac disease take chloroform as well as they take ether. I think, however, there are some circulatory conditions which

render it very difficult to decide what anæsthetic is best and safest. Recently I had to give an anæsthetic to a woman who had a well-marked aneurysm, extending from the arch of the aorta almost to the subclavian, at all events into the innominate. I gave scopolamine and morphine before the general anæsthetic, so as to quiet the patient and render her less liable to excitement, for the chief danger in these cases is the struggling. Subsequently I gave her chloroform and oxygen. With the exception of slightly increased pulsation at first, no untoward symptoms arose, and the result was very satisfactory.

The choice of the anæsthetic in abdominal operations is frequently difficult; of course, one is constantly met with the cry that "the patient is rigid." Can the choice of the anæsthetic help us in avoiding this bugbear? I think to a certain extent it can. I believe that in people who suffer from nervousness it is very important to commence the anæsthetic with chloride of ethyl and ether, and continue with chloroform; but the main point is to avoid any asphyxial complications, because I am convinced that most of the rigidity in the course of abdominal operations is due, not to the anæsthetic, or to the lack of it, but rather to the excess of it, and to the asphyxia; and if you avoid asphyxia, which is sometimes difficult when the patient is inverted, you obviate muscular rigidity; it can be done by the ordinary precautions and by the employment of oxygen. And we must remember that the recti abdominis, which are the muscles at fault, are respiratory muscles. Unless you paralyse the respiratory centre, which we are all anxious to avoid, you cannot absolutely paralyse the recti, at all events in young subjects. You can, however, overcome them by steady traction with retractors. That, you will say, is another story, and rather exceeds the limits of my reference.

One word with regard to the choice of anæsthetics in conditions of the nervous system. The indication in general surgery for giving anæsthetics is to maintain blood-pressure. In brain surgery and in the surgery of the spinal cord the indication is frequently to lower blood-pressure. If you lower blood-pressure, providing you do not carry the lowering beyond a safe degree, you are helping the surgeon, and therefore helping your patient; and it is with that view I always employ chloroform, and deprecate the use of ether as likely to heighten blood-pressure and increase hæmorrhage and generally to do harm rather than assist the patient through his ordeal. The other danger is the asphyxial complication. Frequently, *ab initio*, these people are breathing badly; there is either pressure upon their respiratory centre, or some of the

respiratory muscles or nerves are interfered with so that they are performing respiration with fewer than the ordinary accessories which are considered necessary by nature to carry out that process. Our way of assisting nature is to give a large quantity of oxygen quite from the the commencement with the chloroform, and when the patient has passed into the third degree of narcosis to give very little more anæsthetic.

It has been impossible for me to deal with the wider question of when to select spinal, and when to select local, and when to select general anæsthesia; so I have found it necessary to restrict my pronouncement to general anæsthetics. If the subject has been treated at too great length, it is rather the fault of the subject and my misfortune.

DISCUSSION.

The PRESIDENT (Mr. Gill) thanked Dr. Buxton for the able way in which he had discussed the various points. He agreed with the opener as to the importance of the blood, and especially its quality, in the choice of an anæsthetic. But one point which had not been mentioned was the following: The anæsthetist was often called upon in a hurry to give an anæsthetic, and on arrival had perhaps not decided which kind of anæsthetic to give. Even if he had, he had often to deal with the surgeon, and the surgeon's choice formed the determining element in the choice of the anæsthetic. There were many surgeons in London who would have nothing but chloroform for every operation, whether short or long, and whether it was for a baby aged 3 months or for a man aged 92, no matter what the condition of the patient's body might be.

Dr. G. A. H. BARTON thought that Dr. Buxton dismissed rather cursorily the open method of administration of ether. He (Dr. Barton) was brought up to believe that ether was much better administered by the closed method. Dr. Hewitt, in the 1901 edition of his book, also dismissed the open method rather shortly. Dr. Barton had now changed his view. Dr. Levy first drew his attention to the open method of administering ether at the meeting of the British Medical Association at Exeter; and later there was some correspondence in the *British Medical Journal*,[1] in which Mr. Bellamy Gardner took part, and the latter gentleman had persuaded him to give the open method of

[1] *Brit Med Journ.*, 1907, ii, pp. 1516, 1621, 1747.

ether administration a full trial. During the last twelve months he had not used a Clover's inhaler except on one occasion, when an old-fashioned dentist asked him to use that method for a lengthy extraction. The open method of administration of ether had now become the method of choice for him in all ordinary cases. He found it equally suitable for children, though not very young ones, and for old people. The open administration on a Skinner's mask involved the use of much ether, and the air became much impregnated with the vapour, which was an annoyance to the anæsthetist, and sometimes to the surgeon; and there was the drawback that it was necessary to have the drop-bottle constantly in one's hand. He had had made an inhaler which to a large extent disposed of those objections, and which he would show on a future occasion if desired. In dental cases in children Dr. Buxton spoke of the use of ethyl chloride. He (the speaker) was an advocate of ethyl chloride, and he had not allowed the few deaths from it to deter him from using it; in giving ether by the open method he still induced anæsthesia by C.E. and ethyl chloride, so as to save time; but he did not believe in giving ethyl chloride much for dental cases, as he thought it more dangerous than gas. He had found a method by which gas anæsthesia could be induced readily in children. At the National Dental Hospital a child required many extractions, and the dental surgeon asked that ethyl chloride should be given, but he (the speaker) found that the child had only had its breakfast two hours before. The surgeon said he could not do what was required under gas. Therefore, Dr. Barton said he would give gas by Patterson's apparatus. He put the nasal piece in position and turned on the gas, but there was a little delay in finding the mouthpiece with expiratory valve, and in the meanwhile he found his patient had gone under very quietly without cyanosis or opisthotonos, owing no doubt to his inhaling a certain amount of air through the mouth. He now adopted this method—that is, gas by nose plus air by mouth—as his routine for children in dental cases. With regard to diabetics, probably no one had a large experience of giving anæsthetics to these subjects, because they were not as a rule suitable cases for operation. He had, however, known three sisters, all the subjects of that disease, and two of them were operated upon on several occasions. They all took anæsthetics well. The last sister in the family, who died long after of diabetic coma, took the anæsthetic well for a mastoid operation. He thought the dangers of giving anæsthetics in these subjects had been over-estimated. With regard to the cases with lung complications for which ether was not considered a good anæsthetic, it had been said that spinal anæsthesia was good. He saw recently in the *British Medical Journal*[1] a description of a method of giving ether intravenously in a 5-per-cent. solution in saline, which seemed to offer a very good way of dealing with those cases, and he hoped it would be tried by some London anæsthetists.

[1] *Brit. Med. Journ.*, 1910, i (Epitome, p. 34).

Mrs. DICKINSON BERRY said she felt she was showing some temerity in differing from Dr. Buxton, but in the case of children she followed an opposite plan to his, as she nearly always began with chloroform and continued with ether by the open method. It had always seemed to her that the dangerous time with chloroform was at the end of induction, partly because there was often a sudden change from a condition with lid reflex present to one of apparently very deep anæsthesia. If ether were substituted for chloroform before induction was complete she thought the dangerous period was avoided while the advantages of inducing with chloroform were retained. When inducing with ether by the open method she often got much salivation, the time was distinctly longer, and the process was more disagreeable, especially to children. The nurses at the Alexandra Hospital stated that since ether had been given instead of chloroform after-sickness had been much less. In giving anæsthetics for goitre she also found herself in opposition to Dr. Buxton. She now practically always used ether by the open method, commencing the induction with chloroform. If the ether caused salivation, which was only occasionally the case, chloroform was substituted. Patients could easily be kept very lightly under with ether by the open method, and they were seldom sick afterwards.

Dr. SILK said the question of the selection of the suitable anæsthetic was one which he had always considered of great importance. There were two aspects to which one could address oneself: what one preached and what one practised. The special anæsthetist should be able to give any anæsthetic which the surgeon might desire; but the matter was different in the case of the general practitioner, who had not many opportunities of becoming expert in the various methods. Therefore, he (Dr. Silk) thought it might be worth while considering whether it was better that an individual should be taught to give one anæsthetic well for all cases, or should he be taught a smattering of the various methods? He thought the first alternative was preferable. He admitted that the question of the choice of anæsthetic was a difficult and complicated one; it was bound to raise all sorts of questions. Two speakers had referred to methods of administration rather than to the choice of the anæsthetic. The opener said the fixed rules as to age had now been cast aside. But he (Dr. Silk) did not think that was quite so, for Dr. Buxton himself referred to children by their ages; but there could be no question, of course, that the ages of patients over middle life should be assessed, not according to the calendar, but according to the condition of their arteries. He had been glad to hear the reference to the difficulties connected with circumcision, as he was more in dread of such cases from his own experience than of almost any other; he had heard of many accidents in relation to that operation, and it needed great care. He agreed with the observation that alcoholics sometimes became maniacal on coming round, but in his experience this was less likely to occur if

vomiting took place. Underlying the choice of anæsthetic according to the operation, or the physical condition or age of the patient, was the personal view of the administrator. He thought the time had arrived when the applicability of particular kinds and methods of anæsthetization should be studied scientifically. At present probably each anæsthetist worked out a law for himself, but he would like to hear discussed the several indications for general anæsthetization, local anæsthesia, and spinal anæsthesia. Local and spinal anæsthesia were being run very hard on the Continent, but he thought that they should not always be left entirely to the surgeon.

Dr. LLEWELYN POWELL said Dr. Buxton spoke of ethyl chloride or gas as a preliminary to ether; and he wished to ask that gentleman whether he had a preference for one or the other as a preliminary. He had made a long series of trials of ethyl chloride instead of gas as preliminary to ether, and he had gone back to his preference for gas, though he had been anxious to find reason for preferring ethyl chloride owing to its greater convenience of transport. He had one or two cases in which very severe vomiting occurred afterwards, which was probably started by the ethyl chloride. As regards alcoholics, he had had one or two good results with ethyl chloride instead of gas in dental operations; but, as Dr. Buxton had said, ethyl chloride was uncertain, and might or might not be very successful. With regard to alcoholics, it was one of the few advantages possessed by the alcoholic that he had practically no after-effects from anæsthetics. He asked, in connexion with abdominal rigidity, whether Dr. Buxton had had experience of giving morphine or scopolamine previous to the anæsthetic, and, if so, whether that decreased the reflex of the abdomen when the peritoneum was pulled on.

Dr. DUDLEY BUXTON, in reply, said that if the surgeon made the choice of the anæsthetic, the anæsthetist's preference was put out of court. Some surgeons preferred to say what the anæsthetic should be, and probably in many cases they were right, as they might have had experience in a special type of case for which a particular anæsthetic had been found best. But the responsibility must rest with the anæsthetist, and unless he thought the surgeon's choice was right, the former ought not to fall in with it, and must either retire from the case or suggest another course of action. To give morphine before chloroform for head operations was an old plan. Experience, he believed, had shown that that was a faulty choice. Time had not allowed him to go into detail with regard to methods. The open method was now recognized as of great value, especially for children, though it was not free from drawbacks. He feared he had not made himself clear about the employment of chloride of ethyl for operations on children. At the Royal Dental Hospital he did not employ chloride of ethyl in all cases, but for very young children who were difficult to manage in the dental chair, when nitrous-oxide gas was given, he did employ it,

and with good results. For older children he used nitrous oxide with oxygen, or nitrous oxide by the nasal method, if the operation promised to be a long one. His remarks about glycosurics referred to cases of prolonged operations : he had frequently given nitrous oxide to glycosurics without unpleasant effects, but it was very important to combine it with oxygen. Occasionally he had followed up such cases, and he had not found increased sugar following the administration. His hearers would remember that M. Laffont had stated nitrous oxide frequently produced glycosuria, but the statement was not consonant with the speaker's experience. A large number of drugs had been employed intravenously for persons suffering from glycosuria, but the inclusion method had been adopted in most cases, and the area of operation isolated by means of bands. He thought the injection of ether into the general circulation might lead to clotting, and so the last state of such a patient might be worse than the first. He felt he must re-assert his statement that cases of goitre were best dealt with under chloroform ; he regretted that Dr. Dickinson Berry differed from him. As the result of experience he could say that the main danger from chloroform narcosis lay in the stage of induction. When the blood had reached the degree of saturation which represented anæsthesia, the patient was practically free from any particular peril as far as the anæsthetic was concerned. With regard to Dr. Berry's remarks concerning the jump which occurred when the patient had apparently been going steadily on into chloroform narcosis, a light degree suddenly being replaced by profound narcosis, he thought that was easily understood. An appreciable time elapsed between the taking of the dose of the anæsthetic by the mouth and nose and its reaching the nervous system. There was a tendency to press the anæsthetic at the time when it was judged the patient should be "under" ; then the increased strength of vapour was found to have caused a marked and rapid and often dangerous effect. But this danger could be avoided by employing a method—a regulating inhaler—which enabled one to know what quantity was being employed. He deprecated the giving of ether by the open method in cases of goitre, because he thought the risk of bleeding and congestion was greater. Such patients always salivated, especially under ether, and this hampered respiration, and so the danger was increased. He agreed with Dr. Silk that it was better for the man who was not in the habit of giving anæsthetics every day to perfect himself in one or two methods rather than to get a smattering of many methods. In teaching, he tried to perfect men in the methods of giving nitrous oxide and ether, and urged them only to use chloroform when they found ether was impossible. Each case must be judged on its merits ; one must not be too much wedded to authority in the matter of choice. He regretted that time did not permit him to deal with local and spinal anæsthesia. In some cases when the family practitioner or the surgeon had suggested that a general anæsthetic was impossible, he had been asked to examine the patient and advise as to the best course to be adopted, and in some cases he had said he thought a general anæsthetic could be given. In

others he had agreed with the decision, and had adopted one of the other courses, local analgesia or intrathecal injection. Dr. Powell's experience as to giving ethyl chloride or gas before ether coincided with his own. In many cases gas followed by ether was preferable. When the patient was very nervous, however, the rapid action of ethyl chloride was a great boon ; he did not think it caused more sickness than did gas when given before ether. Probably scopolamine and morphine did lessen the tendency to abdominal rigidity.

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OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE THIRD

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1909-10

SECTION FOR THE STUDY OF DISEASE IN CHILDREN



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1910

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Section for the Study of Disease in Children.

October 22, 1909.

Dr. G. A. SUTHERLAND, in the Chair.

Cases of Subacute Liver Atrophy, persistent Vomiting, and Lymphosarcoma, with Notes on Liver Pathology in Childhood.

By A. DINGWALL FORDYCE, M.D.

THE magnitude of the metabolic functions of the liver and the consequent high pressure under which its cells exist necessitate oxidative processes of great activity and are accompanied by strenuous detoxicating powers. From its position the liver is obviously specially called upon to deal with toxins from the alimentary canal; it is the first line of defence in alimentary toxæmias, and the alimentary canal is a distinctly vulnerable point in early childhood. Likewise, owing to the activity and variety of its metabolic functions, disturbance of them produces very varied results and symptoms. According to the constitution of the child, and according to its previous dietetic and hygienic conditions, it must only be expected that equal infections or equal departures from the dietetic normal will produce diverse results in different children. Bearing upon this subject I wish to report the following three cases:—

CASE I.

G. F., female, aged 6 years, 11 months. Died, February 23, 1909. Subacute yellow atrophy of the liver.

Family history: Two uncles and one aunt died from tuberculosis.

Previous health: Child has been well looked after; had measles and bronchitis when aged 1 year and pertussis when aged 2 years, otherwise

enjoyed good health. Present illness: One year ago child had an attack of jaundice, and since then she has complained of occasional pain in the left side of the abdomen. Seven weeks ago (i.e., seven weeks prior to death) the child began to look ill and was kept in bed. Since this time the mother has noticed that the child's abdomen has been swelling, the pain being still occasionally present. During the last two weeks the rate of increase has been greater. Five days ago the child complained of feeling very tired, and she had a bad headache. On the next day she began to have a watery vomit and diarrhœa, and the pain in the side became worse. Since then the vomit had been bilious, but there had been no motion since the previous midday. From nine o'clock the previous night the child had been comatose. There have been no fits, but before she became unconscious she was screaming and twitching.

The child was admitted to hospital on the morning of February 23, and died the same day.

State on admission: Patient was a child of good development and fair nutrition. She lay on her back in a comatose condition, but during the process of examination moved about restlessly and cried weakly. She had a somewhat dusky and sunken facies, such as is suggestive of deep toxæmia in children, and, though no distinct jaundice was present, her skin was distinctly sallow. There was œdema of the back, paresis of the left leg, squint, and paresis of the left side of the face. The pupils were dilated and equal. Temperature 99° F., pulse 144, respirations 40. Alimentary system: Lips pale, tongue furred, child unable to swallow; abdomen very distended and tense; marked dullness in the flanks, changing with position, and distinct thrill present; liver and spleen not palpable. Respiratory system and circulatory system: No distinct abnormality. Urinary system: Urine required to be drawn off with a catheter, and contained a trace of albumin. Nervous system: No head retraction; internal strabismus, pupils equally dilated; paresis left side of face; marked spasticity of left leg, less marked of right, knee-jerks exaggerated.

The child died shortly after admission. From the history received with the child on admission to hospital and from the existence of abdominal trouble of some standing, associated latterly with head symptoms, I was inclined at first to regard the case as one of abdominal tuberculosis, with terminal meningitis. The child was, however, obviously moribund when admitted, and died a few hours afterwards, and though such a diagnosis did not appear in every respect to fit in with the clinical conditions, yet I was unable to be more definite. A post-mortem

examination of the body was carried out by Dr. Lindsay Milne, to whom I am indebted for the following report:—

Post-mortem report: Fairly well developed child, slightly jaundiced, particularly over the chest and neck; abdomen very considerably distended. Thorax: Right pleura, about 6 oz. fluid, clear; right lung, patchy collapse, no tubercle or pneumonia, some acute congestion throughout; left lung similar, and left pleural cavity contains about 5 oz. to 6 oz. of clear fluid. Heart: Flabby, slightly dilated throughout, no valvular lesion; about 1 oz. clear fluid in pericardium; no mediastinal tuberculosis, except the bronchial glands, which are slightly enlarged on both sides, and on the left side one shows a few early caseous patches of tubercle; trachea and bronchi slightly congested. Abdomen: Large



FIG. 1.

Case I. Subacute liver atrophy. Liver.

quantity of slightly bile-stained clear fluid in peritoneal cavity; intestines dilated and thin; no peritonitis, but the peritoneum was slightly thickened throughout; stomach greatly distended with gas. In the ascending colon, about 2 in. below the hepatic flexure, there was a transverse, moderately acute tubercular ulcer, which extended round the bowel almost completely, but was not indurated, nor were there any acute miliary tubercles on the peritoneal surface over the ulcer; this ulcer measured about $\frac{3}{4}$ in. broad and had a clearly-defined, raised-up hyperæmic margin; there was only very little caseous material lining the ulcer. The mesenteric glands were very slightly enlarged (hyperplastic), and showed no evidence of tubercle, nor did the peritoneum

in any part, nor the intestinal tract, except in the one place where there was the large transverse ulcer. The kidneys were slightly congested and slightly fatty; no cirrhotic condition. The spleen was slightly acutely congested, but was very considerably enlarged (4 in. vertical, $3\frac{1}{4}$ in. broad, $1\frac{1}{2}$ in. thick) by old-standing chronic venous congestion; it was firm and dark purple in colour and its capsule was slightly thickened, as was all the peritoneum. Liver: Very extensively reduced in size, only 4 in. vertical and almost quite flat; it was hardly visible under the epigastric notch when the abdomen was opened; its surface was grossly

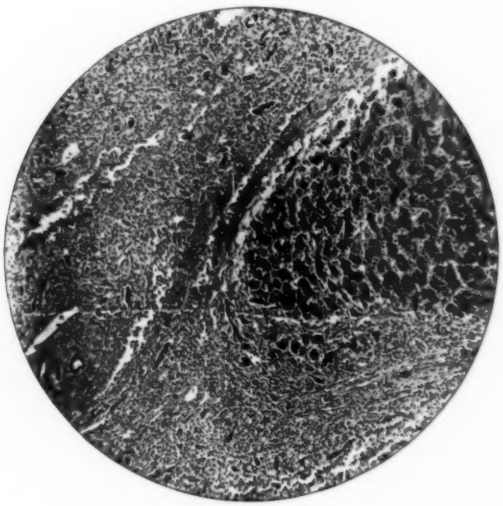


FIG. 2.

Case I. Microphotograph of liver (low power).

irregular, particularly on its anterior surface, where several very large irregularities were obvious; there were also well-marked yellow coloured nodules, up to $\frac{1}{4}$ in. to $\frac{1}{2}$ in. in size, which projected above the surrounding greyish-pink substance of the organ. On section, yellow areas up to $\frac{1}{4}$ in., or slightly larger, pervaded the substance of the liver, and were largest near the outer surface; these yellow areas were rounded in shape, but some were rather more irregular and somewhat festooned; all of them, however, were very clearly defined in their outlines; near the hilum for a large area (nearly 2 sq. in.) there were practically no

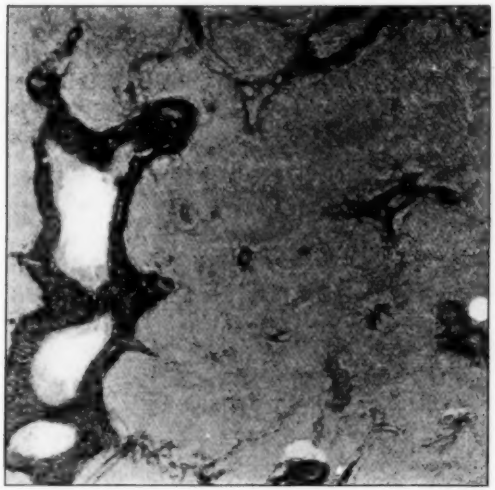


FIG. 3.

Case I. Microphotograph of liver (subacute atrophy) showing elastic tissue (dark). Weigert's resorcin staining method.

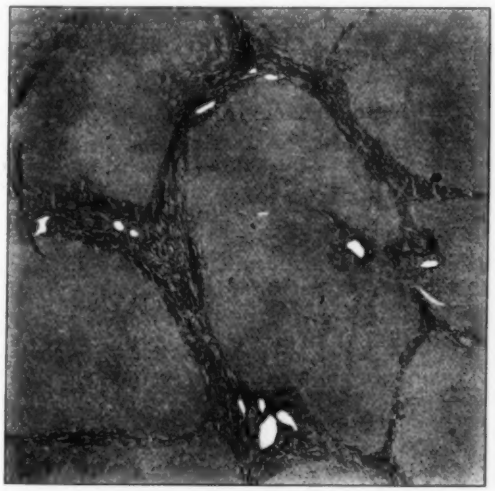


FIG. 4.

Microphotograph of liver, alcoholic cirrhosis, showing elastic tissue (dark). Weigert's resorcin staining method.

yellow masses. The substance intervening between the yellow areas was grey-pink in colour and very firm in consistence. In it the vessels seemed to show white thickenings round them.

Microscopically: The yellow areas were liver-cells, which had been in a state of hyperplasia of very marked degree. The grey tracts contained very few liver-cells, and were made up of fibrous tissue in which ramified, in a somewhat patchy distribution, pretty numerous "bile-ducts." Some columns of liver-cells could also be seen amongst this fibrous tissue. The lobular structure in the grey areas was wholly lost, as it had been completely overgrown by fibrous tissue, which was more or less dense. Brain: Slightly congested, not particularly œdematous, no hydrocephalus, no meningitis or tumour.

Case II.

L. M., female, aged 7 years and 10 months; died September 19, 1908; persistent vomiting. She was admitted to a surgical ward of the hospital suffering from caries of the dorsal spine, at the request of her doctor, because, "owing to her mother's confinement, there was no one to look after her at home." On admission she was put to bed, was given the ordinary hospital diet, and in the evening was given a teaspoonful of castor-oil. She vomited once during the night and twice the following morning, and was then transferred to a medical ward.

Family history: Father suffers from pleurisy; two of his brothers died of consumption; mother suffers from epilepsy. Patient is the sixth child of a family of nine; two brothers and one sister having died from tuberculous affections, and one brother and one sister having died in infancy from unascertained causes.

Previous health: Patient has always lived in the country and enjoyed good health apart from the spinal condition. She tends to be constipated, but has never been subject to attacks of sickness. She had measles when four years old and chicken-pox when five; otherwise she has never had any illness till the present spinal condition, which commenced six months ago and has improved under treatment.

State on admission: Bright, fairly well nourished girl, with slight malar flush. Except for the marked kyphosis affecting the upper dorsal vertebræ, there were no abnormal physical signs.

Such were the conditions when the girl was admitted to the ward on September 16, 1908. During that day she vomited constantly, as also

during the following days, and she died on September 19. Her temperature, previously normal, rose just before death to 103° F., respirations 40, and pulse 140 per minute. There was no marked air-hunger, but there was a very distinct odour of acetone in her breath, and acetone and diacetic acid were present in the urine. During the three days she was in the ward she retained nothing in the stomach—milk, albumin, water, champagne, all were at once returned; washing out the stomach produced no improvement, and sugar and water and bicarbonate of soda, when given by the mouth, were always at once rejected. Nutrient enemata of peptonized milk, sugar, and bicarbonate of soda were, however, retained, and, in addition, she was transfused with large quantities of normal saline solution.

Post-mortem examination: Thorax: Heart small, no naked-eye evidence of fatty change, valves competent, no pericarditis. Lungs: Pleura adherent at right base, marginal emphysema, otherwise no obvious morbid condition. Abdomen: No free fluid in peritoneal cavity, and no evidence of peritonitis. Liver: Weight 460 grm., slightly fatty appearance most marked in left lobe. Spleen shows nothing abnormal. Kidneys show evidence of some congestion, but otherwise nothing abnormal. Pancreas apparently healthy, also the suprarenals. Stomach and intestines: Slight congestion in areas, but otherwise nothing abnormal to be made out. Brain and meninges healthy; no sign of disease in brain, and nothing abnormal to be made out on cutting brain from end to end; middle ear on both sides healthy. Spine: Caries of the bodies of the third, fourth, and fifth dorsal vertebræ; no pressure on the cord.

Case III.

J. L., male, aged 4 years and eight months; died February 26, 1909; lymphosarcoma.

Family history: History of malignant disease on paternal side.

Previous health: Healthy child till aged 3. For about eighteen months prior to death was "uncertain in his feeding"; some days he took his food well, other days badly. He always looked healthy, but latterly (for a year) did not increase in weight and his bowels were irregular during this period. He occasionally took sick turns, but "no more than an ordinary child."

Present illness: Five weeks previous to death he was seen for a rash thought to be German measles, and at this time no swelling of the abdomen was noted. On February 10—i.e., sixteen days prior to death—

he was brought to the out-patient department of the hospital because of indigestion and listlessness; for two days his mother had noticed that his "stomach" was swollen.

State on admission: Weight 2 st. 5 lb. Well-developed and well-nourished boy; slightly anæmic appearance. Thorax well formed; abdomen very tumid; no evidence of rickets or syphilis; pulse regular; temperature normal. Alimentary system: Nothing abnormal in subjective phenomena. On palpating the abdomen the liver appears greatly enlarged; it extends 2½ in. below the costal margin in the right mammillary line; its surface is uneven and its edge uneven and nodular; near the middle line there is a distinct deep furrow in the organ, to the left of which again the left lobe of the organ can be palpated, the condition of which resembles that of the right lobe. Above the right iliac crest and between it and the umbilicus a distinct detached nodule of about the size of a pigeon's egg can be palpated. The rest of the abdomen appears clear of any special resistance, and the spleen cannot be palpated. There is no free fluid. Hæmopoietic system: Spleen not enlarged. Blood examination shows red blood-corpuscles, 5,130,000; hæmoglobin, 92 per cent.; white blood-corpuscles, 22,200; polymorphonuclears, 58 per cent.; lymphocytes, 40 per cent.; eosinophiles, 2 per cent. Nervous, circulatory, respiratory and integumentary systems: Nothing abnormal detected. Urinary system: Urine, copious deposit of urates; slight trace of albumin; no other abnormal constituent; Cammidge's pancreatic reaction positive.

Progress: Temperature, pulse and respiration continued normal; von Pirquet's cutaneous and Calmette's ocular reaction gave negative results. The patient gained in weight, he ate and slept well; he was, however, very discontented and anxious to get home, and, as his general health was fairly good and he ran about the ward freely, he was discharged from hospital on February 18. On February 25 he was readmitted to the ward, the abdomen being very greatly swollen and tense, and the liver considerably larger and harder, more particularly the left lobe. The boy was now obviously extremely ill; he complained of considerable pain in the abdomen, and had a completely felled appearance. The following day an exploratory incision was made with a view to relieve pain, but very little free fluid was found in the peritoneal cavity. The liver was seen to be studded with nodules of malignant growth, but palpation of the head of the pancreas revealed nothing abnormal. The nodules in the liver varied in size from that of a sixpenny-piece to that of a penny. They were of a greyish-white hue

with an encircling zone of marked congestion. The patient died the same evening.

Post-mortem report (Dr. Harvey Pirie): Well-developed boy; abdomen greatly distended; no trace of jaundice. Thorax: Heart healthy. Pleuræ: No adhesions, contain practically no fluid. Lungs: Both lungs are markedly congested and the base of the lower lobe of the right lung shows a patch of collapse. Abdomen: Greatly distended, and contains a considerable quantity of fluid blood and recent clot. Liver: Weight 4½ lb.; is greatly enlarged and is studded throughout with nodules of new growth; gall-bladder collapsed; bile-duct patent. Pancreas: Tail normal in appearance, body and head considerably



FIG. 5.

Case III. (Lympho-sarcoma.) Under surface of liver.

injected. There are numerous discrete, hard, enlarged glands in the posterior mediastinum and a large gland in the mesentery opposite a tumour in the gut. This tumour is annular in form and involves the wall of the ileum about 1 ft. above its lower end. The growth extends for about 1½ in.; it almost involves the whole circumference of the bowel, but does not greatly interfere with the size of its lumen. Kidneys: Enormously enlarged; weight 6 oz. each. Brain apparently healthy.

For the further notes on the microscopic appearances I am indebted to Dr. James Miller. Liver: Where the liver structure is still preserved there is an infiltration of the lobules and of the portal spaces and

subcapsular fibrous tissue with cells. This infiltration is not uniform, but occurs in distinct foci. The cells composing these foci vary very much in size and in appearance. Within the lobules these cells appear to be inside the capillaries in the earliest stage, but they may also be observed as the area enlarges, infiltrating and lying between the separated liver-cells. In the unaffected areas the liver-cells have a fairly healthy appearance, but where infiltration occurs the liver-cells disappear, giving one the impression that they are eaten into by the tumour-cells. These latter—for the cells of the infiltration areas are undoubtedly the same as the tumour-cells, as already indicated—vary much in shape and size. The predominant type is a large cell, round or nearly so, with a relatively large nucleus, staining deeply. The protoplasm is small in amount and finely granular. The nucleus is as a rule round, but may be indented and is not infrequently fragmented. In the smaller cells the nucleus stains more deeply. The cells appear to have only a short life, rapidly breaking down into granular debris. The secondary nodules show a few remains of liver elements, liver and endothelial cells becoming less numerous as one passes from the margin into the interior of the growth. The greater part is formed of cells varying in size and shape, the predominant type being a fairly large round cell, with a relatively large nucleus and small amount of protoplasm. Many of the cells show degenerative changes—all stages to the formation of amorphous granules. Mesenteric gland shows diffuse infiltration of follicles, sinuses and capsule, with medium-sized round cells. Kidney shows an entire separation of tubules and glomeruli from one another by immense numbers of rounded and polyhedral cells. The cells have the same characters as those in the liver. The cells of the tubules are granular and give evidence of a degenerative change. The vessels are numerous and dilated, but no hæmorrhages are to be found. The wall of intestine shows great thickening in places, due to aggregation of tumour-cells, which infiltrate all the coats. The pancreas shows a considerable amount of post-mortem change, and the substance is infiltrated with what appears to be a fibrinous exudate. There is no accumulation of round cells.

These three cases, while interesting individually, seem to me also to serve as a markedly interesting combination. Disease of the liver may be due to disordered metabolism, to the result of infection, or to both combined, the last probably the commonest condition, but the outstanding types being metabolic and infective. As representing the former, we have here the case of acute acidosis; as representing the latter, the case



FIG. 6.

Case III. Microphotograph of liver (low power).



FIG. 7.

Case III. Microphotograph of kidney (low power).

of subacute atrophy of the liver. The third case (lymphosarcoma), considered in conjunction with the known facts about the spread of tubercle in the liver and lung, seems to me to serve as an excellent object-lesson when considering the other two, for this reason—that the spread of these two conditions shows that, according to the constitution of different organs, there is variation to susceptibility to different morbid processes. In the same way, according to the constitution and functional power of any one organ—e.g., the liver—in different individuals there is a different reaction to any one given infection, group of infections, or group of dietetic errors.

For such reasons it appears to me to be rather straining at straws to mark off sharply as different diseases such conditions as subacute atrophy of liver and cirrhosis of liver, or, again, acidosis and cyclic vomiting, delayed chloroform poisoning and some varieties of chronic dyspepsia. M'Donald and Milne[1] state, very dogmatically, "Subacute liver atrophy must not be confused with ordinary liver cirrhosis," and they speak authoritatively, as men who have studied this subject deeply.

But to the mere clinician, hearkening to the words of wisdom uttered by the scientist in the *Journal of Pathology*, various small clouds of doubt appear on the horizon. The pathologist and the clinician are not entirely at one in their view of disease. The pathologist seeks the exciting cause, follows it in its course of derangement, notes the changes wrought by it, from his previous observations deduces the changes which must have led up to the final result, and defines as disease a certain train of events. Not so the clinician. He realizes that the pathologist is scientifically correct in his observations and his deductions, and consequently in his hair-splitting definitions of disease; but as his own essential functions are the prophylaxis and the treatment of diseased conditions he is unable to acquiesce from a practical point of view in such minute differentiations relating to the course and ultimate effects of the exciting cause. And in this particular instance he notes that in the first case reported by the authors previously quoted, the patient under notice suffered from jaundice two years previous to death, again fifteen months previous to death, and again six weeks previous to death, and that finally the authors state "from a study of the early type of fibrous tissue present in this case, and the extremely rapid hyperplasia of the liver-cells taking place uniformly throughout the liver-cell areas, we consider that the first two attacks of jaundice were of accidental

[1] *Journ. of Path. and Bacteriol.*, Camb., 1908-9, xiii, p. 161.

occurrence, and that the histological features of the liver could only be explained if the processes leading up to these appearances had commenced concurrently with an illness of perhaps two, or at the very most, three months' duration, the last forty-three days of which were associated with jaundice." Such a method of explaining clinical phenomena it is difficult to accept. Again, in all the five cases reported by the authors it is stated that practically no elastic structure, or at most but very little, was present in the fibrous tissue, a point which they appear to consider corroborates their view that subacute liver atrophy and ordinary cirrhosis are two distinct affections. But in my present case with a history of one year's illness the fibrous tissue abounds in elastic structure.

We are told that the pathological appearances in subacute liver atrophy are due to the damage to the liver being rapidly accomplished and completed, whereas in ordinary liver cirrhosis they are determined by small necroses being produced at very frequent intervals of time in the liver by destructive agents, possibly derived from the intestine. Even so, it is merely a question of comparative rapidity, and I fail to see that any just reason exists why the clinician should be bound to docket separately the two conditions, or why he should cease to regard toxæmic affections of the liver—affections multiple in their exciting cause and varied by the varying powers of resistance they encounter —as ultimately all conducing to a like condition of the organ, it may be more or it may be less quickly, but with transition stages all along the line and differing only in the changes natural to the rapidity of affection. Likewise a case such as Case II goes under the name acidosis, which is the name not so much of a disease as of the type of symptoms produced by a diseased condition, which, however, in other circumstances may produce markedly different types of symptoms.

I yield to no one in my appreciation of the value of pathological and laboratory work in clinical medicine, and I recognize that in very many cases it is only by such work that a satisfactory diagnosis of disease is at present possible, but I certainly contend that it is unwise for the clinician too slavishly to follow the pathologist's lead in his delimitation of disease.

Dr. F. PARKES WEBER said he thought the conclusions mentioned by Dr. Fordyce in regard to the pathological link between cases of hepatic cirrhosis and cases of subacute and acute atrophy of the liver, were practically acknowledged by most authors who had studied the question. But Dr. Fordyce had not alluded to the supporting evidence derived from experiments on animals.

Various substances had been found to produce in animals either cirrhosis of the liver or subacute or acute atrophy, according to dosage and the way they were given. In certain experiments in dogs, however, conditions of partial acute or subacute atrophy had been produced by injections of chloroform into the portal vein or hepatic artery. In other dogs, in which it might be presumed that more or less subacute or acute atrophy had been produced by the same means, recovery had taken place, and when they had been subsequently killed no cirrhosis of the liver had been found at the post-mortem examinations. Such experiments had been recorded by Whipple and Sperry in the September number of the *Johns Hopkins Hospital Bulletin*.[1] But that was no argument against the view that cases of acute and subacute atrophy of the liver were pathologically linked together in a series with cirrhosis. In the animals alluded to last the exciting cause was completely removed and perfect recovery took place. But if, instead of that, the action of the disease-producing agent was indefinitely prolonged, then if it was not sufficient rapidly to cause atrophy of the liver to the point of producing death, one might ultimately produce a cirrhosis of the liver, constituting the expression of a defensive reaction on the part of the organism.

A Case of Pneumococcal Infection in an Infant simulating Generalized Tuberculosis.

By Walter Carr, M.D.

The patient, a male child, aged 15 months, was admitted to the Victoria Hospital for Children, Chelsea, on March 10, 1908, with an indefinite history of illness for about a month, the chief symptom noted being cough. He had been first seen as an out-patient on February 18, when the temperature was 100·2° F., and there were signs of general bronchitis. There was no history of the child having been ill before or of any illness in the house.

On admission he was thin and extremely pale; temperature 103° F., pulse 140, respirations 44. There were dullness and tubular breathing over the upper lobe of the right lung, the signs being most marked in front and reaching to the fourth cartilage; a few very small crepitations were heard at the end of inspiration; elsewhere the lungs appeared normal. An examination of the blood showed red cells 3,500,000, white 24,000, hæmoglobin barely 70 per cent. The case at this time was

[1] *Johns Hopkins Hosp. Bull.*, 1909, xx, p. 278.

regarded as one of acute pneumonia in an anæmic and badly-nourished child. It is quite common in such children to have a history pointing to some bronchitis for a few days or weeks prior to the commencement of the more acute illness, and it is often impossible to find out, especially in infants, when the latter actually started. The child's temperature from the time of admission was most irregular, varying every day between 99° F. and 103° F., and occasionally falling to or below normal. This was not at first, however, regarded as in any way inconsistent with the diagnosis, for such a remittent or even intermittent type of fever is far from uncommon in cases of acute pneumonia, especially in children, which terminate quite satisfactorily, and the explanation of its occasional occurrence forms an interesting subject for discussion. However, as days passed into weeks and the child's condition remained unaltered, with a persistently irregular temperature and no sign of crisis or resolution, the probability that the disease might be tuberculous was more and more seriously entertained. On March 21 Calmette's ophthalmic test with tuberculin was tried, but the result was negative.

Towards the end of March the abdomen became fuller and more resistant, and on March 27 definite signs of free fluid in the peritoneal cavity were noted. The lung signs were also increasing on the right side, dullness and tubular breathing were extending down the right back, whilst at the extreme right base posteriorly there was deficient resonance with weak breathing. The child's condition now grew steadily worse, persistent vomiting and some diarrhœa developed, the quantity of free fluid in the peritoneal cavity steadily increased, crepitations were heard all over the chest, the signs of consolidation over the upper part of the right lung remaining unchanged, and there was some extension of the cardiac dullness upwards, though no pericardial friction was ever heard. The temperature was still very irregular, but gradually fell as the child's weakness increased. Death occurred on April 15, five weeks after the boy's admission to the hospital.

During the last two or three weeks the condition was regarded as undoubtedly tuberculous; for the persistence and extension of the signs of pulmonary consolidation, followed by signs of scattered bronchitis, suggesting a general tuberculous infiltration of the lungs, the development of symptoms and signs pointing strongly to tuberculous peritonitis, and the persistently irregular temperature, altogether presented a clinical picture which appeared to be perfectly typical of tuberculosis. The upward extension of the cardiac dullness was regarded as possibly due to caseation of the tracheal glands. At the autopsy, however, no

trace of tubercle was found anywhere, and cultivations made by Dr. Ledingham, of the Lister Institute, proved that the condition was probably one of pure pneumococcal infection. There was well-marked pericarditis; the pericardium was thickened with universal and fairly recent adhesions all over, and the cavity contained about ½ oz. of semi-purulent fluid. The myocardium and the valves were normal. The right lung was universally adherent by firm, dense adhesions difficult to break down; the upper lobe showed evidence of old, unresolved pneumonia; the rest of the lung was quite collapsed. The left lung was also adherent all over, but by adhesions less dense than those on the right side; the lung tissue was extensively collapsed, and the lower lobe showed slight evidences of bronchitis.

The peritoneal cavity contained about a pint of semi-purulent fluid, with flakes of lymph all over attached to the different organs. The intestines were matted together by very recent adhesions, easily broken down. There was no ulceration of the intestines and the appendix was apparently perfectly normal. The liver, spleen, and kidneys showed nothing abnormal.

DISCUSSION.

The CHAIRMAN (Dr. Sutherland) said the communication was a most interesting one, and he thought probably most members would agree with the author's final conclusion—namely, that if one were to meet with a similar case he would diagnose tuberculosis. The history of such a case of pneumococcal infection was very interesting, because although physicians were getting more to recognize the nature of the infection and the local complications which followed pneumonia, he thought the present was a very unusual type of case, showing as it did gradual extension of the disease to both lungs, with pleural adhesions, and involvement of the peritoneum. As there was fluid in the peritoneum, he asked whether it occurred to Dr. Carr to explore the peritoneum.

Dr. CARR, in reply, said that if the disease had been limited to the abdominal cavity he would have had the abdomen explored, and no doubt it would have cleared up the diagnosis in the case; but the general condition of the patient was so bad, and the disease in the lungs so extensive, that any operative procedure was out of the question. Tapping the abdominal cavity instead of freely opening it he regarded as dangerous where it was suspected that tuberculosis might be present, because—owing to the possibility of adhesions—one never quite knew what one was going into, and a coil of intestine might easily be perforated.

Specimens from a Case of Lymphosarcomatosis, with Clinical and Post-mortem Notes.

By F. W. Higgs, M.B.

Specimens: (1) Appendix, with adjacent intestine. The appendix is much enlarged, with bulbous extremity, containing a sessile mass of growth arising in submucous layer and filling the lumen of the distal bulbous portion. Subperitoneal masses of growth in adjacent colon and ileum. No growth in submucous layer of the two latter. (2) Kidneys show diffuse growth and hydronephrosis. (3) Bladder shows peritoneum of posterior wall extensively infiltrated with growth. Ureters are dilated owing to obstruction at lower end, produced by growth in wall of each ureter in its lower 1 inch.

Microscopical sections: (1) Growth from appendix—lymphosarcoma. (2) Kidneys showing diffuse interstitial infiltration with lymphosarcoma cells, also advanced granular degeneration of tubal epithelium. (Sections of infiltrated abdominal wall, bladder-growth, and mediastinal growth were also examined, and showed the same type of growth.)

Clinical notes: Male, aged 3; onset of illness with abdominal pain and vomiting, six weeks before admission to hospital. Pain in abdomen continued; vomiting returned four and a half weeks later. The doctor who examined patient then found mass in appendix region. Later hypogastric mass was also felt. No previous illness.

On admission to hospital: Poorly-nourished child. Abdomen: Much distended and tympanitic; no free fluid. Large mass felt extending across lower abdomen; also rounded mass to right of umbilicus. Chest: *Nil* abnormal. Urine: Cloud of albumin, a few epithelial and pus cells; no casts; no tubercle bacilli.

Course: Temperature was intermittent (97-101° F.). Abdomen became more distended and signs of free fluid appeared. Patient rapidly became worse, and died three weeks later (i.e., nine weeks after onset of symptoms). Several days before death dilated veins appeared in anterior thoracic wall.

The diagnosis made was tuberculous peritonitis, with matting of omentum and gut; the venous obstruction in the chest being thought to be due to pressure by tuberculous mediastinal glands.

Post-mortem examination: Wasted body. Abdomen: Intestines

greatly distended; peritoneal coat thickened, especially at mesenteric border. Large hard white rounded sessile subperitoneal masses in connexion with ileum, cæcum, and commencement of large gut. No intestinal ulceration. Appendix: 5 in. long; walls thickened throughout by diffuse growth. Distal one-third, much enlarged and bulbous, and containing a mass of growth of 2½ in. long, apparently arising in lymphoid tissue of submucous layer. A little muco-pus present also. The omentum formed a thick apron of growth adherent to vesical region, and also to anterior abdominal wall, the peritoneal surface of which was widely thickened by diffuse growth. Kidneys: Large (right 5 oz., left 5½ oz.) and pale, like *large white kidneys*, with irregular lobulation on the surface; evidently affected throughout with a diffuse neoplasm; on section considerable dilatation of the pelvis and calices is seen. Ureters: Some dilatation due to obstruction by growth which is present in the wall in the lower 1½ in. of each. Bladder: The posterior wall is a solid mass of lobulated growth about ½ in. thick; epithelial surface is normal. Liver: Normal. Spleen: Normal. Mesenteric glands contain hard, white growth. Chest: A sheet of growth ½ in. thick involves the whole anterior surface of the pericardium and is adherent to the anterior chest-wall. The internal aspect of pericardium is not involved, nor are the mediastinal glands. Heart, lungs, and pleuræ normal. Brain and meninges normal.

Note.—The fact that clinically the disease commenced in the cæcal region, and that pathologically the appendix appeared to be the only organ in which the disease occurred in the submucous layer (the situation of normal lymphoid tissue), suggests strongly that the growth commenced in the appendix.

I am indebted to Dr. R. S. Trevor for the microscopical specimens.

A Case of Chloroma.

By W. Essex Wynter, M.D.

W. M., female, aged 3¼. Other members of family are healthy, but an aunt and uncle died of pulmonary tuberculosis. The patient had measles at end of December, 1908. She has always been considered delicate, and has been attending in the out-patient department on account of loss of appetite and debility since January. The facial

deformity has only been noticeable since the middle of May, and has gradually developed into the state now exhibited. The face is unusually large and bony, with shiny, tense skin over the malar bones, with marked exophthalmia, especially on the right side. The palate is narrow and highly arched, with median groove and lateral bulging. The child appears of average intelligence and is free from pain. The glands about the neck are enlarged. The liver can be felt below costal margin. Temperature slightly above normal. Blood count: Red corpuscles, 3,630,000; white, 30,000; hæmoglobin, 56 per cent.; lymphocytes, 66·2 per cent.; hyaline and transitional cells, 4·8 per cent.; polymorphonuclears, 27 per cent.





A case of Chloroma.

FIG. 1 (June, 1909). FIG. 2 (September, 1909).

DISCUSSION.

Dr. ESSEX WYNTER said that, if his diagnosis were correct, it was an instance of a very rare condition, and very few attempts had been made to diagnose such cases before death. The case was brought up because of general weakness and anæmia. His colleague, Dr. Young, suggested it might be a case of chloroma, and the blood count seemed to support that. So far, all such cases had proved fatal in a comparatively short time, and he believed the present child would scarcely live out the year. The eyes protruded and there was marked papillitis in both. There seemed to be nothing abnormal about the rest of the body except the head. He believed that in the former cases which had

been shown the long bones were never affected, though occasionally the ribs as well as the face bones were involved. His only suggestion as to treatment was to try Coley's fluid. The child had been given six injections of it, at intervals of some days, beginning with $\frac{1}{4}$ m. There was no response after the first four doses, but afterwards there was a reaction, with fever and vomiting. But there was no improvement and no arrest of the growth, and so he did not press the treatment. Neither did large doses of arsenic act beneficially.

The CHAIRMAN (Dr. Sutherland) said he did not recall that the Section or the Children's Society had had such a case before it until now. If the present case were not one of chloroma he did not know what it was, as he supposed secondary suprarenal tumours could be excluded.

Dr. WHIPHAM asked where the growth commenced and whether the injections of Coley's fluid were given where the enlargement was first noticed?

Dr. E. I. SPRIGGS said, as to the lymphocyte proportion, that it was common to get 50 per cent. in hospital children. He remembered a case of chloroma which had some of the features of the present one, but it had others not possessed by this: for example, patches of œdema about the legs. He had a case, in the Victoria Hospital some months ago, of a variety of leukæmia in which the child had a thickened scalp of a dull green slate colour, with bulgings in the temporal region, but none of the eyes; there were enlargements in the long bones, and the case proved not to be one of chloroma.

Mr. SYDNEY STEPHENSON said he recalled a case which was under Dr. James Taylor at the Queen's Hospital for Children, of a child suffering from secondary deposits from suprarenal sarcoma. And in that case, contrary to what one of the speakers had said, both orbits were involved and both eyes were thrust forward.

Dr. ESSEX WYNTER, in reply, said the lymphocytes embraced an equal number of the large and small varieties, and they were 60 per cent. of the whole white count. The growth seemed to have been from the commencement quite symmetrical, and he thought it was first noticed in the malar region on both sides. The proptosis on both sides commenced soon after the malar swelling, and the whole development had been almost perfectly symmetrical. One or two knobs could be felt at one time in the left temporal region, as well as indurations, which could be moved over the bones overlying the malar prominences, but he could not detect those now. The Coley's fluid was injected into the malar prominences, but there was no benefit. He did not feel comfortable about puncturing the growths because of the fear of either distributing the disease or causing undue hæmorrhage.

Two Cases of Acute Anterior Poliomyelitis involving the Four Limbs.

By A. MANUEL, M.B.

Case I.—W. C., aged 1 year and 8 months, was admitted to the Victoria Hospital for Children on September 14 with the following history. On the morning of September 8 the child was in perfect health; in the afternoon he fell down a flight of stairs without apparently sustaining any injury, "as he continued to play afterwards"; in the evening he vomited once, and subsequently spent a restless night. On the following day the child was very irritable and fretful, but it was not until the next morning that the mother first noticed that he was unable to move his legs. Four days later the child suddenly lost the use of his arms, and was then brought up to hospital. When first seen the patient's temperature was 100° F., and all four limbs were in a condition of flaccid paralysis. Feeble flexor movements of the fingers and toes could be elicited on mechanical stimulation of the palmar and plantar surfaces, and in the case of the left arm slight flexion at the wrist-joint was also observed. Respiration was normal, and there was no involvement of trunk muscles. Since the boy has been under observation in hospital the condition of the left arm has so far improved that he is able to hold a spoon and attempts to feed himself, but that of the right arm has not appreciably changed. The extensors of leg and foot remain completely paralyzed on both sides and give no reaction to faradic stimulation, with the exception of the peronei on the right side: the flexors are paretic, but respond to electrical excitation. Definite wasting of affected muscles has occurred, but the general health remains good.

Case II.—A. G., aged 1, was brought up to hospital on October 2, six weeks after the onset of the present disease. The mother gave a definite history of sudden illness without any previous indisposition on August 15, and stated that the child lost complete use of both arms and both legs on the same date; she also volunteered the information that at the same time "the head fell back" and "the jaw dropped," this condition persisting for two or three days. When seen on admission there was nothing in the appearance of the patient to suggest paralysis or paresis of any of the facial muscles. At the present time the right arm appears to have recovered all movements except that of supination, and when

the biceps is stimulated electrically the forearm is flexed in a position of pronation. In the case of the left arm and forearm feeble flexor and extensor responses are obtained on faradic excitation, but no voluntary movement, except flexion of the fingers, has been noted. In both lower extremities there is complete paralysis of all the extensors of foot and leg; the flexors, however, remain intact, and on the left side a very well marked foot-drop has developed partly as a result of their unopposed action. The child takes well and the general nutrition is satisfactory.

Dr. Manuel added that in both cases the abdominal reflexes were obtainable on the left side, which pointed to some involvement of the centre affecting the abdominal muscles, but there had been no undue abdominal distension.

Case of Extracapsular Fracture of Neck of Thigh-bone in Boy aged 6.

By Ralph Thompson, F.R.C.S.

On August 21, 1909, the boy was admitted into the Victoria Hospital with a history of having fallen from a height of 10 ft. He had sustained a small contused wound of his forehead. He also complained of pain in his left hip. The lower limb was slightly flexed at the hip and knee-joints and rotated outwards; it was also slightly abducted. The movements at the hip-joint, both voluntary and passive, were practically *nil*. No other signs of dislocation were made out. The diagnosis lay between acute synovitis of hip, separated epiphysis of the head of the femur, and an extracapsular fracture. A skiagram revealed nothing, and a diagnosis of acute traumatic synovitis was made. Ten days later the boy was allowed to get up, as he could move his hip; but, on putting his foot to the ground, he complained of much pain. A second skiagram was taken and an extracapsular fracture was found. Simple extension was applied, and on October 16 the patient was ordered a Thomas's hip splint. There is no shortening at present.

Mr. Lockhart Mummery said it must be a very rare condition, as practically no text-books on children's diseases spoke of it. But every one must have met with cases of deformity of the hip-joint in which X-ray photographs showed a certain amount of deformity, and which had been difficult of explanation. He thought the explanation put forward by Mr. Corner was the

correct one—namely, that they were probably extracapsular fractures of the femur which had not been detected, and in which union had occurred with deformity. He saw one child with a curious deformity in the right hip, which it was concluded must have been fracture of the neck of the femur two years previously, which had not been properly attended to, and in which deformity occurred.

Congenital Hemidystrophy.

By J. Howell Evans, M.Ch.

F. G., aged 1 year and 10 months, a male infant, was brought to the Prince of Wales's General Hospital on January 20, 1908, when only one month old, because his parents thought that he had a birth-mark on the left side of his tongue. The clinical appearances of the tongue were those of unilateral macroglossia, the left side being larger and more red than the right. When the child was about twelve months old asymmetry of the abdomen was observed, and a more careful survey showed some difference in the upper limbs and marked difference in length and size of the lower limbs.

The interest lies in the antenatal causation of this congenital anomaly; even if the term congenital hemidystrophy is wrong, and should be, in this case, replaced by congenital hemihypertrophy, the causation is enshrouded with the darkness which envelops the majority of questions on antenatal pathology.

DISCUSSION.

Dr. F. Parkes Weber said that he considered the case to be one of hemihypertrophy, but that he thought that such cases of hemihypertrophy and cases of hemiatrophy were both included under the term "hemidystrophy," employed by Mr. Evans. In regard to the enlargement of half the tongue (hemimacroglossia), he supposed that it was neither of neuromatous—that is to say, neurofibromatous—nor of lymphangiomatous nature, but that it was connected with genuine hypertrophy of the muscles of the part.

Mr. Lockhart Mummery said that two years ago he showed a case before the Children's Society which he called hemihypertrophy. There were a certain number of cases in which one arm or leg was affected. He found eleven or twelve cases recorded in literature. He had watched the condition in his case for three years, and it had undoubtedly progressed; the child looked 1½ years

older on one side than on the other, the boot on one side had to be $1\frac{3}{4}$ in. higher than the other, and there was marked hypertrophy on the left side of the tongue. He thought the boy would be unable to walk in two years' time.

Dr. HIGGS said his impression was that the term *dystrophy* rather indicated an increase in size of the part with diminution of function. He himself preferred to call the present case *hemihypertrophy*.

Case for Diagnosis.

By J. P. LOCKHART MUMMERY, F.R.C.S.

THE patient was a boy aged $6\frac{1}{2}$. When ten months old he had a severe attack of whooping-cough, and after this the mother noticed the feet were "drawn down" and the child developed a squint. He had never been able to walk properly. He walked on the tips of his toes and with a kind of staggering run. He could only stand still with difficulty and by stooping right forward. The gait was markedly spastic. Both Achilles tendons were contracted, and the feet could not be brought to a right angle. The muscles showed no paralysis on electrical test and there was no other deformity. The child was mentally quite intelligent. There was a marked squint. Mr. Mummery proposed to divide the tendo Achillis to enable the boy to get his heels to the ground, and expected a marked improvement in the gait would result from the operation.

DISCUSSION.

Dr. SPRIGGS said the case reminded him of one shown by Professor John Thomson at Edinburgh. The boy had a spastic gait and always walked on the toes. After running a few yards he tended to fall into somebody's arms. Mr. Mummery said that was the case with this boy also in hospital. Professor Thomson seemed to regard his case as one of muscular hypertonia of obscure causation.

Dr. JEWESBURY asked whether it might not be a case of cerebral diplegia? He found the reflexes were exaggerated, and especially the knee-jerks, Babinski's sign well marked on both sides, although ankle clonus could not be obtained. He thought the same type of gait occurred fairly often in cases of cerebral diplegia in children, and the spasticity was typical of that condition.

Dr. HIGGS said that he did not find anything wrong with the upper extremities. He regarded the case as one of spastic paraplegia due to

involvement of the pyramidal tracts supplying the lower limbs on both sides. He thought the action of the patient in walking was due to his having, owing to the extreme spasticity, to walk so far forward on his toes, and his consequent inability to balance himself satisfactorily.

Mr. MUMMERY replied that there was no mental defect; the mother said the child was intelligent and the nurse said it was really sharp. But there was a squint, which had dated from the leg condition. Until he brought the patient he had not realized that there was anything the matter with the cord. He proposed to divide the tendo Achillis on both sides and bring the feet into proper position, to enable him to walk on his heels. It had been pointed out by one member that the proper treatment for flat-foot was to tell the patient to walk on the tips of the toes and thus develop the muscles. But this boy had marked flat-foot, though he had been walking on the tips of his toes for four years.

A Case of Acute Inflammation in an Appendix, involuted into the Lumen of the Cæcum, which then formed the Apex of an Intussusception; Operation; Recovery.

By IVOR BACK, F.R.C.S.

F. L., A GIRL, aged 8, was admitted to St. George's Hospital on September 27, 1909. Fourteen days previously she had pain in the left half of the abdomen, which lasted three days, returned again three days later—this time in the hypogastrium, and then persisted up to the time of admission. The pain was paroxysmal in nature. She vomited twice at the onset and three times shortly before admission. The bowels were open daily without purges. She had had no previous illnesses.

On admission at 10 a.m. on September 27 her temperature was 97·5° F., and her pulse 106. The lower abdomen was distended and a hard tender mass could be felt stretching across the epigastrium. No peristalsis was seen then. At 1 p.m. she began to vomit, and continued to do so. I was called at midnight, and then saw the case for the first time. The temperature was then 96° F., pulse 130. Peristalsis of small intestine type visible in centre of abdomen. Rectal examination revealed no abnormality. At 12.30 a.m. on September 28 she passed a large normal stool. At 1 a.m. I opened the abdomen by a vertical incision. I found the mass in the epigastrium was an intussusception whose apex was in the descending colon. I reduced this with some

difficulty. There was no strangulation, but the cæcum was œdematous, and inside an oblong body could be felt. No sign of an appendix was seen outside the cæcum. The cæcum was opened, and the oblong body proved to be an acutely inflamed, involuted appendix. This was removed. The cæcum was closed with two layers of silk, a tube was put down to it, and the wound sewn up. She made an uneventful recovery.

The appendix, which is on view, was opened up longitudinally, and shows the involution of the proximal two-thirds. The distal third twists back again and lies inside the proximal portion. The photomicrograph of a section of the base of the appendix shows the mucous membrane outside and the serous coat inside. I am indebted for the photograph to Dr. Harold Spitta, who is in charge of the Peirse-Duncombe Laboratory at St. George's Hospital.

DISCUSSION.

Mr. BACK said he hoped some members would be able to say what was the sequence of events in that complicated abdominal history. It had been suggested to him that evening that it was possible the appendix was involuted into the cæcum and then formed the apex of the intussusception, and that the subsequent interference with its blood-supply was responsible for the changes which were found in the organ. He himself regarded it as a case of acute inflammation in an appendix, congenitally involuted, and considered that the intussusception was a secondary affair.

Mr. LOCKHART MUMMERY regarded the case as unique. He could not say what the order of events was, but Mr. Back's explanation seemed as likely as any. The puzzle was how the appendix came to be inverted into the cæcum, because it was turned inside out, so that the mucous membrane was outside and the peritoneum inside. Mr. Back said he thought it was congenitally inverted, but he (Mr. Mummery) thought it must have been inverted by some process after it was developed. It was really a surgical puzzle.

Mr. BACK replied that when he said he thought the appendix was congenitally inverted he meant that he presumed it lay in the cæcum before any inflammatory or other change occurred; but he did not know how it got there. He had not seen another appendix even partially involuted into the cæcum.

Tuberculosis of the Tonsil, associated with Tuberculous Glands of Neck.[1]

By E. Scott Carmichael, F.R.C.S.Ed.

It is still a much-vexed question as to what are the commonest channels of primary tuberculous infection, and in what way the primary focus becomes the source of subsequent secondary and more generalized infection. Much has been written on the question of the relative frequency with which infection occurs through the respiratory and alimentary tracts, and opinion would seem to favour the former as the more common source. That the mouth, nasopharynx, and alimentary tract generally are a very frequent source of primary infection has been insisted upon by Woodhead, Sidney Martin, and others.

Woodhead, in a paper on the "Channels of Infection in Tuberculosis," gives weighty evidence to show that much of the tuberculosis in children, even of the lungs, is due rather to ingestion infection than to inhalation. This he has proved from feeding experiments performed by himself and from observation on the lower animals. He has traced tuberculous infection by way of the mesenteric, retroperitoneal, and bronchial glands and lymphatics to the lung, so that all infections of the lung must not necessarily be considered primary in origin. Again, he draws attention to the importance of an ingestion infection of the upper part of the alimentary tract when he says, in referring to the frequency of tuberculous glands in the neck in children, "I am driven to the conclusion that this method of infection of the glands through the tonsil must be of comparatively frequent occurrence, especially in children living under insanitary conditions and subjected to various devitalizing influences."

Sidney Martin [16] has demonstrated that in pigs in which the faucial tonsil is very large ingestion experiments produced tuberculosis in the cervical glands, and he draws attention to the fact that in man chronic enlarged tonsils are often considered to be a predisposing cause of tuberculosis, and he believes that his experiments go far to prove the ætiology of strumous glandular enlargement in children.

Shennan and Still, in investigations on the proportion of ingestion and inhalation infections, from post-mortems conducted in the Edinburgh

[1] Read at the meeting of the Section held in Edinburgh on June 18, 1909.

and Great Ormond Street Hospitals for Sick Children, present results strikingly similar. Shennan states that in 355 post-mortems on tuberculous subjects "he was able to make out the primary focus in 331 cases; 93, or 28·1 per cent., had to be referred to alimentary or ingestion tuberculosis. In all these cases the bacilli had entered by some part of the alimentary tract, including the pharynx and tonsils." This agreed practically with Dr. Still's percentage—namely, 29·1 per cent.

When one considers the anatomical position of the tonsil, exposed as it is to double risk of infection from without—namely, by contaminated air and infected food—one cannot but be surprised that it should remain so free of tuberculous infection as the literature and text-books on the subject would have us suppose. Again, the form of the tonsil, with its deep crypts, offers a ready nidus for the stagnation of food and lodgment of organisms of all kinds. Many of the best-known text-books on diseases of the throat and nose, even in the later editions, either do not refer to tuberculosis of the tonsil at all or treat of it as of rare occurrence, and it is a condition which does not seem to have attracted the attention of surgeons. Pathologists have shown us that tubercle of the tonsils is by no means such an uncommon condition as was formerly supposed, although Virchow [25] himself, within the last forty years, stated that tubercle of the tonsil had not been observed, and Wilks, Coats, and Osler, in their text-books, consider tuberculosis of the tonsil to be a rare condition.

More recently, many observers in post-mortem examinations of the tonsil have proved it to be one of the organs most commonly affected by tubercle secondarily.

Strassmann [24] examined the tonsils microscopically in twenty-one subjects who had died of tuberculosis. In fifteen of these the patients had died of pulmonary tuberculosis, and in thirteen of these he found that the tonsils, although macroscopically apparently healthy, microscopically were affected with a miliary tuberculosis, and in one case there was an associated cervical glandular enlargement. In six others examined, where death had occurred from tubercle of the kidneys, bowel, &c., the tonsils were found to be healthy.

Kruckmann [13] also found that in twenty cases of death from lung tuberculosis the tonsils were tuberculous in twelve. In his cases there was also enlargement of the cervical glands.

Walsham [27], in thirty-four consecutive post-mortems on tuberculous subjects, found the tonsils to be affected in twenty, all of them secondary to a lung affection. He also examined some removed

by operation, and, without stating the number examined, has never found a tuberculous affection of tonsil during life. He has examined many portions of hypertrophied tonsils, without finding any trace of tubercle.

Other authorities, such as Cornil and Ranvier [4] and Glas [8], have corroborated the above observations, from which it may be stated that secondary tuberculosis of the tonsil is of very common occurrence, affecting, as it does, the tonsil in from 60 per cent. to 80 per cent. of those persons dying of pulmonary tuberculosis.

Koplik [12], as the result of much investigation, considers that, while secondary tuberculosis of the tonsil is common, a primary affection is of rare occurrence. This would seem to be the opinion most generally held. The reason probably is not far to seek, in that tubercle of the tonsil alone, giving rise, as it does, to no gross pathological change, is frequently overlooked, or rather is not looked for, and not even when a secondary spread has taken place to cervical glands is the tonsil suspected of being the primary source of infection.

Primary tubercle of the tonsil is by no means so rare as is generally supposed, and the importance of the recognition of it as a primary focus cannot be too much insisted on. The systematic histological examination of tonsils removed by operation during life does not seem to have received the attention it deserves, the result being that the tuberculous condition is overlooked at a time when it is of vital importance to the patient. More or less accidental cases have from time to time been recorded.

Orth [18] seems to have been the first to have recorded a case of primary affection of the tonsil, while Abraham and Kendal Franks [1] and Purves Stewart [23] have published isolated cases, and Ruge, Schlesinger [22], and Glas [9] record tuberculous tonsils removed during life. Piffl [19], in a summary of all published cases, states that of 764 tonsils removed during life, and in which tuberculosis was looked for, thirty, or 4 per cent., were affected.

The following observers have examined microscopically tonsils removed by operation for hypertrophy:—

Lermoyez	...	in	32	excised found	5	tuberculous
Broca ...	...	,,	100	,, ,,	0	,,
Gottstein	...	,,	33	,, ,,	4	,,
Pluder and Fischer		,,	32	,, ,,	5	,,
Piffl ...	...	,,	100	,, ,,	2	,,
Goure ...	...	,,	200	,, ,,	0	,,

Dieulafoy [6] and Latham [14] have obtained positive results by inoculation experiments on guinea-pigs, the former in eighteen out of sixty-one experiments, the latter in seven out of forty-five. They omitted, however, to examine the tonsils microscopically before inoculation.

More recently Kingsford [11] and Friedmann [7] have made observations in children. Kingsford examined seventeen tonsils removed from children post-mortem, varying from four months to nine years of age. In all there was enlargement of the cervical glands. He found seven to be tuberculous, of which three showed signs of tuberculosis to the naked eye, in one there being ulceration, in one scarring, and in a third a caseous focus. In five in which the tonsils were hypertrophied only two were tuberculous, the remaining tuberculous tonsils being normal in size. This point concerning the size of the affected tonsil will be referred to later. In only one did he consider it primary, that of a child eleven months old, with tuberculosis of the left lung, left bronchial and left cervical glands. The left tonsil was markedly tuberculous, and may have been primary. The other cases he believes to have been secondary, five by the blood stream and one by the sputum. He further examined two hypertrophied tonsils removed by operation, with negative results; and he sums up by stating that in 200 cases of hypertrophied tonsils which have been examined and reported upon, only two were tuberculous, and therefore he considers primary tubercle of the tonsil in operated cases so very rare, only occurring in about 1 per cent.

Friedmann has recently made a large number of observations on the tonsils of young children under five years of age, believing that they are the point of entrance of the tubercle bacilli, which become distributed through various parts of the body. He examined the tonsils in fifty-four cases of extirpation during life, and in ninety-one post-mortem. In the latter cases, forty of the subjects had died of tuberculosis chiefly of the lungs, and the tonsils of seventeen of these showed marked tuberculosis, while in many of the others the appearances were highly suggestive of it. Of the fifty-four extirpated tonsils which were removed for chronic hypertrophy, Friedmann only found tubercle in one case.

From these observations, it would appear that primary tubercle of the tonsil occurs only in 1 to 2 per cent. of cases, in which the tonsil is hypertrophied.

The following observations have been made by examining tonsils removed by operation in the out-patient department of the Royal

Hospital for Sick Children, with the object of ascertaining in what proportion of cases tuberculosis of the tonsils co-existed along with tuberculosis of the upper carotid set of cervical glands. Cases were selected in which there was some enlargement of these glands. In examining the cases passing through the surgical out-patient department one cannot help being struck with the large proportion of children affected with enlarged glands in the neck who otherwise have no obvious signs of tuberculosis and are, except for the local condition, apparently healthy. Out of 2,626 new cases treated in the out-patient department during the last year, 208 children, or 8 per cent., suffered from tuberculous cervical glands alone. If that is so, one must look upon tuberculous cervical adenitis as a first stage in the spread of tuberculosis, and one naturally inquires as to the source and channel of infection of the glands. If we consider for a moment the glands that are most frequently affected, we are struck by the fact that there is one gland in the neck which more than any other is liable to become tuberculous. This gland is situated below the posterior belly of the digastric, upon or close to the junction of the common facial and internal jugular veins.

According to Poirier, the collecting lymphatic trunks are of three sets: a superior, draining the vault and lateral walls of the nasopharynx and opening into the lateral retropharyngeal glands; a middle set, which drains the tonsils and adjacent mucous membranes, and, piercing the constrictor muscles, ends in one or two lymphatic glands situated beneath the posterior belly of the digastric; and an inferior set which drains the inferior portion of the pharynx and opens into the internal jugular glands below the digastric.

In examining the milder forms of glandular enlargement, it is the gland beneath and immediately below the posterior belly of the digastric muscle which is enlarged. We see, then, that the gland which probably more commonly than any other is affected by tubercle is that one into which the lymphatics of the tonsil drain. The frequency with which this gland becomes tuberculous leads us to expect that the tonsil through which the bacilli probably pass in order to reach it becomes itself more often diseased, and that more careful examination would lead us to obtain a higher percentage than that given by Kingsford, Friedmann, and others.

We have examined the tonsils removed in the surgical out-patient department of the hospital in a series of fifty cases. The patients, in most instances, were not brought because of the throat affection, but for

the glandular enlargement. The degree of glandular enlargement varied very considerably. In thirty-seven of these cases the enlargement was slight: a gland, or at most two or three glands, situated a little below angle of jaw, beneath the anterior border of sterno-mastoid, being affected, and their size varying from that of a small pea to a marble or pigeon's egg. The microscopical examination of these cases showed that in two instances (Nos. 14 and 28) the tonsil on the side corresponding to the glandular infection was tuberculous.

Case I (No. 14) was that of a boy, aged 5, who came up to the out-patient department with a small tuberculous sinus of some months' duration, under which a small gland could be detected, on the right side of the neck. The tonsils were not enlarged, that on the right side—the affected side—being distinctly the smaller of the two. Microscopic examination revealed the smaller tonsil to be markedly tuberculous. Giant-cell systems were scattered throughout the tonsil, but were more numerous around the bottom of the crypts and under the surface epithelium of the tonsil. Tubercle bacilli were also found present in its substance. The tonsil, on removal, except for being smaller than the opposite tonsil, presented no naked-eye signs of tuberculosis. There were no other signs of tuberculous disease in the boy. An examination of the lungs was negative, except for some increase in the vocal resonance at the right apex, anteriorly and posteriorly.

Case II (No. 28).—Also that of a boy, aged 5, with a superficial abscess on the left side of the neck, arising from a small jugulo-digastric gland. The right tonsil was large, but the left small, somewhat pale, and flat. Both tonsils were removed. The left, or smaller tonsil was tuberculous, isolated giant-cells being distributed throughout its substance, more especially in the deeper part. There was no other evidence of tuberculous disease.

These two cases are of interest, in that they show that considerable affection of the tonsil may occur with but slight glandular enlargement. In the remaining thirteen cases there was extensive glandular affection of the cervical glands, the mass filling the carotid triangle and extending backwards beneath the sterno-mastoid into the posterior triangle as far as the trapezius. Of these, the tonsil on the corresponding side was found to be tuberculous in no fewer than five cases (Nos. 4, 5, 6, 34 and 46). The children in these cases were aged $5\frac{1}{2}$, $6\frac{1}{2}$, 8, 8 and $6\frac{1}{2}$.

Case III (No. 6).—The affected tonsil in this patient was small, and on section presented one typical giant-cell, the parenchymatous tissue being more fibrous than usual. The child showed no other evidence of tubercle.

Case IV (No. 4).—A girl, aged 8, with extensive glandular infection of the right side of the neck, and periadenitis. Both tonsils were hypertrophied and apparently healthy on removal. The tonsil on the affected side was tuberculous, the infection being localized to one lobule and not extending in the

sections examined throughout the tonsil. There were slight dullness and bronchial breathing at apex of right lung.

Case V (No. 5).—A girl, aged 5½; large glands on left side of neck, with some smaller on the right side. Both tonsils, which were large, were removed, and it was noticed that they were extremely soft and friable. The left tonsil was freely studded with giant-cell systems, which were most numerous around the sides and bottom of a crypt. General health was otherwise good; no chest affection.

Case VI (No. 34).—A girl, aged 8, with extensive glandular enlargement on both sides of the neck. Both tonsils were enlarged and were removed. An extensive general tuberculous infiltration of the tonsil was present in this case, the germ centres being in many parts quite indistinguishable.

Case VII (No. 46).—Girl, aged 6½, with extensive enlarged glands on the right side of the neck and periadenitis. Tonsils of medium size, removed, apparently healthy to the naked eye. The tonsil on the affected side showed, microscopically, considerable tuberculous infiltration in one part of the organ.

The examination of these cases has shown us that tubercle of the tonsils removed in persons during life is much commoner than observations of other authorities would have us suppose, seven of the cases out of fifty examined showing undoubted tuberculous affection, giving a percentage of 14. No serial sections were made of the tonsils, but two or three sections, at most, from each tonsil. It is probable, therefore, that if systematic serial sections were made of these organs, a larger percentage of tuberculosis would be found than even our figures represent. In none of these cases was there any appearance in the tonsil to lead one to suspect the presence of tubercle, either on examination of the throat or on naked-eye inspection of the tonsil after removal. This coincides with the view of Piffl, "that macroscopically there is no means of differentiation between tuberculous and non-tuberculous tonsils," and few cases are recorded in which tubercle could be recognized by the naked eye. Hofmann describes a tuberculous ulcer of the tonsil, and Latham also reports a case of ulceration of the tonsil, associated with tuberculous glands in the neck, and Friedmann, in his cases, found three with evidence of tubercle. The size and shape of the tonsil does not seem to have any special relation to the tuberculous infection. In Cases I, II, and III the tonsils were small, and even atrophic, and of firm consistence; in Cases IV, V, and VI they were hypertrophied, while in Case VII the tonsil was of medium size. If we compare these with the size of those described by Strassmann, we find that out of fifteen of his cases the tuberculous tonsils are described as

being atrophied in five cases, of medium size in eight cases, and hypertrophied in only two cases. In none of his cases was there any external evidence of tubercle.

Chiari believes that superficial ulceration of the tonsil is more common in secondary than in primary affections of the tonsil. Are we to consider the tuberculous deposit in these cases of primary or secondary nature? A thorough examination of the chest was made in these cases, and in five there was entire absence of any lung affection. In Case IV there were slight dullness and increase of vocal resonance at the apex of the right lung, but no signs of any active tuberculosis. A similar condition was found in Case I. There was no sign of tuberculosis in other parts of the body. If it is affected in a large percentage of cases, which would seem to be the case, are we doing the best for our patient in simply removing the affected glands, without getting rid of the primary deposit of tubercle; and, secondly, to what extent are we justified in giving a favourable prognosis, if, after removing the tuberculous glands, we find the tonsil to be tuberculous?

Microscopic Appearances.

In Case III (No. 6) the tonsil was exceptionally small. Large, fibrous trabeculæ can be seen dividing the tonsil into lobes. The germ-centres are few in number and small in size, and only two small crypts were present in the sections made. A single giant-cell was observed lying deeply in the tonsillar tissue, the surrounding tissue showing fibroid change.

In Case IV (No. 4) the tonsil was small and showed in a lobule, somewhat distinct from the rest of the organ, a large number of giant-cells, with surrounding fibroid change, situated deeply in the tonsil. The rest of the tonsil appeared to be healthy. In the affected portion the germ-centres were not present.

In Case I there was a pretty diffuse infection of the tonsil, separate and often single giant-cells lying between the germ-centres. Numerous fibroid areas resembling those surrounding giant-cells can be seen throughout the tonsil. In a separate lobule of the tonsil, close under the surface, extensive tuberculous infiltration was present. The germ-centres had disappeared. Nowhere was caseation to be observed.

In Case II a group of giant-cells was present deeply in the substance of the tonsil. The germ-centres and tonsil as a whole were otherwise healthy.

In Case VII (No. 46) there is a diffuse infiltration of a large part of the tonsil. The germ-centres in the part affected were almost entirely absent. There were no signs of caseation. This was a large tonsil.

In Case V (No. 5) the tuberculosis is localized, but extensive in the part affected. A large number of giant-cell systems and fibroid change are noticed around the bottom and sides of a deep crypt. The surrounding tonsil is free of germ-centres.

In Case VI (No. 34) the tonsil shows extremely diffuse infection. Although a fairly large tonsil, it shows extensive fibroid change. The giant-cells are small and not numerous. Throughout the greater part of the tonsil the germ-centres have disappeared.

On consideration of the microscopic appearances of these tonsils, it seems probable that infection has been a direct one from the mouth. There is a tendency for the tuberculosis to start in the region of the crypts, which is shown well in Case V, where numerous giant-cell systems are seen surrounding the sides and bottom of a crypt, the remainder of the tonsil being comparatively free. We are unable to support the view of Kingsford that it is a blood infection, nor that of von Recklinghausen that it is due to backward infection from the lymph-glands, as, although the deepest parts of the tonsil become affected, they are often the most accessible to a mouth infection, surrounding, as they do, the crypts which entered in many instances to the bed of the tonsil. Again, even in early cases, many of the tuberculous deposits are found to lie close under the superficial epithelial lining, which does not support Schlesinger's view that it is an external infection rather than an infection from the mouth.

For these reasons, and from the anatomical arrangement of the lymphatics, we are convinced that the tonsils present an earlier infection to that of the lymphatic glands. Are they, then, the primary source? And, if secondary, the presumption is that the primary focus will be in the lungs, as seen from the work of previous observers above referred to.

In five of our cases a careful examination of the chest revealed no evidence of tubercle, so that in them we can dismiss the probability of a sputum infection, as in the cases of Kruckmann, Ruge, Schlesinger, and others. In the two remaining cases no active evidence of tubercle in the chest was present, although some slight percussion and resonance impairment suggested the possibility of a small focus. We are therefore justified in considering these cases as probably primary infections and the mode of infection by ingestion or respiration, although the former

seems to us the more likely, as supported by experimental evidence of Dieulafoy, Orth, Baumgarten, and others, and the fact that there appears to be a tendency for that portion of the tonsil immediately surrounding crypts to become early affected, as if from a direct spread from stagnant and infected food lying in their interior.

If the tonsils are primarily affected, and have been the cause of a secondary infection of the cervical glands, one must consider whether the general opinion is the correct one—that excision of tuberculously affected glands in the neck is sufficient, with the chance of leaving the original focus unattacked; or, again, how is our prognosis in a case after excision of tuberculous glands to be influenced if we find that the corresponding tonsil is also tuberculous?

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Section for the Study of Disease in Children.

November 26, 1909.

Dr. J. Porter Parkinson in the Chair.

A Case of Strangulation of the Small Intestine by a Band in a Child aged 15 Months.

By P. Lockhart Mummery, F.R.C.S.

The child, a well-nourished and healthy boy, was admitted into the Queen's Hospital for Children on September 7 of this year. He had been quite well till 9 a.m. of the day of admission, when he was suddenly seized with pain in the abdomen and vomited. After the onset of the pain, and before admission, he passed one normal stool. There was no further vomiting, but, as the child continued to be in pain, he was brought up to the Hospital in the afternoon.

On admission the child looked ill, the face was pale, and the pulse rapid and running. The abdomen was rigid and slightly distended. In the right iliac fossa a definite tumour could be felt. This tumour was elastic and freely movable in all directions. Nothing abnormal could be felt *per rectum*.

The history showed that when the child was aged 5 months he had an ileo-cæcal intussusception, and was operated upon successfully in the Queen's Hospital by one of my colleagues. The mother stated that since the previous operation the child had been perfectly well until the present illness began. In view of the history of previous intussusception, and the presence of a tumour in the abdomen, recurrence of the intussusception was diagnosed, and it was decided to operate.

Operation (5 p.m., September 7): I opened the abdomen through an incision parallel to and just outside the scar of the previous operation. On opening the abdominal cavity a dark-red loop of strangulated small

intestine presented, and was drawn out of the abdomen. The loop consisted of about 3 in. of the middle of the ileum, which appeared at first to be twisted, but on closer examination it was seen to be strangulated by a narrow band of peritoneal adhesion. The edge of the omentum was also adherent at this spot. There were no adhesions to the abdominal wall. The bowel was of a dark-chocolate colour and much distended with gas. I divided the band with scissors, and the strangulation was at once relieved. The band was not more than ¼ in. long and very narrow. The bowel was returned into the abdomen and the latter closed in three layers. The whole operation only took about fifteen minutes.

The child slept well after the operation and caused no anxiety. There was a little anæsthetic sickness for a few hours, and the temperature went up to 99·8° F. next morning. But after that the temperature became normal, and there was no more vomiting. The bowels acted well, as the result of the administration of calomel and an enema on the following day, and the child made an uninterrupted recovery and was sent home on September 24.

I thought this case worth recording, because in any case strangulation by a band is not a very common condition and must be very rare in an infant. I have not been able to find a similar case in a child as young as this in which recovery occurred. The good result I attribute to the fact that the child was operated upon so soon after the onset of the symptoms—namely, within twelve hours. I think the band must have been inflammatory and have resulted from the previous intussusception. Two rather interesting facts also are that the child only vomited once previous to operation, whereas one would have expected that vomiting would have been a marked feature with acute strangulation of the small bowel, and that the bowels acted normally after the onset of the strangulation.

Multiple Peripheral Neuritis in a Child.

By A. E. Garrod, M.D.

I. L., aged 8. The child was healthy until March, 1909, when she passed involuntarily, during the night, a motion containing several ounces of blood. This was preceded by an attack of abdominal pain,

which was relieved by the movement of the bowels. A similar attack occurred in May. In August pain in abdomen became more or less constant, not related to food, and was accompanied by severe frontal headaches. At the beginning of September more blood *per rectum* was passed during the night. When she awoke on morning of September 12 she was unable to raise her arms, both sides being affected. On September 16 weakness in legs appeared, and she was brought to hospital. No sore throat; no rash. Youngest of four children; others healthy. On admission: Patient was a well-nourished child; mental condition good; marked loss of power in both upper limbs from deltoid downwards; double wrist drop. Muscles of lower limbs weak, but child walked steadily; no tremor; sensation unimpaired. Muscles of trunk and diaphragm unaffected. Reflexes: Knee-jerks present, but diminished; abdominal, present; plantar, flexor. Other systems showed *nil* abnormal. A throat swab showed no Klebs-Löffler bacilli. For the first week or so the muscles of upper limbs became weaker. Quadriceps extensors also became weaker and knee-jerks disappeared. Occasional frontal headache and pain in epigastrium. Since then slow but gradual improvement.

Electrical reactions: All muscles of both upper and lower extremities (except flexors and special muscles of thumb and little finger, and interossei) gave no reaction to faradaic current and reaction of degeneration to galvanic.

Blood examination: Red blood-corpuscles, 4,984,000; hæmoglobin, 90 per cent.; white blood-corpuscles, 10,500; differential count normal; red blood-corpuscles appeared normal.

Examination of urine and hair for arsenic, and of urine for lead and arsenic gave negative results.

Ophthalmoscopic examination showed no pathological changes.

DISCUSSION.

Dr. GARROD considered that the most probable diagnosis was that the child was suffering from multiple peripheral neuritis of toxic origin, and his colleague (Dr. F. E. Batten), who had seen the case, took this view of its nature. The alternative diagnosis—but, in his opinion, a far less probable one—was that of anterior poliomyelitis affecting all the limbs. The nature of the poison at work remained wholly obscure. Alcohol could be excluded, and there was no tenderness of the weakened muscles. There were none of the characteristic features of diphtheritic paralysis. The gums showed no blue line, and the increase of

hæmatoporphyrin in the urine, which was, in his experience, a constant feature of lead poisoning, was wholly wanting. The urine contained only the normal trace of that pigment. The failure to detect arsenic in the hair and urine (by Dr. Hurtley, Lecturer on Chemistry at St. Bartholomew's, who had been good enough to carry out the necessary examinations) appeared to exclude arsenic. The nature of the three attacks of abdominal pain, with passage of blood in the motions, was also obscure. There was no history of a purpuric erythema nor of pains in joints to suggest Henoch's purpura, and no polypus could be felt in the rectum.

Dr. G. A. SUTHERLAND regarded the case as one of very special interest, but he did not think members would be able to throw much additional light on the subject in view of the full statement which had been made. He thought time might help in settling the question of anterior poliomyelitis, as certain paralyses might remain while others were recovered from. Last year he had a somewhat similar case, in which a final diagnosis was never reached. The paralysis came on rather suddenly and the child was absolutely paralysed in all the extremities and partially in the trunk. The patient continued in that condition for a long time, but at length there was what appeared to be complete recovery. Some weakness, however, remained in the left leg, with wasting, and later definite spinal curvature developed. In some of those cases it was practically impossible to tell during the illness whether it was peripheral nerve trouble or a spinal lesion.

Dr. E. I. SPRIGGS referred to two cases shown at the last meeting of the Section which illustrated what Dr. Sutherland had just said. They were cases of anterior poliomyelitis affecting all four limbs. He had had the opportunity of observing their subsequent progress, and already it was easy to distinguish a difference in the rate of improvement in different muscles, whilst in Dr. Garrod's case the limbs, after the lapse of two and a half months, showed no differentiation in muscles or groups of muscles.

Dr. GARROD replied that there had been no evidence of affection of special groups of muscles, and the improvement now in progress had been uniform throughout the affected parts.

Cirrhosis of Liver.

By R. HUTCHISON, M.D.

I. M., FEMALE, aged 9; swelling of abdomen one year; no previous illness of importance. Two still-births preceded this child. Snuffled as a baby. Liver enlarged and irregular; spleen considerably enlarged; some ascites; no other evidence of congenital syphilis. Cirrhotic facies.

Achondroplasia in a Twin.

By R. HUTCHISON, M.D.

M. S., FEMALE, aged 1½, a case of well-marked achondroplasia; the other twin, a boy, is healthy (see fig.). The children were born at full time; the health of the mother during pregnancy was good. There have been two miscarriages.



Achondroplasia in a twin.

A Case of Unidactyly.

By R. HUTCHISON, M.D.

H. N., MALE, aged 13 weeks; one digit only on hands and feet, otherwise normal. First child of this generation. The father and his two brothers and one sister are similarly affected; so were his grandfather and his brother. A daughter of the father's sister also exhibits the condition.

DISCUSSION.

The CHAIRMAN (Dr. Porter Parkinson), discussing the case of cirrhosis of the liver, said that one of the most interesting points in congenital syphilis was the curious way in which it, as well as some other diseases, often selected one organ, apparently, for its operations. He remembered one child which had disseminated choroiditis, with no other evidence of syphilis, but the other children of the family were typically syphilitic. He had seen a case of paroxysmal hæmoglobinuria with the same. Other diseases, of course, such as rickets and lead, often picked out spinal tissues on which to exert their morbid influences.

Dr. SHUTTLEWORTH said he thought the cases of twins, one of whom was normal and the other abnormal, were extremely interesting and somewhat puzzling. As presenting points of analogy to Dr. Hutchison's first case, he showed two photographs of twins which Dr. Jeffreys Wood, of Melbourne, had kindly sent him, in which one twin had all the characteristics of Mongolism and the other was healthy. It was hard to say why, with the same mother though presumably different ova, one twin child should be normal and the other twin, exposed to similar intra-uterine conditions, should be affected either with achondroplasia or with Mongolism, as in the cases pictured. Dr. Eustace Smith had remarked that one often saw marked differences in twins even among such children who were normal: one was vigorous and the other weakly. The question was whether that was not the case *ab initio* from the stage of the ovum; the weakly child might always have had but small power of resistance to toxins. The effect of syphilitic poison, again, was a matter which one might cite as having certain analogies. There were cases recorded of twins one of whom had had from birth all the symptoms of inherited syphilis, whereas in the other, up to a fairly good age, there had been no sign of syphilitic infection. With syphilis, however, one did not know what the later manifestations might be for a number of years. In the case of unidactyly it seemed to him truly remarkable that there should be such defect, which one must consider was originally acquired, transmitted in so many cases amongst three generations. It would be most interesting to bring the case under the notice of Professor Karl Pearson and others engaged in statistical lines of inquiry, because they reckoned a co-efficient of the probability of any defect being handed on; and he thought the present instance of abnormal inheritance was in excess of any calculation which had been made in that direction.

Dr. CHARLES W. CHAPMAN, referring to the case of unidactyly, said he exhibited at the Clinical Society a case of clawed hands in the father and baby, the latter having the additional deformity of the absence of the radius in one arm. There is a full description with skiagrams in the Society's *Transactions* for 1900.[1]

[1] xxxiii, p. 235.

Dr. G. A. SUTHERLAND asked why Dr. Hutchison called the case of I. M. one of "cirrhosis of the liver." Probably all would agree that the masses on the liver were specific; the spleen was also enlarged. The exhibitor said there was, or had been, some ascites, but he (Dr. Sutherland) could not find any at present. Also, what was the "cirrhotic facies"? He (Dr. Sutherland) regarded it as a case of multiple gummata of the liver, a condition well known in connexion with the late manifestations of hereditary syphilis. He believed that with care the child would recover.

Dr. GARROD pointed out the importance of bearing in mind the fact that some twins were "false" and some "true"—i.e., developed from a single ovum—when discussing the occurrence of abnormalities in one of a pair. In the case referred to the twins were of different sexes, and therefore obviously belonged to the group of false twins. The abnormality of one and normality of the other member of such a pair were no more remarkable than similar differences in successive children of a family.

Dr. HUTCHISON replied that Dr. Garrod's point was a very important one. He had a photograph of twins one of which was a cretin and the other quite normal; but there again they were false twins—i.e., of different sexes. With regard to the case which he called cirrhosis of the liver, he did not think one would get so much enlargement of the spleen in simple gummata of the liver. The spleen was nearly a hand's breadth below the edge of the ribs; and when she came in there was an appreciable amount of ascites to be detected easily by the shifting dullness. He also attached considerable importance to the "cirrhotic facies," and was surprised Dr. Sutherland did not know what that was. He had always considered the facies a considerable help in diagnosis. The chief thing was the presence of spider-like telangiectases, and in grown-up persons there was a wasted and somewhat jaundiced appearance. The undidactyly cases were particularly interesting from a Mendelian point of view, and he hoped to have them worked up from that side. The case mentioned by Dr. Chapman was a similar kind of deformity, also showing the hereditary tendency, but it did not affect so many generations as in his own cases.

Congenital Enlargement of One Limb occurring in Brother and Sister.

By O. L. ADDISON, F.R.C.S.

MALE, aged 9. Enlargement of right arm, shoulder, and hand. There is 1 in. increase in length. The muscles of the arm and shoulder are well developed. The skin and subcutaneous tissues appear to be normal.

Female, aged 5. Enlargement of left leg. There is $1\frac{3}{4}$ in. increase in length. The calf is relatively much larger than the thigh, and feels hotter than the rest of the limb. The tibia is thickened.

DISCUSSION.

Dr. F. PARKES WEBER said he regarded the cases as "half-cases" (that is to say, incomplete cases) of hemihypertrophy. The characteristic of such cases was that the affected limb and its bones were always increased in length, whereas in other congenital and developmental enlargements of limbs (notably those called "trophœdema") there was never any decided increase in the length of the bones. Another feature of such cases was that they were nearly always associated with more or less angioma-formation. In the arm of the boy now shown there was a little congenital superficial telangiectasis; and he believed that some of the enlargement in the soft parts of the girl's leg was connected with enlarged veins (not the superficial veins), and possibly also lymphatics. In the *British Journal of Dermatology* for 1907 he published a paper[1] on such cases, but he would now like to change the term employed for the abnormality in question to "hypertrophy of one limb of the type of hemihypertrophy."

Mr. LOCKHART MUMMERY expressed his disagreement with Dr. Parkes Weber's contention, because in the cases which had been seen that day, and in many of the cases of gigantism of one limb, there was plenty of evidence that it was the blood-supply or the lymphatic system which was at fault. The boy showed alteration in the blood-vessels of the groin, and the girl had telangiectasis in the back and much thickening of the soft tissues. In hemihypertrophy the whole half of the body shared, including head, limbs, chest, epiphyses of bones, and tongue; and it was not conceivable that that could be due to any condition of the blood-vessels. There was no system of the body which was anatomically bilateral except the nervous system. In hemihypertrophy the only possible explanation seemed to be that the nervous system was affected; and there must be some central lesion.

Dr. PARKES WEBER rejoined that in the paper he wrote he alluded to the vascular abnormalities often found both in the cases of hemihypertrophy and in hypertrophy of one limb of the type shown that evening by Mr. Addison. § Quite possibly there might be some primary nervous disorder, as Mr. Mummery suggested; but no one knew whether there was or was not, and he had not discussed the point. He believed that in one or two cases the brain on the side opposite to that of the hemihypertrophy of the trunk was also hypertrophied. If he might use the term "hypertrophy of the

[1] F. P. Weber. "Angioma-formation in Connexion with Hypertrophy of Limbs," *Brit. Journ. Derm.*, London, 1907, xix, p. 231.

hemihypertrophy type," he would say that it might be found limited to one arm or one leg, that it might affect both the upper and lower extremities of the same side, that sometimes the whole of one-half of the body might be involved (including half the tongue), and that in certain cases the cerebral hemisphere on the opposite side (that is to say, the side corresponding to the movements of the hypertrophied portions of the body) was the larger of the two hemispheres.

Infantile Heart showing Nodules on Endocardium.

By R. C. JEWESBURY, M.B.

THE specimen was taken from a child aged 4 months, who died in the Victoria Hospital from broncho-pneumonia. During life the area of cardiac dullness was not increased, and the cardiac sounds were quite normal in character.

Post-mortem it was found that a large number of wart-like nodules were present, especially on the mitral and tricuspid valves; a few also on the aortic and pulmonary valves, and also on the septum of the foramen ovale. These nodules did not look like vegetations to the naked eye. On microscopic examination a section taken through one of the nodules shows a delicate connective-tissue substratum supporting a layer of epithelium. The vascular supply is limited; only here and there is there firm, fibrous tissue to be seen. There are no signs of recent inflammation and no collection of round cells; no signs of inflammatory products beginning to organize, nor any signs of old inflammatory products which have already been organized, nor is there any evidence of deposits of fibrin having at any time occurred during life on the surface of the nodules. There is therefore nothing to suggest that the nodules are "vegetations" (Microscopic Report from Lister Institute).

These nodules apparently occur normally, to a certain extent, in the valves in the hearts of infants, but they are exceptionally well marked in this case.

DISCUSSION.

The CHAIRMAN said that occasionally those who had seen post-mortem examinations on young infants saw light vegetations of that sort; but the present ones were larger than any he had seen, and more numerous.

Dr. F. PARKES WEBER asked if the nodules in question did not represent a stage of the so-called "hæmatomata of the valves."[1]

Dr. E. I. SPRIGGS said that when he saw the specimen shortly after the post-mortem he thought the lumps must be regarded as a hypertrophy of the little nodules in the hearts of young infants which they were warned as students not to mistake for vegetations. Dr. Jewesbury had had the nodules examined microscopically, and the report confirmed that conclusion. It was interesting that they occurred on the septum between the auricles, as well as on the mitral and tricuspid valves.

Compression of the Trachea by an Enlarged Thymus.

By GEORGE CARPENTER, M.D.

THE specimen shown was removed from a previously healthy male baby, aged 10 months, who, at five o'clock in the morning of July 16, suddenly developed rapid and laboured respiration. The respirations becoming more embarrassed, the parents became alarmed, and he was brought to the Queen's Hospital for Children at 10 p.m. the same day. When admitted into hospital the temperature was 100° F. and it subsequently rose to 103° F. The respiration-rate, which was 60 to the minute, rose to 80 to the minute. There was marked recession of the lower intercostal spaces, the larynx moved up and down with the laboured breathing, but the voice was clear. The chest was resonant, but there was greatly diminished air entry and a thin sibilant stridor was audible all over it. There were no râles. The stridor was most marked over the trachea in expiration. The tonsils were larger than natural and showed follicular exudation. Nothing else abnormal was detected. He was given a dose of ipecacuanha wine and vomited afterwards, but from this he derived no benefit, for the respiration became more embarrassed, and he died at 2 o'clock the following morning. There is one other child of the marriage, who is quite well.

At the autopsy the thymus was found to extend in a solid mass from the thyroid gland to behind the manubrium. At the manubrium it split into two lobes, the left the larger of the two, extending as far as the third costal cartilage, but the right, which was shorter, reached only

[1] *Vide* descriptions in recent years by Fahr, Hammes, Meinhardt, Wegelin, and Nichols—the last one in *Journ. of Experim. Med.*, 1908, x, p. 368. The so-called hæmatomata are supposed not to be real hæmatomata, but to be structures connected with the involution of the blood-vessels of the fœtal cardiac valves, and to disappear before the child reaches the age of two years.

as far as the corresponding second costal cartilage. Just above the manubrium the trachea was greatly compressed from before backwards. Dr. Ivy McKenzie's report on the microscopical appearances of the gland is as follows: "The lobules are of an average size and consist, for the most part, of lymphoid cells. There is a rich blood-supply; Hassall's corpuscles are present and show nothing abnormal. The connective tissue between the lobules and round the vessels is relatively small in amount. A small number of eosinophile cells are present in the neighbourhood of the small vessels." The thyroid was distinctly enlarged and red, as also the parathyroids. The lungs were crepitant everywhere and normal. The liver was very pale and friable, displaying white, fatty-looking areas on section. The kidneys also showed a similar fatty look on section. The spleen, intestines, and mesenteric glands were normal. There was no general enlargement of the lymphatic glands or lymph follicles, Peyer's patches and other lymphatic structures being healthy-looking. The lungs, which were microscopically examined by Professor Keith at the Royal College of Surgeons, showed extravasation of blood in the alveoli, indicating death from suffocation.

P.S., December 6: A subsequent examination by Professor Keith revealed a deposit of false membrane in the trachea, of diphtheritic origin. This was invisible until the trachea was opened. The compressed trachea was not, therefore, the actual cause of death by suffocation, although contributing to it in great measure. The rise of temperature is also explained by this discovery.

DISCUSSION.

Dr. J. D. ROLLESTON, who showed the case for Dr. Carpenter, said the question of thymus death had for fifty years interested the medical profession. Many doubted the existence of a thymus death at all. Recently two remarkable papers had appeared; one by von Sury, of Vienna,[1] who had had extensive experience of autopsies on children, and in 2,341 autopsies of children up to 15 years, at the Vienna Medico-Legal Institute, he could not find a case where death could be attributed to thymus enlargement. He thought that in most cases death had been due to some other cause, such as enteritis or bronchitis. In the *Archives of Pediatrics*,[2] Warthin criticised von Sury and believed many of the cases in literature justified the thymus-death supposition. In favour of the mechanical cause of death was the fact that microscopical sections of the lung in this case had been examined by Professor Keith at the Royal College of Surgeons, who said that they showed an effusion of blood into the air cells, thus giving evidence of suffocation.

[1] *Vierteljahrschr. f. gerichtl. Med.*, Berl., 1908, xxxvi, p. 88.
[2] August, 1909, p. 597.

The CHAIRMAN said that a point against the mechanical view was, how did the child live ten months with such a condition? If it was mechanical pressure, the thymus was probably bigger at one month, and the thorax was smaller, so that the pressure would be greater at the younger age. Was there any evidence of an acute enlargement just before death? There were many causes in young infants which would cause rapid death—for instance, various kinds of glottic spasm, or acute suppurative catarrh, which left practically no trace in the lungs.

Dr. THURSFIELD said he had been for some years interested in the question of thymus death, by which he meant death occurring in cases of enlarged thymus with evidence of lymphatism. In the present specimen he did not see any evidence of compression of the trachea; nor did the present thymus look large enough to cause death in that way. In the absence of evidence of lymphatism he thought the cause of death should be sought elsewhere. Were any cultures made from the heart's blood? Or did the lung, in the fresh state, show evidence of broncho-pneumonia? One case which would, he thought, carry conviction as to the possibility of death by compression of the trachea was that published by H. D. Rolleston twelve years ago, in which the thymus was of enormous weight.[1]

Dr. J. D. ROLLESTON said he could not answer the Chairman's question; Dr. Carpenter and he were discussing it, and they could not satisfy themselves about it. He certainly thought the trachea was flattened. It was another point whether the flattening was due to the thymus. Von Sury said there were some cases of flattened trachea where the flattening was not due to an enlarged thymus. No cultures of the blood were taken, but he did not think there was evidence of broncho-pneumonia in the lung in the fresh state.

Microscopical Section from the Liver of a Case of Icterus in an Infant.

By GEORGE CARPENTER, M.D.

THE section of the liver shown was taken from a female infant who was admitted into the Queen's Hospital for Children on August 15 last, at the age of 5 months. The child was apparently well the previous day, but the same night it became sick and "rolled its eyes." When admitted into hospital it showed well-marked jaundice, the skin and conjunctivæ being of a deep lemon-yellow colour. The urine stained the diapers. The fæces were dark and formed, being a mixture of dark brown and yellow ochre. The liver was not enlarged, and nothing abnormal was detected on a physical examination. The family history

[1] *Trans. Path. Soc.*, Lond., 1897, xlviii, p. 200.

was unimportant. For the first three days it had remittent pyrexia, the temperature ranging from 98·6° F. to 101·8° F., and the jaundice deepened, the body being of a bright yellow hue. On August 18 a blood-count showed red blood-corpuscles 1,652,000, with many normoblasts and poikilocytes; the white blood-corpuscles were 40,000. A differential count showed: Large lymphocytes, 7 per cent.; small lymphocytes, 38 per cent.; polymorphonuclears, 47 per cent.; eosinophiles, 7 per cent.; hæmoglobin, 90 per cent.

On August 25 she developed green and offensive stools, and was fed on whey. The stools improving under this dietary and calomel combined with pulvis ipecacuanhae compositus, she was returned to ordinary milk on August 28. On this date the red blood-corpuscles numbered 2,450,285, and a differential leucocyte count showed: Large lymphocytes, 7 per cent.; small lymphocytes, 50 per cent.; polymorphonuclears, 38 per cent.; eosinophiles, 3 per cent. The infant, however, did not thrive during the subsequent fortnight, although the jaundice decreased; she became rather wasted and died without further symptoms. At the time of death the jaundice had disappeared; the blood-count remained at just over 2,000,000 per cmm. At no time while the infant was under observation were the stools devoid of bile.

At the autopsy the liver was found to be of a uniform deep-red colour externally and on section. The gall-bladder was full of green bile; there were no enlarged glands or other obstruction to the flow of bile. The kidneys appeared to be normal to the naked eye. All the other organs were normal. The body displayed no icteric tinge.

Microscopically the liver capillaries are filled with red blood-corpuscles; there is disintegration of cells in the neighbourhood of a few clusters of leucocytes, these probably being all that is left of old hæmorrhage. The bile-ducts and Glisson's capsule are natural.

Congenital Malformation of Heart showing Complete Atresia of the Pulmonary Artery.

By A. C. D. Firth and E. I. Spriggs, M.D.

The specimen was taken from an infant aged 18 weeks. During life the cyanosis was persistent, and showed no improvement on the administration of oxygen. No increase in the area of cardiac dullness

was detected, and the cardiac sounds appeared normal except that the second sound was more audible at the right border of the sternum. No murmurs were present. The number of red blood-corpuscles per cubic millimetre was 6,800,000.

The specimen shows thickening of the muscular wall of the right ventricle. Both ventricles communicate easily by means of a patency in the upper part of the interventricular septum, and the right opens into a much-dilated arterial trunk. The pulmonary artery is represented by a thin-walled vessel which terminates blindly at the base of the arterial trunk, and which has no trace of an opening into the left ventricle. Fluids cannot be forced from the pulmonary artery into the ventricle with a syringe. The ductus arteriosus is about the size of a crow's quill and opens into the commencement of the right pulmonary artery. The right auricle is markedly dilated, the left extremely small, and the foramen ovale is patent.

The CHAIRMAN said he had treated some cases of congenital heart disease with oxygen, but, as far as he could make out, with no benefit at all.

Section for the Study of Disease in Children.

January 28, 1910.

Dr. H. D. Rolleston in the Chair.

A Case of Congenital Œdema with Dilatation of the Intestinal Lymphatics.

By David Forsyth, M.D.

The patient, a breast-fed boy of seven weeks, weighing 4·250 kilos, was admitted to the Evelina Hospital on account of his œdematous arms and legs. An only child, he was born without difficulty at full term, but from the first his left hand, left foot, penis, and scrotum were much swollen. His right leg was affected to a less degree. No family history of congenital œdema could be obtained. The swellings gradually increased, and at the time of his admission his appearance was striking.

The back of the left hand was enormously swollen, looking as if it had been blown up tightly with air. The fingers and wrist were normal, but the œdema appeared again in the forearm. The dorsum of the left foot, as far as the toes, was tense and swollen like the corresponding hand, and the outer side of the left leg showed another large œdematous mass. The right arm and foot were moderately swollen, though the fingers and toes were unaffected. The penis and scrotum were quite thrice their proper size. All these œdematous areas pitted readily. The face and trunk showed nothing amiss. The right testicle was imperfectly descended.

The patient seemed to pass an average quantity of urine, which contained no albumin. During the few days he survived his œdema appreciably decreased, but he took his food badly and was sick at times.

He lost weight, and, after a sudden rise of temperature, died somewhat unexpectedly.

Post-mortem examination: The œdematous patch on the left leg was incised, and appeared as a greyish-white, gelatinous mass. Microscopically, it was composed of connective tissue, with large spaces which had been distended with clear fluid. The abdomen presented an unusual condition. The peritoneum covering the stomach, intestines, and mesentery was opaque, slate-coloured, and very greatly thickened. It could be readily peeled off the underlying tissue. Freely scattered over it were numbers of white specks which at first sight seemed to be lymph, but on closer examination were found to lie beneath the peritoneum. In many places they were arranged in fairly regular lines running across



Congenital œdema.

the axis of the gut, and many projected above the surface of the peritoneum. The biggest were as large as a pin's head, and when they were pricked a milky fluid exuded. Under the microscope they proved to be distended lacteals.

The mesenteric glands were healthy. About 100 cc. of non-fatty turbid fluid had collected in the peritoneum. The thyroid gland was normal to the naked eye and on section. The heart, lungs, and other viscera were healthy.

Dr. FORSYTH, in reply to the Chairman, said that no cause was found for the obstruction: the thickening of the peritoneum was strictly limited to the stomach, intestines, and mesentery. The lymphatic return from the liver seemed not to be obstructed.

Hemiatrophy of Cerebral Hemisphere.

By A. C. D. Firth, B.C., and J. C. G. Ledingham, M.B.

The child, a girl aged 1 year and 2 months, was admitted for hemiplegia. The history of the child's illness, given by the mother, was as follows: The child was full term and apparently healthy at birth; the labour was not prolonged, and no instruments were used. On reaching the age of seven months the child began to have fits at irregular intervals, becoming stiff and rolling the eyes about. No sequelæ were noticed following the fits until the last occurred, six weeks before admission; this was most severe, the twitching, which commenced on the right side, becoming general and lasting for sixteen hours. After the fit the child remained unconscious and helpless for a week, and then began to recover. At this stage the mother noticed that the right side, including the face, was paralysed, and, finding after five weeks that the child made little improvement, brought her to hospital. No evidence of syphilis was obtained from the history.

On admission the child was fat and looked well, but could not walk. No facial paralysis was present, but there was evident spastic paralysis of the right arm and leg; the right arm was semi-flexed, and no effort was made to use the right hand. The triceps and knee-jerks were increased on the affected side and Babinski's sign was present; no clonus was obtained. The pupils were equal and active; no strabismus was to be seen, and the fundi appeared normal on ophthalmoscopic examination, but there was hemianopsia involving the temporal half of the left retina and the nasal half of the right retina. The other cranial nerves, as far as they could be tested, appeared normal.

Three weeks after admission the child was discharged, owing to an outbreak of measles in the ward, being readmitted two months later, suffering from meningitis, to which she succumbed.

For the notes on the post-mortem examination and microscopical appearance of the brain I am indebted to Dr. Ledingham:—

On removing the skull-cap the left hemisphere was seen to be much smaller than the right, and had a shrunken appearance, owing to the complete adherence of the pia-arachnoid to the underlying cortex.

Tape-line Measurements (hardened condition).

	Left	Right
Centre of pons to vertex (coronally)	5⅜ in.	7¼ in.
Anterior frontal pole to posterior pole of cerebellum (mesial line)	8½ in.	9 in.
Height of vertex above corpus callosum	1 in.	1¾ in.

On palpation the superficial portion of the left hemisphere was hard and beaded, with intervening softer depressions. This condition was most marked in the parietal and occipital lobes. The temporo-sphenoidal lobe did not appear to be affected. On section of the brain in the left upper parietal region the superficial grey matter was found to be in great part destroyed, its place being taken by a loose cribriform tissue extending from the thickened pia downwards between adjacent convolutions; this material had a peculiar reddish, gelatinous-like appearance. The right cerebral hemisphere and spinal cord appeared normal to the naked eye on section. No evidence of fracture of the skull or old meningeal hæmorrhage was present, and every cranial sinus was quite healthy. The viscera of the thorax and abdomen were normal, the left carotid being dissected out and examined.

Histological Report on Section of Leptomeninges and Underlying Brain Matter from Upper Parietal Region of Atrophic Side.

The surface of the pia mater shows marked irregularity of contour compared with the normal side. It has a crumpled appearance, and with its contained vessels has more than twice the thickness of the pia on the right side. The pial vessels show some thickening of their muscular walls, and are all quite stuffed with blood-corpuscles. Between the convolutions the leptomeninges penetrate deeply, and they show a considerable development of new blood-vessels, all of which are quite filled with blood-cells; there is no evidence of inflammatory change in the leptomeninges. Adhering to the pia there is still a thin band of atrophic cortex which has escaped complete absorption; no nerve-cells are present in it—only glia-cells. Below this thin band is an irregularly-defined area of softening; only a few nerve-cells remain in this part. The softened area is pervaded by an enormous number of venous sinuses surrounded by thick fibro-cellular adventitial sheaths; between these sinuses is a mesh-work containing large numbers of swollen and granular glia-cells; the nuclei of these cells retain the stain well, and, as a rule, are placed eccentrically. There has been no hæmorrhage into the softened area, the reddish appearance being obviously due to the large number of congested blood-vessels ramifying in this area.

Sections of parietal cortex of the sound side, both optic nerves, medulla, dorsal and lumbar cord showed no obvious change, and the vessels at the base of the affected side appeared healthy on microscopical examination.

Prolapse of Intestine through Ectopia Vesicæ.

By A. C. D. FIRTH, B.C.

THE child was admitted, when a few hours old, for ectopia vesicæ and imperforate anus. Shortly after admission meconium was passed via the bladder, and three days later a portion of intestine, 1½ in. in length, prolapsed the same way. Flatus and fæces were passed through the prolapsed gut. The child died on the sixth day after admission. The specimen, consisting of bladder and intestines, was removed *en masse*. Coming through the posterior wall of the bladder is the portion of prolapsed intestine. On the posterior aspect of the bladder the small intestine alone enters the fistula. Immediately to the left of the opening into the bladder are the cæcum and appendix; the rest of the large intestine lies below and to the left, somewhat curled up upon itself and ending blindly above the pelvic floor. No mesentery is present. In addition to these defects, post-mortem examination showed congenital cystic disease of both kidneys and marked hypertrophy of the pylorus.

DISCUSSION.

Mr. A. R. THOMPSON said that four years ago there was a case at Guy's Hospital which, as resident surgical officer, he was called to see. The intestine ended at the umbilicus, and about 1½ in. had prolapsed through the umbilicus, and into the prolapse all the small intestine had passed; so that there was also a strangulated hernia.

Dr. FIRTH, replying to Dr. Box, said the fistula seemed to be in the middle of the posterior wall of the bladder.

Case of Enlarged Spleen.

By VINCENT DICKINSON, M.D.

C. S. M., FEMALE, aged 2 years, born of Italian parents in England. Father had suffered from some venereal disease when aged 23, after which he lost his teeth, and his gums were often bleeding. Mother healthy, no miscarriages. Four children: (1), a boy aged 7 years, healthy; (2), a boy, aged 3 years, weakly and stated to have a big head, swollen wrists and curved legs: ? rickets. Patient was breast-fed for seven months, afterwards on milk, cocoa, and bread. Always had a

large abdomen. When four months old began to refuse food. She had previously been admitted into the Italian Hospital with diarrhœa, enlargement of the abdomen of indefinite nature, wasting, no ascites. Calmette's reaction negative. She improved under general treatment and went out. Readmitted December 15, 1909. Emaciated; anæmia; rickety thorax; distended abdomen; spleen much enlarged, sharp border, with notch, reaching almost to middle line, and downwards nearly to iliac crest; liver felt below costal border; no marked glandular enlargement. Blood-film showed red cells rather pale and discoloured, a very few poikilocytes, no normoblasts; leucocytes normal in amount, and normal proportion of different varieties. Ten days after admission diarrhœa supervened, and on January 6 she had an attack of tetany involving the hands and feet, with retraction of the head. The child has now greatly improved, and weighs 14½ lb. The spleen is much reduced in size; liver still palpable; Calmette's reaction again negative. The last blood-films, made by my house physician (Dr. Mayer), showed no abnormal leucocytes, no myelocytes or normoblasts, no mast-cells, slight poikilocytosis, the number of red blood-corpuscles rather above the normal.

Dr. FORSYTH said he recently had a patient in the Evelina Hospital whose history and condition largely tallied with the present one. On admission her spleen was much enlarged, reaching well into the left iliac fossa. Her liver was moderately enlarged, and she had a purpuric rash over her chest and back. Calmette reaction was negative. During the three months she was in hospital her spleen became gradually smaller, until it reached only a couple of inches below the ribs, but the liver remained unaltered. She died, and post mortem the spleen was hard from an increase of fibrous tissue; but, in addition, a mass of caseous tuberculous mesenteric glands was found, which had not been felt during life. Could so enlarged a spleen be the result of tuberculosis, or did the child have splenic anæmia plus tuberculosis? During life, he thought the diagnosis of tubercle was excluded on account of the very big spleen.

Separation and Displacement Forwards of the Lower Epiphysis of the Femur treated by Plate and Screws.

By DOUGLAS DREW, F.R.C.S.

MALE, aged 8½, was knocked down and run over by a van on November 30, 1909. He was admitted to the Queen's Hospital for Children shortly after the accident. The region of the knee was much

swollen, and above the swelling there was a depression on the front of the thigh as though the knee were displaced forwards. The radiograph (taken a few days later, after failure to reduce the deformity under anæsthesia) shows the lower epiphysis of the femur displaced forwards on to the front of the shaft of the bone and rotated through nearly 90°, so that the articular surface looks directly forwards. It is to be noted that the epiphysial line is oblique, downwards and backwards.

December 9: The fracture was treated by operation, the epiphysis being fixed in position by means of Lane's plate and screws. The limb was placed in a splint bent to a right angle. Massage and passive movement were begun within three weeks, and active movement a week later. January 19 (six weeks): Child can now walk with assistance. Movements at knee are good.

DISCUSSION.

Mr. DREW attributed the displacement and the difficulty in retaining the fragments in position after reduction to the slope of the epiphysial line—i.e., downwards and backwards. At the operation, in whatever position the limb was placed, the epiphysis tended to slide forwards, but particularly when the limb was extended, doubtless owing to the tension on the muscles in this position and the sloping surface of the epiphysis. Mr. Drew said he was strongly opposed to operating upon simple fractures as a routine measure, and had always considered that this most useful method should be reserved for those cases in which it was found impossible to reduce the deformity by manipulation and retain it in position, as in the case under consideration.

Mr. A. R. THOMPSON desired to offer a suggestion as to the displacement of the epiphysis, and to ask Mr. Drew if he agreed with it. At the College of Surgeons there was a good specimen showing extreme forward displacement of the lower epiphysis of the femur. He had made many dissections in the fœtus to elucidate the point, and, though he would not say that the gastrocnemius was attached to the diaphysis, he said it was attached to the periosteum which went to the diaphysis, and that therefore the same muscle was pulling the upper fragment back in cases of separated epiphysis which pulled the lower fragment back in cases of fracture of the lower end of the shaft of the femur. The present must be an isolated case. Professor Arthur Thomson showed, in the *Journal of Anatomy*,[1] that the epiphysial line was practically transverse; there was a peculiar arrangement of humps upon the epiphysis which prevented it being displaced; and, though on a horizontal plane in a transverse direction, if one cut sections of the epiphysial line of the knee-joint of a child, it would be found that the epiphysial line sloped downwards and forwards in front, and

[1] *Journ. Anat. and Phys.*, Lond, 1902, xxxvi, pp. 95-105.

downwards and backwards behind. That greatly increased the retentive force which held the epiphysis and diaphysis. He thought the gastrocnemius was chiefly responsible for pulling the upper fragment backwards, and that the displacement forward of the lower fragment was more or less passive, but depending on it.

Mr. LOCKHART MUMMERY thought Mr. Drew was to be congratulated on the case; as much on the after-treatment as on the operation. It would be interesting to hear what the after-treatment was, and how soon after the operation he allowed the boy to walk about.

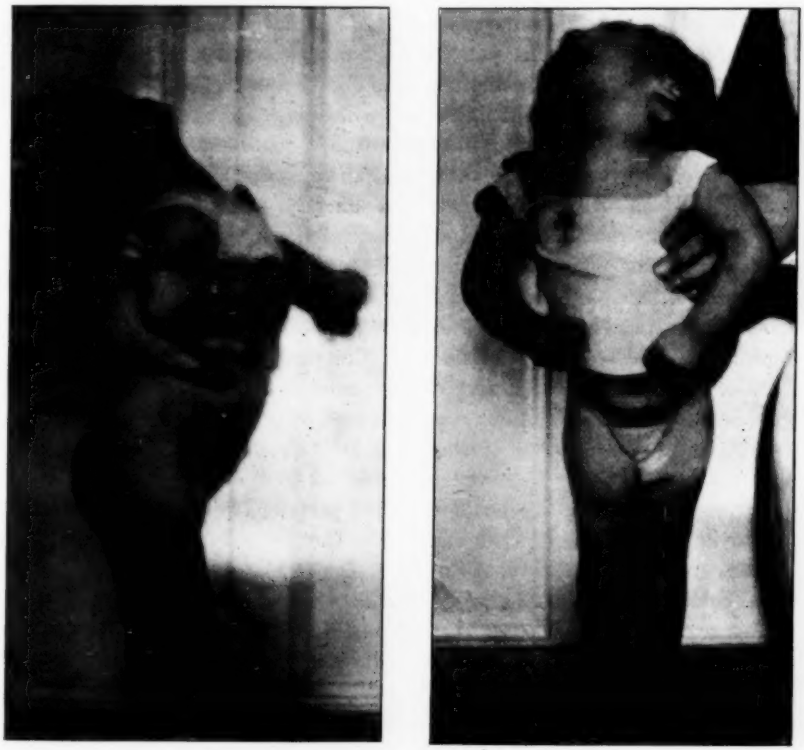
Mr. DREW replied that he was unable to verify that the gastrocnemius was attached to the diaphysis, but it was not attached to the epiphysis. The periosteum was stripped very much from the shaft of the bone, and the heads of the gastrocnemius were attached to the periosteum. On examining the slope of the epiphysial line, it was found to be quite smooth, sloping downwards and backwards. The after-treatment was that the child was kept on splints for about three weeks, and then passive movements of the knee were begun, with very light massage. A week later, active movements of the joint were permitted.

Family Amaurotic Idiocy without Characteristic Ophthalmoscopic Signs.

By F. PARKES WEBER, M.D.

THE child, M. A., female, aged 14 months, is not wasted, but is unable to sit up, and, owing to weakness in the muscles of the back and neck, its head (which seems rather large and heavy, and measures 18½ in. in circumference) falls backwards if not supported ("head-lolling"). There is occasionally likewise active retraction of the head, which must not be confused with the merely passive head drop. It cannot move about; it cannot even turn or roll itself over in the bed from one side to the other. It can, of course, only be fed with fluids, and when it sucks from a feeding-bottle the movements of its jaws appear automatic, rhythmic, and exaggerated; they suggest the automatic movements of a frog deprived of its cerebrum. Similar rhythmic movements of the jaws and mouth are sometimes present when the child is not feeding. It occasionally "swallows the wrong way." It generally lies apathetic or somnolent, taking no notice of anything unless disturbed by medical examination or otherwise, but will cry out when the feeding-bottle is removed. It never attempts to grasp or even

to touch anything, not even the feeding-bottle. There is a variable amount of rigidity, due to tonic muscular spasm, affecting the trunk and both upper and lower extremities. The lower extremities are often rigidly extended, with the feet in a tetany-like position; the upper extremities are flexed at the elbows, and the hands are often clenched,



Photographs showing the head-drop and the position of the extremities.

with the thumbs between the middle and index fingers. The knee-jerks can be obtained, and are at present rather exaggerated. No ankle clonus. The plantar reflexes tend, on the whole, to be of the extensor type (Babinski sign). The tendon reflexes of the upper extremities are present. There is vertical nystagmus, which varies much in degree from time to time: sometimes it is very well marked, rendering

ophthalmoscopic examination difficult; sometimes it is temporarily absent.

Ophthalmoscopic examination (Dr. R. Gruber and Dr. C. Markus) shows nothing characteristic of family amaurotic idiocy. There is no pallor of the macular region with a cherry-red spot (corresponding to the fovea centralis) in the centre, such as was described first by Waren Tay in 1881, and by B. Sachs in 1887, and has since been found to be present in nearly all (some say all the certain) cases of the disease.[1] Both optic disks are rather pale, but not distinctly atrophic, and there is no evidence of past optic neuritis. The child is practically blind, though it can still perceive a bright light, and will occasionally follow an electric lamp with its eyes. The pupils react promptly to light.

There seems to be nothing abnormal in regard to cutaneous sensation, hearing, or taste. The child notices at once if its milk is unsweetened. The only teeth visible are the lower central incisors; there are no signs of rickets at the ribs or epiphyses. There is no evidence of disease in the abdominal or thoracic viscera, excepting some bronchitis, possibly connected with "swallowing the wrong way." Lately there has been variable fever. The urine is free from albumin and sugar.

The disease has been a very chronic one. According to the mother, the child was normal at birth and was suckled for the first five months. Weakness in the muscles of the back and neck, so that it could not hold up its head, was observed at three months of age. The mother does not know whether it could ever see properly. There is no history of convulsions or any illnesses other than the present one.

The parents, both of them said to be healthy, are Jews, but there is no blood-relationship (consanguinity) between them. They have had six children, of which the present patient is the youngest. The mother says she has had no miscarriages. The eldest child is a girl, now aged 11 years, living and healthy. The second, a boy, is likewise living and healthy, aged 9½ years. The third, a girl, had the same disease, so at least the mother thinks, as the present patient, and died at one year of age in the mother's home; there was no necropsy. The fourth child was a boy (Ben A.), who likewise, according to the mother, suffered from the same disease; at the age of 2 years he was admitted with convulsions to

[1] E. C. Kingdon and Risien Russell, in their classical paper (*Med.-Chir. Trans.*, Lond., 1897, lxxx, p. 106), say: "The ocular symptoms, which are an early and, we believe, absolutely diagnostic sign of the disease, consist in symmetrical changes at the macula lutea, in which situation, covering a space nearly twice the size of the optic disk, is a whitish-grey patch. . . . In the centre of the patch is seen the fovea centralis as a dark cherry-red spot."

the London Hospital on June 2, 1906, and died on the following day. Dr. H. L. Tidy, the Medical Registrar of the London Hospital, has kindly furnished the following notes of the case: The head was 20¾ in. in circumference, and the forehead large and square. There were no marked epiphysial signs of rickets. The boy had been healthy at birth and had been breast-fed for the first twelve months. He had never been able to take solid food. The head had been inclined backwards since three months of age. He had had convulsions every few weeks since six months old. He had never been able to walk or talk. On admission to the London Hospital the boy was quite stiff, the arms in a tetany-like position, and the head thrown back. There was a patch of pleural friction on the right side; afterwards the rigidity passed off and the child slept. Temperature, 103·6° F. At the necropsy the lungs showed areas of collapse, and the larynx contained vomit. A large pad of adenoids was found in the nasopharynx. There was internal hydrocephalus. The skull-cap was thin and the fontanelle was widely open. The brain weighed 54 oz., and the brain-substance was soft.

The fifth child, a male, appeared healthy at first, but died of bronchitis at the age of eleven months, in the mother's home. There was no necropsy.

Dr. Weber thinks that the present case is almost certainly one of family amaurotic idiocy, possibly associated with internal hydrocephalus. The interesting point in the present case is that, as the typical ophthalmoscopic appearances of the disease are absent, and as there is no atrophy of the optic disks, it is unlikely that the amaurosis is connected either with local changes in the retina or with changes secondary to hydrocephalus (even if there be any hydrocephalus). The amaurosis is, therefore (in the present case), probably dependent on the diffuse changes (atrophy of ganglion cells and fibres) such as have been demonstrated in the brains of cases of family amaurotic idiocy in which during life the characteristic ophthalmoscopic changes were present. There is no a priori reason why the ganglion cells of the retina, the atrophy of which is supposed to give rise to the ophthalmoscopic changes in question, should be affected in every case of the disease. Indeed there are several records[1] suggesting that the retinal changes, although they led to the discovery of the disease, are not an altogether essential

[1] See the cases quoted from the literature of the subject by Wilbrand and Saenger, *Die Neurologie des Auges*, Wiesbaden, iv, part 1 (1909), pp. 396—447. Moreover, in the so-called "juvenile type" of the disease the characteristic ophthalmoscopic changes are not found.

part of the pathological process. Unfortunately necropsy records of such clinically "incomplete" cases are wanting, so that it has not yet been proved that the central nervous system in the "incomplete" cases shows changes exactly similar to those which have been proved to be present in "clinically" complete cases. Many of the clinical signs (spasticity of limbs, peculiarity in sucking, and difficulty in swallowing) in the present case, as in typical cases, are doubtless really due to a kind of chronic cerebral diplegia produced by the morbid process; and other signs can be explained by the cerebellum being affected also in the same way as the cerebrum.

Internal Hydrocephalus and Amaurosis without Definite Ophthalmoscopic Changes, following Symptoms of Posterior Basic Meningitis or Ependymitis.

By F. Parkes Weber, M.D.

The patient, H. H., a boy, was aged 4½ months when admitted to the German Hospital on October 11, 1909. He had seemed healthy until eight days before admission, when he commenced to suffer from slight convulsions, spasmodic movements of eyes, and diarrhœa. There was nothing of special interest in the family history, except that a brother of the patient, aged 11 years, suffered from congenital hydrocephalus. Since admission to the hospital the child has seemed almost blind, and has become increasingly apathetic, although at first there was considerable irritability. There have been no convulsions, excepting a slight one when the child was being bathed directly after admission. Decided retraction of the head was at first very noticeable, but is less marked at present. The head has gradually increased in size. Its circumference on November 8, 1909, was 18⅛ in., and on January 24, 1910, was 20⅝ in. At first there was slight fever, but, excepting during three days in January, there has been none since October 21. At present the child is practically blind, though it can perceive a bright light. Ophthalmoscopic examination shows nothing abnormal beyond slight pallor of the optic disks. The child lies quite apathetic in its bed, not moving from the position in which it is placed. It never attempts to catch hold of anything, and does not even put its hands towards the feeding-bottle. It can distinguish between sweetened and unsweetened

milk, and can evidently hear a loud, sudden sound. There is often considerable spasticity of the lower extremities, on which occasions (though not always on other occasions) the plantar reflexes are of the extensor type (Babinski's sign).

Splenomegaly and Hydrocephalus.

By F. Parkes Weber, M.D.

The patient, W. J., a boy, was aged 13 months when admitted to the German Hospital in August, 1909. He was very pale, with a large head, measuring $20\frac{7}{8}$ in. in circumference, and an open anterior fontanelle of about the size of a shilling. There was doubtless some internal hydrocephalus. The spleen was enlarged, and reached nearly to the ilium. The liver extended three finger-breadths below the costal margin. The urine was free from albumin and sugar. Examination of the blood showed: Hæmoglobin, 29 per cent. (Sahli's method); red cells, 2,568,000 in the cubic millimetre of blood; white cells, 6,300 (neutrophile polymorphonuclears 32 per cent., small lymphocytes 33 per cent., large lymphocytes 24 per cent., eosinophiles 1 per cent., basophiles 6 per cent.); no nucleated red cells were seen. Although there was no history of congenital syphilis, the treatment adopted has been chiefly anti-syphilitic. The child's condition has slowly but decidedly improved. Examination of the blood on January 22, 1910, showed: Hæmoglobin, 40 per cent. (Sahli's method); red cells, 4,480,000 in the cubic millimetre of blood; white cells, 13,200 (neutrophile polymorphonuclears 43·2 per cent., small lymphocytes 44·6 per cent., large lymphocytes 4·8 per cent., transitionals 3·4 per cent., eosinophiles 1·2 per cent., basophiles 2·8 per cent.); during the count of 500 white cells, 11 nucleated red cells were observed. The circumference of the head is now $21\frac{1}{4}$ in., and the fontanelle has almost closed. The spleen hardly reaches below the umbilical level, and the liver cannot be felt below the costal margin in the right nipple line. Beyond moderate beading of the ribs, the child has not shown marked signs of rickets. There was slight pyrexia on admission, but otherwise the child has generally been free from fever. Wassermann's sero-reaction for syphilis, kindly tried by Dr. Henderson Smith on November 28, 1909, gave a negative result.

X-ray Photographs from a Boy, aged 3 years, the subject of Habitual Constipation.

By E. I. Spriggs, M.D., and H. F. L. Hugo, M.B.

The boy has been constipated since birth, the bowels being only opened with injections, except for a few periods of diarrhœa, which have occurred about every six months. He has vomited occasionally, especially after purgatives have been given in vain. His appetite has been poor. A sister, aged 7 months, is also constipated, and a brother died at the age of 10 months of diarrhœa after some trouble with the

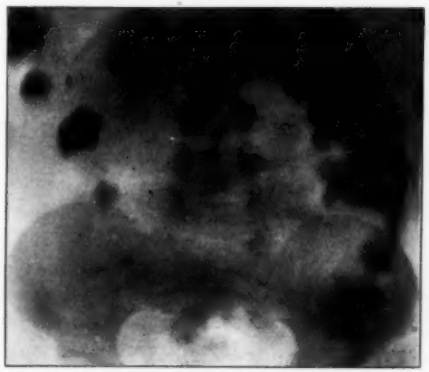


L Fig. 1. *R*

After twelve hours.

bowels. Neither parent is subject to constipation. In the hospital syrup of senna, cascara sagrada, magnesium sulphate, and liquorice powder were ineffectual: an enema usually produced a result. Nothing abnormal was found on examination of the abdomen and the rectum. The photographs (by Dr. Hugo) show the bismuth (given with bread and milk) in the small intestines and cæcum in six hours; in the ascending colon and the whole of the transverse colon in twelve hours (fig. 1); the descending colon in eighteen hours (fig. 2); the hepatic flexure, transverse colon and sigmoid in thirty hours; the descending colon and sigmoid in forty-two hours (fig. 3); the sigmoid entirely in fifty-four hours. The bowels were not opened during the observations. Comparing these times with those found by Dr. Hertz, it appears that

the delay in the passage of pure residue took place almost entirely in the descending colon and the sigmoid flexure, probably chiefly the latter. Most of the colon was traversed in a normal time. After the photographs were taken he was given a diet of wholemeal bread, porridge, treacle, fruit, butter, dripping, greens, potatoes, meat and fish, with



L FIG. 2. *R*

After eighteen hours.



L FIG. 3. *R*

After forty-two hours.

plenty of fluid. For the first three days of this diet enemas were used, on the fourth the bowels were open naturally and on the three succeeding days of his stay in hospital, which was brought to an end by the occurrence of infectious disease.

It is generally held that a "stimulating" and bulky diet should be prescribed when constipation is due to delay in the passage of food residues through the cæcum and the ascending and transverse colon; but that when it is due to delay in the sigmoid flexure owing to the sigmoid or rectal reflex not having been acquired, or having been lost, that dietetic treatment is not indicated.

The result of treatment in this case shows that a suitable diet is also of advantage in those cases of constipation in which food passes through the small intestine and the major part of the large intestine at a proper rate.

DISCUSSION.

Dr. ERIC PRITCHARD said he understood the child had never had a natural action, and that its bowels were only relieved after an enema had been injected. If that were the case, the result coincided with his usual experience of cases in which enemata were used during the early days of infancy. The infant readily established a reflex act of this kind in response to definite stimuli, and the reflex would not occur in response to other forms of stimuli. Enemata and glycerine suppositories should therefore be avoided in such cases, and the treatment should consist in attempting to induce a natural reflex by means of deep massage applied to the iliac fossa at the same time every day. Touching the anus with a piece of soap or with the finger sometimes assisted in establishing the reflex.

Mr. MILNER BURGESS asked whether such cases might not be due to absence of muscular tissue. He assisted at an operation on a lady, and it was found that she had no muscular tissue in her colon.

Mr. LOCKHART MUMMERY agreed that massage was very valuable in the cases such as Dr. Spriggs had detailed, but there was another method which he had used at St. Mark's Hospital in connexion with constipation—namely, to give something by the mouth which would prevent the fæces from getting solid; loose stools could then be insured without the giving of aperients. The best way of doing this was to give a quantity of fat in the diet; 3 oz. or 4 oz. of cream would suffice to cause an easy motion; or petroleum could be used in place of the fat. A small quantity only of the fat would be absorbed, and the remainder would pass along the colon. He had adopted that plan in cases where he had resected the colon, as the prevention of formed fæces was here of great importance.

Abnormal Congenital Pigmentation of one Eye.

By N. Bishop Harman, F.R.C.S.

The case is that of a little girl aged $2\frac{3}{4}$ years. The mother noticed that the right eye was blacker than the left when the child reached the age of 6 months; by the time she was 18 months old it was noticed that the white of the eye was different also, and for this she was brought



Congenital pigmentation of right eye. (N.B.H., *del. ad nat.*)

to me. Right eye: Iris almost black; at the distance of a yard the difference between iris and pupil can scarcely be detected; the sclera is covered with patches of brownish-black pigment—there are particularly deep spots at the site of the perforations of the anterior ciliary vessels; fundus is a dark slatey-brown tint. Left eye: White sclera and dark-brown iris. Parents are English and normal eyed. This excessive pigmentation is very rare amongst pure whites, and uncommon in this degree in blacks, especially in an asymmetrical form. It is occasionally seen in Dalmatian (plum-pudding) dogs.

Two Cases of Familial Discoid or "Coppock" Cataract.

By N. Bishop Harman, F.R.C.S.

The form of cataract known as discoid, or "Coppock," after the name of the family in which it was first seen, is a recent discovery, and an ocular defect of considerable rarity. Of the two cases shown, the younger girl came to see me on account of a small occasional divergent squint. Recognising the presence in her lenses of this rare form of cataract, I examined at subsequent times the rest of her family, with results that appear below.

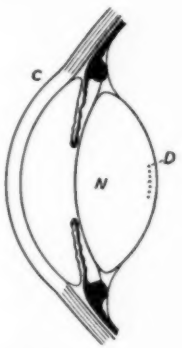
The cataract is one of singular delicacy. It consists of a small plaque of density, of perfectly circular outline, of uniform density, which are perfectly symmetrical in the two eyes. It measures from 2·5 mm. to 4 mm. in diameter. By taking the parallax it is determined that the opacity is in the posterior region of the lens, between the nucleus and the posterior pole. No microscopical sections have yet been made of any case, but it seems pretty evident that the opacity is caused by some deposit between the lens fibres. The size and density of the cataract have varied slightly in the different cases that have been described, but in the two shown to you the only difference between the condition of the two sisters is a very slight variation in the size of the opacity. The density is so slight that the details of the fundus can be seen through it by the direct method, and unless the media of the eye be examined by the reflection of light thrown into the eye by a plane mirror, the opacity is likely to be missed.

In some of the cases described by Nettleship the density has been sufficient to cause diminution of visual acuity, and the same occurred in cases described by Chance; but in these children there is little or no measurable diminution of visual acuity consequent on the presence of the opacity, as will be seen below.

The cataract is believed to be congenital—that is to say, it has been seen to be present at the earliest ages of life. It does not progress, for it is similar in character in both old and young subjects; but quite recently Doyne, who saw the first of these cases twenty years ago, said he was inclined to think that in some of these the opacity was slightly denser after the lapse of those years. The teeth in these two children are good, so far as the permanent dentition teeth are cut, and this is what has been found in other cases.

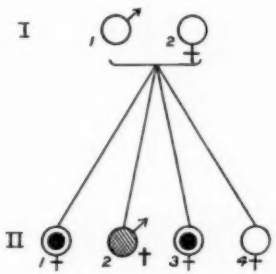


Discoid cataract seen against fundus reflex. (N.B.H., *del. ad nat.*) (x 5·25.)



C = Cornea.
N = Nucleus of Lens.
D = Discoid Cataract.

Section of front of an eye to show position of discoid cataract.



= Discoid Cataract.
= Normal Lenses.
= Died Young.

Pedigree of family.

Family history: The family is a small one; all its members have been examined. Generation I: (1) The father is a healthy man, aged 42; his lenses are perfect and free from any opacity; vision $\frac{6}{6}$, hypermetropia 0·5 D. (2) The mother is also healthy, aged 35; lenses perfect, vision $\frac{6}{6}$, hypermetropia 0·75 D. The parents bear no blood relationship to each other; they came from different counties, and they do not appear to be related to any of the families exhibiting these lens defects which will be noted later. Generation II: (1) Girl, aged 9; right eye, hypermetropia 2 D.; left, 2·25 D.; vision, with glasses, $\frac{6}{6}$. Each lens presents the characteristic discoid opacity in the posterior layers. The opacity measures 3 mm. in diameter; fundus normal. (2) Boy, died aged 4. (3) Girl, aged 7; occasional divergent squint of the left eye; refraction by retinoscopy, right, + 1·75 D., sphere, $\bar{c}$ — 2·75 D. cylinder, axis horizontal; left, + 1·5 D., sphere, $\bar{c}$ — 2·5 D. cylinder; axis horizontal. Vision, with glasses, $\frac{6}{9}$ partly. The failure to get $\frac{6}{6}$ is sufficiently accounted for by her age and by the astigmatism. Each lens has the characteristic discoid opacity of about 2·5 mm. diameter; it is of extreme delicacy. (4) Girl, aged 2; lenses perfect; no trace of any opacity.

As I noted at the outset, this form of cataract is quite a recent discovery: the first case was seen by Doyne at Oxford in 1888, in a family of the name of Coppock. Since that date an enormous pedigree has been worked out for that family by Nettleship and Ogilvy [3], showing a remarkable hereditary transmission of the defect in the lens; large numbers of the family in several generations have been proved to bear this curious family mark.

Another equally remarkable pedigree of a similar lens defect has been worked out by Nettleship [2], and in this particular family a coincident liability to lamellar cataract was found; indeed, Nettleship was able to collect a graduated series of lens defects, from the small discoid to the full-sized lamellar cataract. Another pedigree of pure discoid cataract has been recently published by Priestley Smith, of Birmingham. This form of cataract has been recognized, and a short pedigree published, showing hereditary transmission, by Burton Chance [1], in America. A single case of the kind was published by Parsons in 1905.

There is one peculiar feature in the cases of familial discoid cataract shown to you: the parents are free from any signs of lens defect. It follows, therefore, that the defect was either latent in the germ-plasm of one or other of the parents, or also that the combination of the

germ-plasm of the parents was defective in its resultant. So far I have been unable to examine any of the parents' relatives—they live far away—but it may be possible later. In any case, these cases will be of interest: either they are initial cases of the defect, or, should defects of similar character be found in collateral ancestors, we should have a discontinuous inheritance, a feature that is directly contrary to the rule of hereditary cataract.

REFERENCES.

[1] Chance, Burton. *Archives of Ophthal.*, New York, 1907, xxxvi, p. 505.
[2] Nettleship. *Trans. Ophthal. Soc.*, Lond, 1908, xxviii, p. 226.
[3] Nettleship and Ogilvy. *Ibid.*, 1906, xxvi, p. 191.
[4] Parsons, J. H. *Ibid.*, 1905, xxv, p. 89.

DISCUSSION.

Mr. Sydney Stephenson suggested that the name "Coppock" be changed to "Doyne," as Mr. R. W. Doyne was the first person to describe that form. There was some objection to having diseases named after men, but if it was to be done, the correct one should at least be applied.

Mr. Harman agreed with Mr. Stephenson's suggestion, and said that the title of the paper recently read before the Ophthalmological Society by Mr. Priestley Smith, of Birmingham, had been amended in this fashion.

Spastic Diplegia with Mental Defect.

By O. K. Williamson, M.D.

M. C., aged 10½ years, came to the Shadwell Children's Hospital on December 10, 1909, on account of "inability to speak much or to do things for herself."

History: Forceps were used at the child's birth. She did not walk until three years old, and from this time the mother noticed that she used frequently to knock her knees together, also to fall, and that she walked stiffly. She did not speak until about eight years of age. She usually lies with her legs drawn up.

Family history: Mother has had nine other children and one miscarriage. One child died of morbus cordis. This child likewise had instruments used at birth, and the mother states that it was unable to learn. None of the other children were paralysed.

State: Girl gives the impression of being mentally defective. She can only say a few words, such as "Mammy" and "Daddy." If she sees a picture of a dog, she says "Dog," and understands what is said to her. It is said that she is mischievous and hides things, and that her memory is very deficient. She is excitable; is not dirty in her habits. Circumference of head 19¾ in.; head is symmetrical, forehead low; palate high-arched, teeth normal. Is able to walk, but there is distinct rigidity of lower extremities and also some loss of power. Feet, not extended at ankle-joints, are normal. Distinct loss of power in both arms and also hands. Sensation appears to be normal. Both knee-jerks greatly exaggerated; ankle clonus not definite. Extensor response to plantar stimulation on left side, not definitely so on right side. Wrist-jerks exaggerated. On attempting to grasp an object, coarse movements of the arm take place, and the mother has often noticed shaking movements of the limbs. Occasionally a rather fine tremor of the right arm is seen. The heart is normal, and no physical defects other than those mentioned are noticeable. The hands feel cold; she suffers from constipation. The child's condition has remained unchanged up till the present time.

Cystic Swelling at the Root of the Nose.

By P. Maynard Heath, M.S.

J. H., boy, aged 5 years. A swelling at the root of the nose has been noticed for two months. It came on after an attack of chicken-pox. The superficial part is cystic, and it appears to protrude between the nasal bones. It extends upwards between the eyebrows, and ends about the middle of the forehead. Below is a deep groove in which the bridge of the boy's spectacles rests; below this groove the cyst extends on to the nose. The bridge of the nose is widened and there is some irregularity of the nasal bones. Near the tip of the nose, in the mid-line of the dorsum, is a minute opening from which sebaceous matter can be squeezed. The opening is the result of an operation two years ago for the removal of a cyst from the top of the nose. It appears to have no connexion with the swelling at the root of the nose. The swelling was explored on February 2. A cyst the size of a pea was removed from the subcutaneous tissues, but the main swelling was found to be due to an abscess under the periosteum at the root of the nose. From it a track led down into the nose at the junction of the nasal and frontal bones.

Strangulated Ovarian Tumour in a Child: Operation: Recovery.

By J. M. G. Swainson, F.R.C.S.

Child, aged 2 years, brought to hospital with acute abdominal symptoms; pain and vomiting of four days' duration. Abdomen rigid and somewhat distended, tumour felt in right iliac fossa. Temperature 100·4° F. Median laparotomy performed; commencing peritonitis observed, and some dark, sanious fluid present. A strangulated ovarian tumour, of about the size of a tangerine orange and greyish-black in colour, was removed. The other ovary was normal. Some saline solution was introduced into the peritoneal cavity, and the wound closed. Uninterrupted recovery.

A definite diagnosis was not made. It was thought that early peritonitis was present. Intussusception, appendicitis and tuberculous peritonitis were thought of, and on palpation under the anæsthetic a mobile rounded tumour was so distinctly felt in the right iliac fossa that the idea of an ovarian tumour was entertained, but not seriously, owing to its improbability.

Pseudo-hypertrophic Muscular Paralysis.

By A. Manuel, M.B.

S. S., male, aged 8½ years, was admitted to the Victoria Hospital on January 14, suffering from general weakness and difficulty in walking. The condition was first observed about eighteen months ago, but has very definitely increased in severity during the past six months. On examination the case proved to be a typical one of pseudo-hypertrophic muscular paralysis, the lordosis, waddling gait, and method of rising from the ground being quite characteristic. The muscles of the calves and the infra-spinati are markedly enlarged, but the glutei do not appear to be increased in bulk. The supra-spinati and sternal portions of the pectorales are atrophied, the latter almost completely so. The interesting feature of the case is that no history can be elicited of any past or present member of the patient's family having suffered from the disease.

A Case of (?) Congenital Aortic Stenosis.

By A. MANUEL, M.B.

T. R., MALE, aged 5 years, was brought up to the Out-patient Department of the Hampstead General Hospital on January 6 for epistaxis. In the course of a superficial investigation a very definite thrill, systolic in time, was observed over both carotid arteries, being more marked on the left side; on further examination the thrill was felt below the clavicles over the axillary arteries, but not over the præcordium proper. The cardiac dullness is within normal limits, and the apex-beat is in the fifth space just internal to the nipple. A loud systolic murmur is audible over the entire præcordium, the point of maximum intensity being over the aortic area; at the base it is conducted into the large trunks arising from the aorta, and may also be heard in the domes of the axillæ. At the apex it has precisely the same character as at the base, but is not quite so loud; it is transmitted to the axilla, and is distinctly audible all over the chest behind.

The patient is an anæmic child of intelligent appearance, and there is nothing in his past medical history to suggest acquired morbus cordis. I regard the case tentatively as one of congenital aortic stenosis.

A Case of Graves' Disease in a Lad aged 8.

By ERIC PRITCHARD, M.D., and SYDNEY STEPHENSON, C.M.

H. A., AGED 8 years, attended the Queen's Hospital for Children on December 6, 1909, as he was said to suffer from a degree of mental dullness. His father had been an inmate of an asylum for two years. The thyroid was enlarged, and its isthmus was quite palpable. There was slight proptosis. No tremor of hands.

On examination in the ophthalmic department of the hospital the condition was found to be as under: Graefe's sign well marked, especially as regards the left eye; slight exophthalmos; the eyes were perpetually watering (Berger's sign); the thyroid gland was obviously enlarged. Pulse 82 per minute. Moebius's sign not present. The retinal veins were rather tortuous. Right vision, $\frac{5}{60}$; − 0·5 D. sph., with − 5·0 D. cyl. axis, 180° $\frac{5}{6}$. Left vision, $\frac{5}{12}$; + 0·5 D. sph., with − 4·0 D. cyl. axis, 170° $\frac{5}{6}$ partly.

Malignant Diphtheria with Multiple Lesions in an Infant.

By J. D. Rolleston, M.D.

A female child, aged 6 weeks, hand fed, was admitted to the Grove Fever Hospital, on December 4, 1909, on the fourth day of disease, with membrane on the tonsils, anterior pillars, uvula, pharynx, and epiglottis. Membrane was also seen in the anterior nares, on the buccal mucosa extending inwards from each corner of the mouth, on the labia majora, and on the skin round the anus. From all these lesions diphtheria bacilli were cultivated. Twelve thousand units of antitoxin were given subcutaneously on admission, and on each of the two following days. Palatal palsy occurred on December 6, and death took place the same day. The temperature was subnormal throughout the child's stay in hospital. The autopsy showed double broncho-pneumonia, and an infarct in the lower lobe of the right lung. An ante-mortem clot was found in the right ventricle. A specimen of the fauces and pharynx was exhibited.

Dr. J. D. Rolleston said that this case was of interest on account of the early age of the patient, the multiplicity and unusual situation of the lesions, and the malignant character of the attack. The rarity of diphtheria during the first year, and especially during the first two months of life, was illustrated by the following figures: Of 7,285 cases of diphtheria admitted to the Grove Fever Hospital between August 23, 1899, and December 31, 1909, only seventy-six, or 1·04 per cent., were under 1 year, and of these only four, including the present case, were under 2 months of age.

Diphtheria in infants is often confined to the nose, in some cases it affects the larynx as well, but in the present case not only were the fauces, nose and epiglottis attacked, but also the buccal mucosa and skin of the ano-genital region. The ano-genital diphtheria was probably caused by auto-inoculation of pre-existent lesions due to diarrhœa. Cutaneous diphtheria, co-existent with diphtheritic angina, which was so frequent in the time of Bretonneau and Trousseau, was rarely seen nowadays. The malignant character of the attack was shown by the absence of reaction to large doses of antitoxin and by the early development of palatal paralysis.

The present case had been artificially fed, but children at the breast

were not altogether immune to diphtheria. Epidemics in sucklings had been recorded by Siredey[1] in 1877 at the Hôpital Lariboisière and Hôpital des Enfants Assistés in Paris, by Schlichter in 1892,[2] and by Riethe in 1897[3] at foundling hospitals in Vienna, and there were about a dozen sporadic cases in recent literature of diphtheria acquired by sucklings within the first two months of life. In many, but by no means all, of them the mother or other members of the same family had recently had diphtheria. In the present case the source of infection could not be determined.

A Case of Purpura Fulminans.

By J. D. Rolleston, M.D., and T. McCririck, M.B.

A boy, aged 6 years, with no family history of purpura or bleeding, was admitted to the Grove Fever Hospital at 10.30 p.m. on January 14, 1910, certified as suffering from hæmorrhagic diphtheria. There had been a history of sore throat, headache, and vomiting ten days before admission. No cultures of the throat had been taken and no antitoxin had been given. Between 4.30 p.m. and 5 p.m. on the day of admission his mother had first observed a large bruise on the right thigh. On admission, an extensive blackish-red ecchymosis was seen on the outer side of the right thigh, and there was a similar lesion on the right buttock. The fauces were normal, but there were numerous carious teeth with pus exuding from the sockets. The right submaxillary lymph-glands were enlarged and tender. There was some indefinite desquamation on the trunk. Temperature 100·4° F. During the night and following day the ecchymosis rapidly spread so as to occupy the distribution represented in the photograph taken shortly before death, which occurred at 3.30 p.m. on January 15—less than twenty-four hours after the first appearance of purpura. Apart from a small area over the left elbow, the lesions were confined to the lower limbs. They were markedly tender to the touch, and were accompanied by œdema of the feet and legs.

[1] "De la diphthérie chez les enfants nouveau-nés," *Thèse de Paris*, 1877.

[2] *Archiv. f. Kinderheilk.*, Stuttg., 1892, xiv, p. 129.

[3] *Wien. klin. Woch.*, 1897, x, p. 666.

Death was preceded by extreme anæmia, vomiting, restlessness, and a subnormal temperature. The mind remained clear until the end. No hæmorrhages from any mucous membranes occurred. The vomit consisted of green fluid, and a stool passed a few hours before death was of normal colour and consistency. The urine contained a trace of albumin, but no blood.

Dr. J. D. Rolleston said that this case exactly corresponded to Henoch's description[1] of purpura fulminans in the extreme rapidity of the ecchymosis formation, the entire absence of hæmorrhages from the mucous membranes or in the internal organs, and in its rapidly fatal course. As in Henoch's original cases, nothing was to be found at the autopsy beyond marked anæmia of all the organs, including the brain and suprarenals, hæmorrhage into which had been noted in some cases of



Purpura fulminans.

purpura. Microscopical sections of the liver and kidneys were made by Dr. McCririck and examined by Dr. H. D. Rolleston, who could find practically no morbid change in them. Elliott,[2] of Chicago, had recently collected 56 cases of purpura fulminans, including a personal case; but of these, 18 had hæmorrhages from the mucous membranes, 9 of the 20 on whom an autopsy was performed showed hæmorrhages in the viscera, and 4 recovered, so that comparatively few, like the present case, corresponded to Henoch's original description. The average duration of the 52 fatal cases, according to Elliott, was 52½ hours after the first appearance of purpura; 19, like the present case, died within 24 hours. Had this boy survived longer he would probably have developed gangrene or

[1] *Berl. klin. Woch.*, 1887, xxiv, p. 8.

[2] *Archives of Intern. Med.*, Chicago, iii, 1909, p. 193.

hæmorrhagic bullæ in the lesions, as occurred in several of the cases recorded.

Sixteen of the cases published had followed scarlet fever (besides the eleven mentioned by Elliott were those of Rice-Oxley,[1] Biss,[2] Cullen,[3] Miller,[4] and Bertling[5]). As in one of Henoch's cases, the pre-existence of scarlet fever in the present case was possible, but not certain. In favour of scarlet fever were the suggestive history, the desquamation, the submaxillary adenitis which often occurs in convalescence from scarlet fever and was noted in many of the cases, and the isolation of a streptococcus from the heart's blood which, according to Dr. McCririck, presented the following characters of the *Streptococcus scarlatinæ:* well-formed chains, much acid formation, and marked curdling of litmus milk, and no turbidity in broth or gelatine at 37° C. In any case, it is highly probable that the condition of oral sepsis contributed to the development of purpura.

The diagnosis of hæmorrhagic diphtheria might be unhesitatingly rejected. In the first place, the throat cultures showed no diphtheria bacilli. Secondly, apart from the very rare cases of purpura occurring in convalescence, skin hæmorrhages in diphtheria always occur during the acute stage of a severe attack, and are associated with other signs of malignancy, such as extensive membrane, faucial and palatal œdema, disproportionate adenopathy and fœtor, none of which were present in this case. Lastly, the distribution and size of the skin lesions were quite unlike those seen in hæmorrhagic diphtheria, in which they are almost invariably small and discrete.[6]

The possibility of the case being one of hæmorrhagic smallpox, which Henoch mentions only to dismiss, may also be rejected. The boy had four good vaccination cicatrices, and the character of the onset, attendant symptoms, and distribution of the lesions, as well as the absence of any exposure to infection, entirely negatived such a diagnosis.

Photographs of hæmorrhagic diphtheria and hæmorrhagic smallpox were then shown.

Blood examination by Dr. McCririck: Hæmoglobin, 50 per cent.;

[1] *Lancet*, 1900, ii, p. 485.

[2] *Lancet*, 1902, ii, p. 286.

[3] *Brit. Med. Journ.*, 1903, i, p. 197.

[4] *Lancet*, 1905, i, p. 929.

[5] *Journ. Amer. Med. Assoc.*, Chicago, 1909, ii, p. 383.

[6] *Vide* J. D. Rolleston, Metropolitan Asylums Board Reports, Lond., 1908, "Hæmorrhagic Diphtheria," p. 273.

red cells, 1,780,000; colour index, 1·4; numerous microcytes; no normoblasts nor poikilocytes; white cells, 57,600. Differential count (1000 cells counted): Polymorphonuclears, 63·4 per cent.; small lymphocytes, 26·8 per cent.; large mononuclears, 2·5 per cent.; eosinophiles, 1 per cent.; myelocytes, one of which was eosinophilic, 6·3 per cent.; mast-cells, *nil*. The blood-platelets were not increased. The serum was markedly hæmolytic to normal human corpuscles in twelve hours in 1 in 50 dilution. Streptococco-opsonic index, 2·31. At the autopsy the blood was found to be remarkably fluid and to show no tendency whatever to clot.

A Case of Cyclic or Recurrent Vomiting associated with Hypertrophic Stenosis of the Pylorus.

By A. E. Russell, M.D.

The patient was a boy, C. G., aged 4 years and 9 months. He was admitted into St. Thomas's Hospital on July 24, 1909, and died on August 22. From birth he had been liable every few months to attacks of vomiting associated with epigastric pain. These would come on suddenly and would last for about twenty-four hours, the vomiting being repeated several times in the day. He was much exhausted by the attacks, and would be kept in bed for a few days after one. The mother described him as a nervous child. The bowels were rather constipated and the stools frequently contained mucus. At the age of 2½ years he had measles with bronchitis. The mother has one other child, a girl aged 9 years, who is in good health.

History of Final Illness.

On the evening of July 22 the child complained of epigastric pain and began to vomit, six or eight times daily. The colour of the vomited material was yellow until July 24, when it became coffee coloured. The bowels had been regular up to July 21, but from that date constipation was present. They were opened by enema on July 24.

On admission to hospital on July 24 the child was emaciated, pale, and in a state of collapse, with sunken eyes and feeble pulse. He frequently vomited small quantities of coffee-ground material. His

breath smelt strongly of acetone. The abdomen was retracted, and occasional peristalsis from left to right was observed in the epigastrium. The liver dullness commenced at the sixth rib in the nipple line and extended to the costal margin. Its edge could not be felt. The spleen could not be felt. The thoracic viscera showed no signs of disease. The knee-jerks were normal; plantar reflexes flexor. Temperature, 101·4° F. Urine: Specific gravity, 1039; slightly alkaline with a large deposit of phosphates. It contained no albumin, sugar, bile, pus or blood.

The boy's illness in the hospital can be divided into three stages: (1) The full development of the attack for which he was admitted; (2) A stage of remission; (3) Recurrence of symptoms, with fatal result.

The first stage lasted from July 24 to July 29. On admission the boy was so collapsed that subcutaneous infusion of 1 pint of normal saline was performed, and this was repeated on the three following days. He vomited several times each day, and, as he could retain nothing by the mouth, he was given 5 oz. of normal saline containing 1 dr. of bicarbonate of soda *per rectum* every six hours, and later 1 dr. of glucose was added. Strychnine was given subcutaneously. The urine was very scanty, measuring only 2 oz. to 4 oz. daily; it contained both acetone and diacetic acid. His temperature varied from 99° to 100·2° F. The outline of the stomach could be seen occasionally. Gastric lavage was performed twice and a considerable quantity of gastric contents was evacuated each time. The bowels were absolutely constipated and were opened by enema on July 28.

Second Stage: Stage of remission, July 30 to August 16. The vomiting ceased on the night of July 29. Chloretone, gr. ii, was given on July 30 in a suppository. He was put on small quantities of milk and albumin water by mouth, and, as vomiting did not recur, this was rapidly increased; bread and butter, fish and chicken, were gradually added, and by August 5 he was taking food well. As soon as the vomiting ceased, the urine increased in quantity and averaged about 25 oz. daily. On August 2 it still contained acetone and diacetic acid, but was free from these bodies on August 5.

Third Stage: Recurrence. Death. August 17 to August 22. The vomiting recurred on August 17. Food by mouth was discontinued and the rectal injections were resumed. On August 18 the breath was free from any odour of acetone, and the urine contained neither acetone nor diacetic acid. On August 20 acetone was present both in the breath and in the urine. The vomiting persisted. Any attempt at feeding by the mouth was followed by vomiting. The boy complained of epigastric

pain and slight fullness was observed in that region. Chloretone suppositories were again tried, but on August 22 the temperature rose to 104·2° F. before death.

Autopsy (made by Dr. H. B. Weir).

The body was extremely emaciated. The weight six days before death was only 27½ lb. The stomach was considerably dilated and contained a fair amount of foodstuff. The lumen of the pylorus was very small and its wall was thickened, its length measuring about ¾ in. No ulcer and no scar could be found. The transverse colon presented one portion, 1 in. in length, which was narrowed and collapsed, with its walls in contact; the neighbouring parts of the colon contained gas, and the transition was abrupt. The mucous membrane of small and large gut was healthy to the naked eye. The lower lobe of the right lung showed much purulent broncho-pneumonia. The remaining thoracic and abdominal viscera were normal. Microscopically the pylorus showed thickening of the muscular wall. The liver gave no fat reaction with Scharlach.

Remarks.

A review of the symptoms observed in this case warrants the statement that they are practically identical with those described under the heading of cyclic or periodic vomiting of children. I asked my colleague, Dr. W. S. Colman, to see the child with me, and he regarded it as undoubtedly suffering from that affection.

I have been able to collect only ten other fatal cases. F. Langmead has recorded three [10, 11]. In one, aged 4 years and 11 months, the gastric glands were found to be degenerated with small hæmorrhages between the tubules; in the second, aged 4 years, there was also some degeneration of the glandular epithelium; in the third, aged 4 years and 11 months, there were several shallow follicular ulcers in the stomach. Fatty changes were present in the liver in all three cases. Eleanor C. Jones records one [8], aged 3 years. There was no acetone in the urine; the stomach was normal, and the liver was fatty. Holt records one, without post-mortem details, in which death occurred after an attack lasting seven weeks [6]. Crozier Griffiths records two [4], aged 5 and 6 years respectively, with autopsy in one (aged 6 years) in which were noted necrotic changes in the mucous membrane of the

stomach and intestine, slight parenchymatous alterations in the pancreas, spleen and kidneys, and fatty infiltration in the liver. The body was extremely emaciated. J. Comby notes three fatal cases in boys, each aged 6 years; but gives no details as regards autopsy findings [2].

Current Views of the Pathology of Cyclic Vomiting.

Before discussing any bearing of the above case on this disease, the current views of the pathology of cyclic vomiting will be mentioned very briefly. The discovery of the acetone series of bodies naturally led to the supposition that they must be responsible for the attacks, but with a wider knowledge of the frequency of acidosis under various conditions this view is being modified. Acetone may be found in the breath and urine of children under a variety of conditions. An adequate hypothesis must explain the attacks of vomiting, the presence of the acetone bodies, and the fatty liver which has been found in the majority of fatal cases.

Langmead [11] assumes that some poison is absorbed from the gastro-intestinal tract, which causes the tissues to be flooded by an excess of fat derived from the subcutaneous tissues, an excess which gives rise to imperfect oxidation, and so to acid poisoning. The fatty liver he regards as the result, and not as the cause, of the altered metabolism. Howland and Richards [7] have observed an increased quantity of indican in the urine before and in the first few days of an attack. They assume in common with many writers on the subject, an unstable nervous system, and conclude that, as the result of some shock or excitement, "a diminished power of oxidation results, and the organism loses the power to detoxify substances absorbed from the intestine which have been present there in excess. These circulate in the blood, exciting their poisonous action, and cannot be excreted by the kidneys because they are not brought to them in the proper form. It seems probable that they are excreted and reabsorbed by the stomach and intestine, in the light of which vomiting would appear to be eliminative and thus a protective mechanism." Ewing [3] regards it as "a complex disturbance of metabolism occurring in neurotic and predisposed subjects, in which rapid burning of body fats, defective function of the liver, and poisoning by intestinal putrefactive products are combined." Comby [2], in an exhaustive paper on a series of 100 cases, suggests that the cause of cyclic vomiting is chronic appendicitis, and records eight cases in which appendicectomy was performed for the disease. In five cases no further attacks of vomiting occurred, but in three the attacks persisted. In

Crozer Griffiths' [5] fatal case the appendix had been removed in the last attacked preceding the fatal one. It would therefore seem very improbable that the cause of the disease is to be found in chronic appendicitis.

Poisoning arising from the intestinal tract, with resulting imperfect oxidation of fats and their accumulation in the liver, seems therefore to represent the current views as to the pathology of this remarkable affection. A nervous temperament has also been postulated as a factor, and it appears in some cases to have been associated with a tendency to migraine.

Acute Starvation.

Now one of the most striking clinical manifestations of cyclic vomiting is the rapidity with which extreme emaciation occurs. Whatever view be held as to the causation of the disease, *acute starvation* accompanies it. The phenomena of acute starvation must therefore be considered, or at least those having a direct bearing on the subject.

Acetone bodies rapidly appear in the urine and breath in starvation. Ewing states that a pronounced acetone reaction is present in the urine in a few hours from the beginning of a fast. Howland and Richards state that the acetone bodies may appear in cyclic vomiting from three to four hours after the last full meal. In my case the final attack began on August 17. On the following day there was no smell of acetone in the breath and no diacetic acid in the urine. Both these signs were present on August 20. No note as to their presence or absence was made on August 19. They did not appear, at the lowest estimate, until over twenty-four hours had elapsed.

The relation of starvation to acidosis is a well-established fact and needs no further comment here. The subject has been critically reviewed by Dr. E. I. Spriggs [15], who is also of opinion that the acidosis is secondary to the starvation induced by recurrent vomiting.

Fatty Liver.

The liver has been found to be noticeably fatty in patients who have died of starvation. Leathes, in particular, has discussed the question of so-called fatty degeneration. Lebedeff in 1883 showed that the fat in the liver in phosphorus poisoning was not formed from the proteid of the liver-cells, but was imported from the fat depots of the body. This has been confirmed by Rosenfeld. Leathes and Dudgeon have shown

that the fatty change in the heart brought about by the action of diphtheria toxin is due to the unmasking of fat already present in the heart but not susceptible of histological staining, together with importation of additional fat. Mottram [12] has recently shown that in rabbits and guinea-pigs after twenty-four hours' starvation there is a huge increase in the visible liver fat, and an increase in the percentage of the fat, but owing to the shrinkage of the liver in hunger the rise in percentage cannot always be attributed to an absolute increase of the total fat in the liver, though in many cases a large increase does occur, and that such increase is due to a migration of the depot fat into the liver.

Fatty change would then be reasonably expected to be present in the liver of patients dying of cyclic vomiting, and such appears to be usually the case. It was, however, absent in my case, but the child was very emaciated. In the middle of the attack for which the boy was admitted into the hospital he weighed only 26 lb., a weight which would have been normal at the end of the second year of age, whereas his age was 4 years and 9 months. Lebedeff found no fatty change in the liver of an extremely emaciated man who died from phosphorus poisoning; and Rosenfeld has shown in both dogs and fowls that the amount of fatty change in the liver after phosphorus poisoning varied directly with the amount of fat in the body, and in emaciated animals there might be none appreciable. The fact, therefore, that with extreme emaciation, in which the depot fat must have been all used up, there is no fat in the liver is an additional argument in favour of the view that the fat in the liver is derived from the depot fat. The absence of fat in the liver in the case under consideration may also be partly accounted for by the fact that glucose was administered *per rectum*. The metabolism of fat seems to require the presence of carbohydrate, and the glucose may have enabled it to be oxidized in this case.

I have gone into these details as they tend to show that both the acidosis and fatty liver can be explained by the acute starvation involved in cyclic vomiting, and that the cause of the vomiting is to be sought elsewhere.

The Presence of Hypertrophic Stenosis of the Pylorus in the Author's Case.

The interest of the case under discussion lies in the fact that the autopsy revealed the existence of well-marked hypertrophic stenosis of

the pylorus with dilatation of the stomach. The possibility that this condition may have been the cause of the attacks of cyclic vomiting has therefore to be considered.

Hypertrophic stenosis of the pylorus is an affection of young infants, and the only case at all comparable to the one above described that I have been able to find in the literature was recorded by Dr. F. W. Shaw [14]. Unfortunately, I have not been able to find the original paper, but it is quoted by Dr. J. Finley Bell [1]. The patient was a boy, aged 5 years, subject to recurrent attacks of vomiting and abdominal pain, each lasting several days; death occurred after an attack lasting ten days. At the post mortem a hard mass of muscular tissue was found at the pylorus, and, while the canal admitted a probe under pressure, it was absolutely tight to fluid. No mention is made in the abstract of the acetone bodies.

On examination of the post-mortem records of the fatal cases of cyclic vomiting no mention is made either of stenosis of the pylorus or of dilatation of the stomach. [In his series of 100 cases Comby notes dilatation of stomach as having been present in twenty-five, but gives no details as to the exact physical signs on which he bases the statement.]

Now, whilst it is possible that hypertrophic stenosis of the pylorus might be overlooked at an autopsy on a child past the age at which that condition is apt to be present, yet it is impossible that it could have escaped detection in all. Whilst, therefore, it is obvious that an actual stenosis of the pylorus is not an essential factor in the disease, its presence in this case may supply a clue to the causation of some attacks of this nature. Despite the well-marked stenosis, the attacks were essentially intermittent, and it is submitted that they were due to the occurrence of *pyloric spasm* rendering the obstruction complete.

If we consider the current views of the pathology of hypertrophic stenosis of the pylorus, we find that pyloric spasm is credited by many not only with the production of the symptoms, but with the production secondarily of the muscular hypertrophy. Mr. Shattock supports this view [13], and assumes a hyperæsthetic condition of the pyloric mucosa with reflex closure when the stomach tries to pass its contents into the duodenum. Koplik [9] classifies the cases according as spasm only is present or spasm with muscular hypertrophy. G. F. Still regards spasm as the cause of the obstruction, and further is of opinion that the degree of spasm may vary at different times in the same case [16].

We are probably justified in assuming that in the case under discussion there would be a tendency to muscular spasm. On this

hypothesis, we have an adequate cause for the attacks of vomiting. So long as the spasm lasted, the obstruction would be complete. Should it persist long enough, the phenomena of acute starvation would of necessity follow, with resulting acidosis. The fatal issue followed on the inanition and exhaustion.

In the present vague condition of our knowledge, it is probable that more than one clinical entity lies hidden under the symptomatic and rather misleading name "Cyclic vomiting." It is not suggested that pyloric spasm is the active factor in all; but it is submitted that the evidence of this case goes far to prove that recurrent pyloric spasm is quite sufficient to account for recurrent attacks of vomiting associated with other symptoms, and presenting all the features described as characteristic of cyclic vomiting.

The hypothesis of muscular spasm is consistent also with the fact that in many cases the attack commences absolutely suddenly and unexpectedly. Relaxation of the spasm would be followed by the sudden cessation of the attack which is often a very noticeable feature. In the event of death all evidence of narrowing of the pylorus would have disappeared. It would be premature to speculate on the causes that might lead to pyloric spasm in young children until further observations have been made on cases of this nature.

Bearings on Treatment.

The indications for treatment based on the view that pyloric spasm is the cause of many attacks of cyclic vomiting harmonize with what experience has been found to be useful.

All food should be withheld from the stomach from the commencement of an attack. Lavage would also alleviate symptoms, and might quickly cut an attack short. Rectal feeding with normal saline should be instituted to replace loss of water from the tissues and to relieve the intense thirst. Glucose should be added to the saline, not only as a food, but also because the adequate utilization of fat requires the co-operation of active carbohydrate metabolism. The symptoms attributable to the acidosis may be treated by adding bicarbonate of soda to the rectal feeds. In a severe case subcutaneous injections of saline should be used. Sedatives such as morphia have been used, and would be given either hypodermically or as a suppository. In this case chloretone was tried.

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DISCUSSION.

Dr. Langmead said one should be careful before diagnosing a case as cyclical vomiting, and every child who had attacks of vomiting from time to time could not be said to have this special condition. He would not apply the name to a condition where there was any known morbid anatomy to account for it. The post-mortems he had made in fatal cases of this disease had not had anything wrong with the pylorus; if there had been, it could scarcely have been missed. The ætiology of cyclical vomiting was at present under a cloud, though many causes were alleged. It was certain that acetone and diacetic acid in the urine might be present in many children who showed no symptoms at all; and symptoms very like those of cyclical vomiting might be engrafted on to other diseases. Somewhat similar symptoms, due to acid intoxication, were found in diabetes, in toxic forms of pneumonia, in summer diarrhœa, and in many other states; but there remained a small group of cases in which vomiting recurred from time to time when the patient was very much prostrated by that vomiting. There was a very definite clinical picture. The child had two or three days' malaise, the temperature rose, there was a little abdominal pain, flushed face and bright eyes. The vomiting then started, and

persisted no matter what was given, the vomit containing coffee-grounds material. Air-hunger followed, and acetone, diacetic acid, and β oxybutyric acid were detectable in the breath and urine in considerable quantity. In the fatal cases which he had seen, coma succeeded, with intervals of restlessness, almost as in meningitis, the child struggling and being controlled only with difficulty. In certain cases there was jaundice. The condition ended in a few days, though it might be delayed as long as a fortnight or more. With recovery the change came quite suddenly, and the patients then put on weight again with surprising rapidity. Post mortem, nothing definite was found except fatty change in the liver and other organs. Though he did not say that the fatty liver was the cause, he thought he could pick out from a series of livers those of patients who had died of vomiting, or delayed anæsthetic poisoning, which he regarded as analogous. The organ was a bright canary or buff colour.

Dr. SPRIGGS said that he was in entire sympathy with what Dr. Russell had said in discussing whether acidosis or acetonuria had any part in the causation of such a condition as cyclic vomiting. It had struck him in considering gastric ulcers, delayed chloroform poisoning, acute yellow atrophy, phosphorus poisoning, cyclic vomiting, other gastro-enteritic disorders in children, and the malignant vomiting of pregnancy, that in cases in which acid poisoning occurred with severity, and often with an extreme degree of liver change, there was one frequent factor—namely, persistent vomiting. At the Sheffield meeting of the British Medical Association [1] he had suggested that the severity of the cases followed directly on the vomiting; the acid intoxication, which might still be the cause of death, being not primary but secondary to exhaustion, and to the absence of carbohydrate food—a view he took to be in agreement with that expressed by Dr. Russell in his interesting paper. It seemed unlikely that a fatty liver was the cause of the condition; a fatty liver, like acidosis, would occur rapidly as the result of starvation. The symptoms of diabetic coma, which was the culmination of a chronic acid poisoning, differed in some respects from that of acute acid poisoning such as occurred in the conditions he had referred to. Vomiting was not a common feature of diabetic coma, though it occurred more often when large injections of sodium-bicarbonate solution were made into a vein.

Dr. RUSSELL, in reply, said the name "cyclic vomiting" was bad, because it suggested that the vomiting came on at definitely recurring intervals. "Recurring vomiting," he considered, would be a better term. He did not say every case was due to muscular thickening of the pylorus, but that in this particular case the presence of muscular thickening led to the conclusion that superadded spasm must have been the cause of the attacks. Therefore the suggestion was that in certain cases which presented this symptom-complex, spasm without any muscular hypertrophy might be the cause of the attack; and so in the event of death, the spasm having disappeared, there would be nothing

[1] *Brit. Med. Journ.*, 1908, ii, p. 1161.

to show for it. In some cases premonitory symptoms occurred, but in others the condition started quite suddenly, and that would be consonant with the idea of pyloric spasm. And, as Dr. Langmead said, they also disappeared very quickly, the child being well except for the exhaustion and wasting which had resulted from the vomiting, and that was consonant with the relaxation of the spasm. But he would not say that every case was due to pyloric spasm. He was glad to have Dr. Spriggs's confirmation of what he had said; had he not made his paper so brief, he would have been glad to refer more fully to the work done by Spriggs, Beddard, and others on the subject.

Section for the Study of Disease in Children.

February 25, 1910.

Dr. E. CAUTLEY in the Chair.

Case of Congenital Œdema of the Leg.

By MAYNARD SMITH, F.R.C.S.

FEMALE child. There is well-marked œdematous swelling of the sole and dorsum of the right foot. There is also swelling of the leg on its front and outer aspects. The right leg is ½ in. longer than the left, measured from the knee to the heel. No history of similar conditions in the family can be obtained. The swelling was noticed at birth.

DISCUSSION.

Dr. ESSEX WYNTER suggested that it might be a nævoid condition; possibly a deep one, which did not show on the surface. It might be either lymphatic or vascular. He asked whether Mr. Smith had attempted by continuous even pressure to reduce it.

Dr. F. PARKES WEBER said he thought that, in cases of enlargement of one extremity associated with angiomatous conditions, it was invariable to find some slight increase in the length of the long bones of the affected part, but in this case examination by X-rays was said to show no such enlargement. And as the appearance and the feel of the limb did not suggest any deep angioma, he thought that in the case shown by Mr. Maynard Smith the presence of deep angiomatous formation, as accounting for the local condition, might be decidedly excluded.

Dr. PERNET said he understood that the condition did not run in the family; yet he (the speaker) thought that was a kind of case which came into the family-œdema category, and, supposing the boy survived, might be the starting point of future cases.

Specimen of the Urinary Organs of a Boy who Died apparently from Uræmia; Diphtheritic Membrane being found in the Trachea after Death.

By J. Walter Carr, M.D.

The specimen consists of the urinary organs of a boy aged 12. Both kidneys are shrunken, the right extremely so, and are very granular on the surface. The capsules strip off fairly easily. The kidney tissue in both organs is pale and firm. The right kidney contains a few dilated calices. There is no hydronephrosis. Both ureters are thin walled and dilated throughout. The bladder is considerably dilated and its walls moderately thickened. The wall of the left ventricle was somewhat hypertrophied. The condition present suggests the existence of some obstruction to the outflow of urine, but none could be found. There was some degree of phimosis; the prepuce was firmly adherent to the glands, but did not extend beyond it, and a small soft rubber catheter was passed without difficulty.

The boy was said to have been always delicate, but there was nothing in his history either to explain the origin of his kidney disease or to suggest its presence; whooping-cough was the only acute specific disease from which he was said to have suffered. He was in his ordinary health until January 23. After two or three days of slight malaise he began to suffer, on January 26, from dyspnœa, and was admitted to the Royal Free Hospital on January 29 in an apathetic condition, with rapid and noisy breathing exactly resembling that of diabetic coma. Coarse râles were heard all over the chest, but the air-entry was quite free. Very little urine was obtained, but what was examined was pale, contained a little albumin and no sugar. The breath had a markedly urinous smell. The temperature was sub-normal. The boy rapidly got worse and died comatose on January 31. At the autopsy, in addition to the changes already described, the whole of the lower end of the trachea was found covered with diphtheritic membrane, in which the diphtheria bacillus was isolated. It extended into the bronchial tubes on both sides as far as the third ramifications. There was none on the larynx or pharynx, and a culture taken from the throat two or three days before death was negative. During life the dyspnœa was regarded as probably uræmic

in origin, but the post-mortem findings suggest that it may have been due to the membrane in the air-passages. It is possible, however, that the diphtheria led to the rapid development of acute uræmia in a boy already suffering from advanced renal disease.

DISCUSSION.

The CHAIRMAN (Dr. Cautley) suggested that the child died from diphtheria rather than from uræmia; it was not necessary to invoke the latter as a cause. The condition of the kidneys and urinary passages was compatible with a number of years of life, and he thought from the history it was congenital.

Dr. HIGGS said that, although he did not think it was possible to say that the renal condition was not congenital, the pathological appearance and the clinical condition were very similar to the ordinary small white kidney, a disease of young adult life. In the cases he had seen, though the condition had obviously been present some time, there were practically no symptoms until a few weeks, or occasionally a month or two, before death. The onset had been rather sudden, with malaise, and the patient complained of vomiting or rapid impairment of sight, and it was in that type of renal disease that there were the most marked changes in the fundi. He thought that the urinous odour of the breath in this case indicated that the renal condition was at any rate partly responsible for the death, and that the dyspnœa must have been to some extent of uræmic origin. He asked if the fundi of this case had been examined, either before or after death; also what was the state of the arterial system.

Dr. CARR replied that it was possible the diphtheria was the immediate cause of the death, but the gradually deepening coma was very suggestive of the last stage of ordinary granular kidney as so often seen in adults. It was not the coma of dyspnœa from which the patient suffered at the last; it was too prolonged for that. He agreed that the condition was very possibly congenital, and that would explain the mother's statement that the child had always been delicate. If the mischief were assumed to be primarily in the kidneys, there was no explanation of the marked dilatation of the urinary passages. The fundi were looked at during life, but no satisfactory view was obtained; no marked changes in the disks could be seen, but there was apparently a slight œdema of the retinæ.

Myelogenous Leukæmia in an Infant aged 18 months.

By T. R. Whipham, M.D.

The patient is a male infant, aged 18 months, who seemed healthy until he was one year old. He then began to waste, but showed no other definite symptoms. The father had had syphilis, which was not treated, but there is no other family history bearing on this point. The child was bottle-fed and cut his first teeth at 6 months. He could support his head at 6 to 8 months, but cannot sit up or walk. He is a dark-skinned, flat-nosed child with a widely open anterior fontanelle. There is no bossing of the skull. The ribs are slightly beaded, but there is no enlargement of the epiphyses of the long bones. The lymphatic glands in the neck, axillæ, and groins are just palpable and somewhat hard. The abdomen is greatly enlarged, especially on the left side, where the spleen can be seen extending nearly half across the abdomen and to 4½ in. below the umbilical level. The liver is enlarged and its edge can be felt 1½ in. below the tip of the ninth rib. The other viscera appear normal.

Blood Count (January 28, 1910).

Red corpuscles	4,080,000 per c.mm.
White corpuscles	63,400 ,,
Hæmoglobin	80 per cent.

Differential Leucocyte Count.

Polymorphonuclears	32·4 per cent.
Lymphocytes	5·2 ,,
Large mononuclears	18·8 ,,
Transitionals	12·8 ,,
Eosinophiles	2·8 ,,
Basophiles	0·6 ,,
Myelocytes { Neutrophile	25·8 ,,
Myelocytes { Eosinophile	1·6 ,,
Normoblasts	1·6 per 100 leucocytes.

On admission there was a large subcutaneous hæmorrhage on the extensor surface of the right arm and a similar patch on the right shin. Subsequently other hæmorrhages have occurred on the scalp, left ear, left eyelids, and left side of the face.

Further blood-counts show that the number of leucocytes is increasing; thus on February 8 there were 71,800 per c.mm., and on February 18, 101,000 per c.mm.

The child has been under observation for one month, and seems to be gradually getting worse. The body-weight has dropped, the leucocytosis has increased, and further hæmorrhages have appeared on the scalp, some of which have suppurated. In looking at the differential count, it will be noticed that the number of myelocytes is not so very large: the proportion is not so great as is found in cases of myelogenous leukæmia in adults, and this appears to be usually the case when the disease occurs in children. On the other hand, the number of large mononuclear cells is excessive. It is possible that these large mononuclear cells are the precursors of myelocytes—i.e., myelocytes before they have taken on a fine granulation. In support of this view the work of Opie, Jochmann, and others may be mentioned. These observers found that in the polymorphonuclear cells and in the finely granular neutrophile myelocytes there was a proteolytic ferment which was not present in the smaller lymphocytes. And, again, Donhauser and Longcope, in investigating a case of acute lymphatic leukæmia, found that the large lymphocytes contained the same ferment. This seems to point to some morphological connexion between the two.

For the past two weeks Mr. Higham Cooper has had the patient under treatment with X-rays. The child has been given exposures of five minutes' duration three times a week, but without any appreciable benefit up to the present.

The interest of the case lies in the fact that it is one of myelogenous leukæmia occurring at a very early age. Lymphatic leukæmia is not so very uncommon in childhood, but the myelogenous variety is distinctly rare, and especially so in infancy. In his Goulstonian Lectures for 1904 Dr. Hutchison remarks that after searching the literature he could only find five genuine cases under the age of 10 years. These were respectively of the ages of 9, 8, 8, 5 years and 1 year. Dr. Osler's earliest case of leukæmia was in an infant aged 8 months, and, though it is not clear in his "Principles and Practice of Medicine" whether it was of the myelogenous or lymphatic variety, he states that it was of the former type, and adds: "There have been, of course, congenital cases." Since 1904 the records of only two undoubted cases of myelogenous leukæmia in children can be found. One is reported by Zylberlast[1]

[1] "Un cas de leucémie myéloide chez un enfant de neuf mois," Thesis, Geneva University, 1907.

of an infant, aged 9 months, in which the leucocytes numbered 81,000 per c.mm., the myelocytes being 25·7 per cent., the large mononuclear cells 34·3 per cent., the polymorphonuclear 31·9 per cent., and the eosinophiles 2·5 per cent. The other is recorded by Ménetrier and Aubertin.[1] The child was 4 months of age, and the number of leucocytes was 87,100 per c.mm.; of these, 41·9 per cent. were myelocytes, 25·4 per cent. mononuclears, and 25·5 per cent. polymorphonuclears. Ginsburg[2] also claims to have seen a case in a child aged 2½ years, but no record is given of the number of leucocytes or of the proportions of the various cells.

DISCUSSION.

The CHAIRMAN said such cases were so rare that he asked Dr. Whipham whether he felt any hesitation in accepting it as a case of the condition. They were difficult to diagnose, and differential counts of blood in such young children were somewhat unreliable, as the blood so readily reverted to the infantile type. Did Dr. Whipham base his diagnosis on the symptoms, or on the blood-count in conjunction with enlargement of the spleen, or on the myelocytosis? He had seen cases of so-called splenic anæmia, or pseudo-leukæmia infantum, in which the patient had been going rapidly down hill and large numbers of myelocytes had appeared in the blood, yet subsequently the child improved and eventually recovered. The liver in the present case was only a little enlarged.

Dr. F. PARKES WEBER said he did not think myeloid leukæmia in infants and children, though extremely rare, was quite so rare as Dr. Whipham appeared to think. A few other cases had been published in recent years.[3] He thought Dr. Whipham had amply proved his diagnosis; even from the blood-film which he showed under the microscope it could be established. He doubted whether any child with similar splenomegaly, hepatomegaly, and blood-state had actually recovered.

Dr. D. FORSYTH said the case reminded him of one he had seen last year of myelogenous leukæmia in a woman who had been recently confined. After the birth of her child she was found to have a greatly enlarged spleen and liver, and

[1] "La leucémie myéloide," Aide Mémoire Léauté, Section du Biologiste, 1906.

[2] "Ueber einen Fall von akuter myelogener Leukämie im Kindesalter," Dissertation, Zürich University, 1905.

[3] *See* Benjamin's case of acute myeloid leukæmia in a boy aged 9, shown at the Münchener Gesellschaft für Kinderheilkunde, *Münch. med. Wochenschr.*, 1908, lv, p. 484; I. A. Abt's case in a boy aged 10, described in *Arch. of Pediatrics*, New York, 1908, xxv, p. 794; Ziegler and Jochmann's case in a boy aged 15, *Deut. med. Wochenschr.*, 1907, xxxiii, p. 749; Bingel's case in a girl aged 19, *Münch. med. Wochenschr.*, 1910, lvii, p. 268; H. Hirschfeld's case in a boy aged 6, *Berl. klin. Wochenschr.*, 1907, xliv, p. 772.

when admitted to hospital her blood presented the characteristic changes. Her baby, then a fortnight old, was examined, but had neither an enlarged spleen nor any blood changes. It was a noteworthy fact that the maternal blood presented striking abnormalities, and yet on the other side of the placenta healthy conditions prevailed.

Dr. WHIPHAM, in reply, said it would be interesting to hear Dr. Cautley's own diagnosis after his remarks. He was interested to hear from Dr. Weber that other cases of the kind had been recorded. He (Dr. Whipham) went through the *Index Medicus*, and those which he had mentioned were the only ones he could find referred to. Dr. Forsyth's remarks were interesting as showing that the disease was not necessarily hereditary. It was remarkable that normal blood should be circulating on the fœtal side of the placenta when the mother's showed such marked abnormalities. At a future date he might be able to give the further history of the present patient.

A Case of Non-cretinous Mental Deficiency with Goitre.

By DAVID FORSYTH, M.D.

THE patient, a boy aged 5½, and well up to average height and weight, has suffered from fits since his second year. He is restless, talkative, and decidedly odd in his manner. His face is elongated and his expression vacant. His forehead is narrow and asymmetrical, the left temple being more hollowed out than the right. His eyes have marked epicanthic folds. His ears are large, showing Darwin's tubercles, and his palate is very high and narrow. He presents a moderate, non-cystic enlargement of the thyroid gland affecting both lobes and the isthmus. He has no other indication of thyroid disease.

Dr. FORSYTH added that no family history of thyroid trouble could be obtained. Since the patient's admission his goitre had varied in size, and at present the right lobe was larger than the left and the isthmus was definitely enlarged. In answer to the Chairman, he said he had not inquired from the mother whether she had taken chlorate of potash during her pregnancy.

Sequel to a Case of Chloroma.[1]

By W. Essex Wynter, M.D.

The child lingered till Thursday, February 10, dying of asthenia. There was some wasting, and the complexion assumed a yellowish-green tint towards the end. Double optic neuritis developed and epistaxis occurred.

The blood condition previously reported became more accentuated; the final report by Mr. G. R. Ward, of Westminster Hospital (February 1, 1910), being as follows:—

Erythrocytes		770,000 per c.mm.
Normoblasts	949 per c.mm.	1,020 ,,
Megaloblasts	71 ,,	

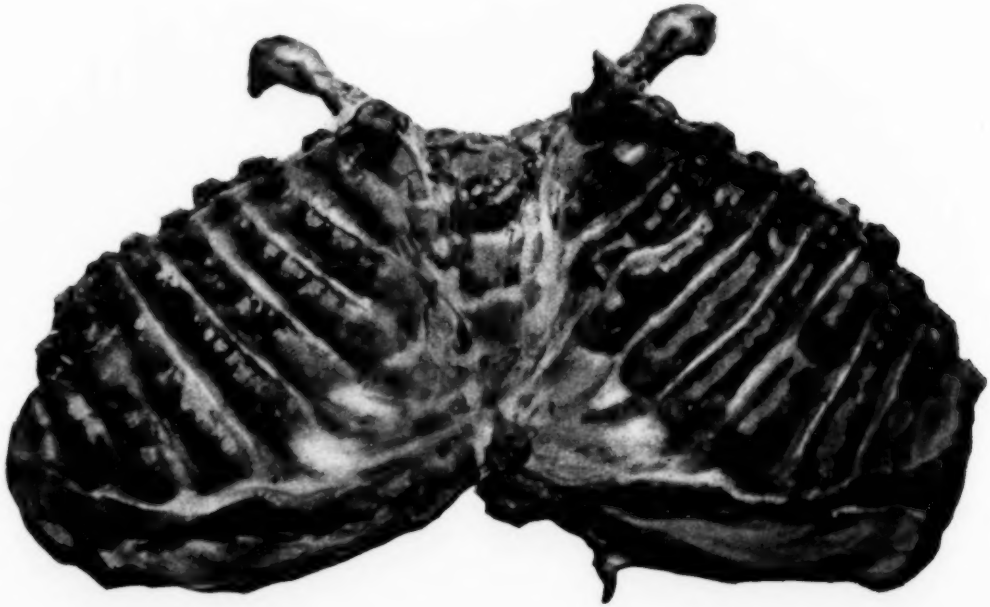
Polychromasia and basophile granulation well marked. The red corpuscles resemble those of pernicious anæmia, but there is less poikilocytosis. The cells measure from 4 μ to 6 μ (average 8 μ to 10 μ).

Hæmoglobin (Gowers)	24 per cent.
Index value	1·55
Leucocytes	170,000 per c.mm.
Polymorphonuclears	30·6 per cent.
Small mononuclears	6·8 ,,
,, ,, (chloroma cells)	62·6 ,,
Eosinophiles	*Nil*
Mast cells	*Nil*
Myelocytes	A few large ones

One thousand cells were counted. All gradations between a typical polymorph and a large monomorph were seen. The count was made on a film stained by Giemsa's method. No mitoses were seen. The mononuclears varied from 12 μ to 20 μ.

The autopsy revealed diffuse chloromatous growth, transforming all the red marrow and being conspicuous in the flat bones and ends of the long bones. These also showed deposits beneath the periosteum. Glands in the iliac, vertebral and cervical regions were diffusely affected, and the spleen was tinted of a similar colour. But there were no

[1] Shown at the meeting on October 22, 1909; *vide Proceedings*, p. 18.



Inner Surface of Ribs and Sternum. [*From a colour photograph (Lumière process) by Mr. Somerville Hastings.*]

distinct secondary growths in the organs or elsewhere. The heart showed marked tabby-cat striation.

Specimens exhibited: Plastron, vertebral column, right femur, dura mater, calvarium; with colour-photograph and water-colour drawings.

Examination of the pigment by Dr. A. E. Garrod showed that it corresponded closely with that of other cases, but was a rather duller green.

DISCUSSION.

Dr. LEDINGHAM said he was much interested in the specimens, as they presented in some particulars a striking similarity to the condition met with in a case which he had examined for Dr. Spriggs at the Victoria Hospital for Children, and which would be published shortly. The condition was taken to be chloroma during life, but at the post mortem there was only a slight greenish tinge of the skin of the face and scalp. No actual tumours were found in the scalp, but there was a remarkably diffuse subperiosteal infiltration forming thick sheaths round all the long bones. The dura mater was similarly thickened and infiltrated, and sticking into it were enormous numbers of osteophytes. The skin over the scalp and round the orbits and malar bones was similarly affected. The lymph glands and bone-marrow were infiltrated, and there was a microscopical infiltration of the kidneys, but not of other organs examined. He thought it was impossible to divorce chloroma from acute lymphatic leukæmia, as some had attempted to do; there was possibly a whole series of intermediate types. Cases had been recorded in which the greenish coloration of the tumours had been only slight, and in others it had been absent altogether. On the other hand, cases had occurred in which the subperiosteal infiltrates had been absent, while the green tumours were confined to other organs. Dr. Wynter's case presented infiltrates round the long bones (as in Dr. Spriggs's case), which was rather strange, as he (the speaker) had been under the impression that chloromatous tumours were confined to the skull-cap, vertebræ, sternum, and ribs. He regarded this case, and also Dr. Spriggs's case, as transition types between acute lymphatic leukæmia and chloroma.

Dr. WYNTER replied that the case presented similar features to that just mentioned; though there were no deposits in the organs, there was a greenish tinge around the apex of one or two of the pyramids in one kidney. It might be that the material was secreted by way of the kidney, and became deposited on its way; but there was nothing suggestive of the ordinary secondary deposits from new growths, either in the lungs, liver, or spleen. He believed that in rickets it was usual to find something representing the red bone-marrow immediately beneath the periosteum, and in growing bones it might not be altogether abnormal for a layer corresponding to red marrow to exist under the periosteum. It was probably the transformation of that which accounted for the presence of the chloroma material along the shaft of the femur.

Cyst of the Humerus: Fracture; Operation.

By Hugh Lett, F.R.C.S.

H. R., a boy aged 13, knocked down on August 11, 1909, fell on to his shoulder. He was brought to the London Hospital and admitted. On examination, the upper end of the humerus was much thickened for about 2 in. below the head of the bone. It was firm, no egg-shell crackling could be obtained, and it was not particularly tender on palpation. A fracture was discovered a short distance below the surgical neck. A skiagram showed expansion and rarefaction of the upper 3 in. of the diaphysis of the humerus, with trabeculæ passing across the clear area. A short distance above the lower margin the bone was fractured.

Previous history: Was in the hospital two years previously under another surgeon. A diagnosis was then made of endosteal sarcoma, and amputation advised. Operation was refused. He remained well until his re-admission. In 1898 tuberculous disease of the right hip developed, with subsequent abscesses and sinus formation. The joint was excised on April 4, 1899.

Operation was performed on August 20, 1909: An incision 4 in. long made on the antero-external aspect of the arm. The periosteum was exposed and appeared to be normal; it was then reflected from the expanded bone. The shell of bone was opened by the raspatory and a thin mucoid fluid escaped. The affected part of the diaphysis was rapidly removed. There was then an interval of 3 in. between the epiphysis and the healthy part of the shaft. The bone removed from this region was replaced by an ebony rod such as is used for securing the intestine outside the abdomen in the operation of colotomy. The point was inserted into the epiphysis, and the blunt end into the medullary cavity of the shaft. In this way the epiphysis was firmly connected with the shaft by a rigid rod. The periosteum was sutured, and the wound closed.

The wound healed by first intention, and subsequent progress was uneventful. A poroplastic splint was moulded to fit the shoulder and extended down the forearm as far as the wrist. The elbow was flexed to a right angle; the splint was secured by straps passing round to the opposite side of the chest. Three months after the operation the splint was removed and the patient given regular massage.

At the present time movement of the right upper extremity is

perfect, and the muscles are rapidly increasing in size. The new bone is firm and regular, and slightly thicker than the opposite humerus. The accompanying skiagrams show the gradual deposit of new bone from the periosteum, and absorption of the ebony rod.

On microscopical examination no cartilage nor other tumour-cells were found. The condition appeared to be one of cystic fibroma.

Mr. LOCKHART MUMMERY said Mr. Lett was to be congratulated on the surgical result in the case, especially when it was remembered that in many such cases the arm was amputated. In this case amputation had previously been recommended. He remembered the case of a girl, aged 12, whose arm was amputated at the shoulder-joint for supposed sarcoma, and a cyst at the upper end of the humerus was found. There seemed to be no means of making a certain diagnosis between cyst of the humerus and sarcoma, as no surgeon would like to cut into a sarcoma first, which he proposed to amputate, because of the risk of infecting the tissues. The obvious thing to do seemed to be to cut a piece out for microscopical examination some time previous to the operation. The lesson of the case was the need for being quite certain of the diagnosis before amputating; he was sure there had been cases of needless loss of a limb owing to lack of that precaution.

Specimen of Hypertrophic Stenosis of the Pylorus.

By J. PORTER PARKINSON, M.D.

THIS specimen is from a female infant aged 5 weeks and 3 days at the time of death. The infant was a small baby at birth, bottle fed, and took food well; did not vomit till twenty days old. It never cried much. Bowels very constipated from birth.

On admission (January 29) the infant weighed 6 lb. 11 oz. Temperature 99° F. to 100° F. Vomited after feeds once or twice daily; motions very hard, yellow. Abdomen not distended, but occasionally a projection appeared below the left costal margin, which gradually travelled towards the right side, above the level of the umbilicus. On deep palpation a small, hard nodule, about the size of the tip of the finger, could be felt in the mid-line, half-way between the xiphisternum and the umbilicus; at times this could not be felt. The infant was treated by lavage and small frequent feeds, but steadily lost weight. I asked my colleague, Mr. Drew, to see the child, and it was agreed to wait for a time before surgical treatment was attempted. The child continued to vomit, and

on February 10 the weight had fallen to 5 lb. 9 oz. Strength rapidly failed, and the infant died on February 15.

At the necropsy the stomach was removed entire, and displaced 2½ oz. of water, so it was slightly dilated, and, no doubt, was larger during life. The coats are thickened. The pylorus is thickened for about an inch into a hard, elastic-feeling mass, the lumen being so encroached on that I was unable to pass an ordinary probe through it. The intestines seemed somewhat shrunken. Apart from a fatty liver, there were no other obvious morbid changes.

DISCUSSION.

The CHAIRMAN said it was a good illustration of the usual result of attempting to cure the condition by medical means. There were still many people who supported the view that these cases were curable in this manner, and possibly a slight degree of hypertrophy might be so cured, but not one which produced marked obstruction. It would be found that in those hospitals where medical treatment was relied on, the museums had most of such specimens to show. The strongest argument in favour of treating such cases medically was that the condition was due to spasm. Dr. Morse, in America, reported a case which had been operated upon by gastro-enterostomy, and died six months subsequently. The post-mortem examination showed that the condition of the pylorus was quite unaltered. It was difficult to believe that after a satisfactory gastro-enterostomy a spasmodic condition of the pylorus could be maintained for six months.

Mr. LOCKHART MUMMERY said he had operated upon two such cases, and could endorse what Dr. Cautley had said. There might be cases which could recover by medical means, but in the two cases he operated upon, and two others he had seen which had not been operated upon and died, the thickening was so great that he did not see how any medical means could have improved the condition to any extent. If the surgeon was to have any chance, the cases must be operated upon early.

Dr. C. R. BOX said he was in favour of early operation, but he believed there were two sides to the question, and some of these cases did recover without. If it was a congenital condition, as he himself thought, and not due to spasm, and was found in the adult, it must have lasted from birth. In St. Thomas's Museum there was a specimen from a patient aged 61, and another from an adult whose age he had forgotten; so that the condition could not appreciably interfere with life in all cases. The man aged 61 died from a condition not related to the stomach.

Dr. PARKINSON replied that quite early in the case, as he thought he could feel the pylorus and the hypertrophied wall of the stomach, he called a surgeon in as soon as he could be obtained, and that gentleman thought it would be well to wait a short time. When cases were as bad as that, the sooner they were

operated upon the better. In old days, for carcinoma of the œsophagus patients were tried with one thing and another, and when operation was at last resorted to they were practically at their last gasp. The cases such as he had described, if at all severe, never recovered by medical means, whatever form that took, and medical treatment was out of place.

Case of Congenital Absence of the Left Femur.

By P. Lockhart Mummery, F.R.C.S.

A male child, aged 2 months. The left femur is absent, and there is no thigh. (The knee-joint appears to be normal, but is close up in the position of the hip in a normal child.) The left leg, below the knee, is



Congenital absence of left femur.

quite normal, but is rotated outwards. The child is the youngest of three, both the others being quite normal; the parents are also normal, and the labour was easy and natural. The X-ray photograph shows that the diaphysis of the left femur is missing. The lower epiphysis of the femur appears to be present, and there is a small island of cartilage just above this; otherwise the femur is absent. (The tibia and pelvis on the left side are quite normal.) The child shows no other congenital defect and is a healthy baby.

Congenital Thoracic Deformity: Absence of Part of the Pectoralis Major.

By E. I. Spriggs, M.D.

The patient is a boy aged 11. The sternocostal part of the pectoralis major is absent on the left side. The lower boundary of the remainder passes from the arm to the second rib, but no fibrous band can be felt, such as has been described in similar cases. The pectoralis minor appears to be absent also. The nipple is normal in appearance, but is higher and nearer the middle line than that on the right side. The mammary fat is wanting. The boy's arm is strong, and his mother did not notice the deformity until it was pointed out to her after his admission to the Victoria Hospital for Children, for hæmaturia.

A number of thoracic anomalies of this nature have been described. Eighty-nine were collected by Dr. John Thomson, three of which came under his own observation, and were published in January, 1895.[1] Dr. Thomson's plates (thrown on the screen by the epidiascope) show the same deformity. In some of these cases the costal cartilages and anterior ends of the ribs may be wanting in the mammary region, only a thin layer of skin and subcutaneous tissue protecting the lung. Such a case was shown by Dr. Thomson to members of the Section at its visit to Edinburgh last summer. In others the hand and forearm show deformities, and, failing any known developmental cause of a malformation which may affect so many structures, the condition was ascribed by Froriep, in 1839, to pressure of the upper limb of the affected side upon the thorax *in utero;* this view is supported by Dr. Thomson in the paper referred to above.

The Chairman said both those congenital conditions were very rare. With regard to the pressure theory of causation, in Dr. Spriggs's patient it was worth mentioning that in some cases there was webbing of the arm to the chest, and in others webbing of the fingers, conditions which could scarcely be explained by pressure.

[1] *Teratologia*, Lond. and Edin., 1895, ii, p. 1.

Section for the Study of Disease in Children.

April 22, 1910.

Mr. MILNER BURGESS in the Chair.

Eighty consecutive Cases of Wasting Infants fed on Undiluted Citrated Milk.

By FREDERICK LANGMEAD, M.D.

As is well known, the method of modifying cows' milk by the addition of citrate of soda was first advocated by Sir Almroth Wright [15]. It was based theoretically on the influence which calcium salts had been proved to exert on coagulability, and especially on the experiment of Arthus and Pagés, who showed that the clotting of milk could be entirely prevented if the calcium salts in it had been precipitated by the addition of oxalates and fluorides. Sir Almroth Wright substituted citrate of soda for these poisonous salts, and showed that it was equally efficacious as a means of influencing the coagulability of milk. By a series of experiments *in vitro* he demonstrated not only that the coagulation of milk could be entirely prevented by the addition of this salt, but that the softness and fineness of the curd varied directly with the amount of sodium citrate used. Here, then, was an obvious means of combating the chief source of the digestive disturbances of bottle-fed babies—the toughness and firmness of the curd of cows' milk—and rendering it like the more soft and flocculent curd of human milk. The relative amount of calcium salts in cows' milk and human milk substantiates this view. Cows' milk is the food provided by nature for the growing calf, an animal possessing a large quantity of rapidly

growing bone, and therefore requiring a correspondingly large supply of calcium salts, shown by Bunge to amount to 0·17 per cent. Human milk, on the other hand, is destined for a progeny in whom bone formation is relatively slow, and contains only 0·03 per cent. of lime salts.

The profession has to thank Dr. F. J. Poynton [11] for re-introducing to its notice this valuable method of feeding, which had been in abeyance for about nine years. In 1904, in a paper in the *Lancet*, he recorded the success which he had obtained by its means among fifty infants who had been brought to the Hospital for Sick Children, Great Ormond Street, suffering from various degrees of nutritional and digestive disturbance. He, however, combined citration with dilution. Since then many papers have been written fully bearing out the opinion which he had formed of the great value of citration, and the method has come into very general use. It is, however, almost always employed in combination with the older method of dilution as advocated by Dr. Poynton, and few practitioners have followed out the original suggestion of Sir Almroth Wright, of giving whole milk to which sodium citrate only had been added. Although it has been proved beyond any shadow of doubt that the administration of citrate of soda profoundly alters the digestibility of cows' milk and renders it more suitable for the infant, yet the principle on which it is given has not escaped criticism. Aibinder [1] contends that sodium citrate not only does not precipitate the salts of lime, but actually re-dissolves them after they have been precipitated by other means. He believes that the sodium citrate in some way checks the action of the gastric juice. Dr. T. Wood Clarke [5], at the Rockefeller Institute, who has conducted a series of experiments on the effect of various milk modifiers on the gastric digestion, attributes the value of sodium citrate to the fact that it acts on the hydrochloric acid of the stomach, converting it into sodium chloride, and thus markedly reduces the available acid.

About three years ago I set out to determine whether it was necessary to dilute as well as to citrate the milk. The importance of this was borne in upon me by the obvious disadvantages of dilution, especially among the hospital class of cases. Over and over again one sees infants who have been fed on diluted milk and who, for no reason unconnected with their feeding, show the effects of malassimilation and indigestion, as evidenced by marasmus, vomiting, constipation, abdominal distension and pain, rickets and other dietetic disturbances, and defects.

Disadvantages of Dilution.

This state of things can be partly explained, at any rate, by the disadvantages which diluted milk must always possess. In the first place, the addition of water or other diluent increases the bulk of the meal. This is probably a matter of some moment, for with large meals there must necessarily be a greater tendency to distension and atony of the stomach wall; also it is no uncommon experience to have to deal with a child who regularly shows signs of pain and discomfort after being fed by diluted milk, but who is comfortable if the volume of the feed is reduced. If this be done there is, however, the danger of underfeeding to be considered.

Secondly: If milk be diluted, the fat and sugar are reduced to proportions which are below that in human milk, and inadequate for healthy growth. There is need to add cream and sugar, and in amounts varying with the dilution employed. Although this is the orthodox procedure, very little experience shows that it is one which often is not adhered to. Not only is this so among the poor, who cannot possibly afford to buy the necessary cream, but also among their richer neighbours, who have never been informed of the necessity for it. Most samples of cream, especially in the summer months, contain preservatives, which, however harmless they may be to adults, are unsafe for use in infant food. So frequently is some preservative used in cream that it may almost be taken for granted that any cream obtained by the hospital class of patients would contain it. Under the directions of Dr. Collingridge, twelve samples were obtained recently in order to determine the extent to which preservatives are used in preparing cream for retail in the City of London. Ten of these contained boracic-acid preservatives, and three—i.e., one-fourth—contained it in excess of the limit, 0·25 per cent., recommended in the Departmental Committee's report. Dr. Hamill [9] has also shown that benzoate or salicylate of sodium is sometimes used, and that the cream is not uncommonly thickened with gelatine, starch paste, or sucrate of lime.

Thirdly: The varying degrees of dilution suitable for different ages constitute a serious drawback. If a certain dilution has been prescribed at any time, it happens not infrequently that the baby is kept strictly to that dilution long after the necessity for it has passed, and consequently is in danger of underfeeding. The complexity of the dilution method often supplies too great a task for the mother, entailing as it does

a knowledge of the dilution suitable for each month and the amount of cream and sugar to be added.

Fourthly: Dilution only, without citration, often leaves the chief source of digestive disturbance—the toughness of the curd—untouched. It is true that dilution with plain boiled water diminishes the bulk of the coagulum, but its consistence is the same as before, since the proportion of casein to lime salts remains *in statu quo ante*. Barley water is given with the intention of physically influencing the toughness of the curd, but it must be remembered that, as ordinarily made, it contains from 1 to 2 per cent. of starch. Lime water, contrary to former belief, either maintains or enhances the acidity of the gastric juice (Clarke [5]). It has, however, been shown that the acid curds of milk are not the cause of indigestion, but those produced by rennin, for, as Sir Almroth Wright pointed out, under ordinary circumstances the latter only are formed when milk is taken into the fasting stomach. In certain cases, however, hyperacidity undoubtedly influences milk digestion, and under these circumstances citration is advantageously combined with the addition of sodium bicarbonate.

Whole-milk Feeding.

Under these circumstances it seemed that it would be a great boon if the original suggestion could be complied with, and dilution left out of the process altogether. The giving of whole milk is, of course, no novelty. It has long been recognized that certain strong, healthy babies will digest unmodified and undiluted cows' milk, but these constitute only a small minority. In Paris, Professor Budin [3] had vast experience in the administration of whole milk in distribution centres or *gouttes de lait*, and his results were entirely favourable. This was ascribed by Chavane to differences in the curd, dependent upon the sterilization of the milk at 100° C. for 15 minutes, which Professor Budin employed. "According to my experiments *in vitro*," writes Chavane, [4] "the clot of milk thus sterilized offers no resistance or sense of elasticity to the finger on pressure; it yields like a thick fluid such as cream, and on microscopical examination, although the fat globules are not appreciably altered, the particles of casein are found to be smaller and more homogeneous." Following Budin's work, many observers—Variot [14], Comby [6], Lazard [10], Drapier [7], Ruffié [13], Bonifas [2], Gillet [8], to mention some of the more prominent—strongly corroborated his views as to the efficacy of undiluted sterilized milk.

Before I employed undiluted citrated milk, I knew also of some few children in England who had been fed upon it from earliest infancy, and had grown strong and well.

Method of Procedure.

The exact plan which I have followed at Paddington Green Children's Hospital must be described. When the baby is first brought to the out-patient department, supposing it is a case which is not thriving, it is weighed, and its weight is recorded on a chart which has been devised for the purpose. In the case of breast-fed children, an attempt is made to avoid artificial feeding, and the breast is continued, reinforced by additional milk for the mother, combined sometimes by the administration of cod-liver oil to the infant. Only after this has been found fruitless by future weighings is weaning permitted, and even then the mother is advised to give the breast night and morning still. By taking these precautions unnecessary weaning has often been avoided, and occasionally after a few weeks the breast milk has improved sufficiently in quality and quantity to allow of breast feeding alone.

When artificial feeding has been decided upon, the number of feeds per diem and the amount of each meal are then arranged. No hard-and-fast line can be drawn either with regard to the frequency or the size of the feeds, since so much depends upon the vigour of each particular child, on the degree to which it has lost ground, and on its previous feeding ; but the following rule affords a useful guide :—

By Day.

	1—5 weeks	5—10 weeks	10 weeks—6 months
Feeds ...	2 hourly ...	2½ hourly ...	3 hourly

By Night (10 p.m. to 7 a.m.)

	1—3 weeks	3—6 weeks
Feeds ...	2 meals ...	1 meal

After six weeks a slight readjustment of meals can be made, so that the baby goes without food from 11 p.m. until 5 a.m., which it is quite able to do. For quantities I was guided at first by the following table, slightly modified from "Holt's Diseases of Childhood" :—

	Feeding quantities oz.		Approximately oz.
First week	1 —1½	=	1
Second—third week	1½—3	=	2
Fourth—fifth week	2½—3½	=	3
Sixth—twelfth week	3 —4½	=	4
Third—fifth month	4 —5½	=	5
Fifth—ninth month	5½—7	=	6
Ninth—twelfth month...	7½—9	=	8

This table was intended for strong healthy children, and, as I have had to deal with children in various stages of malnutrition and maldevelopment, I soon found that its amounts were too great for my purposes, and I have since relied simply on the weight and condition of the baby. In such cases it is important to start well below the theoretically adequate amount and to feel one's way from week to week.

My experience of citrated milk feeding for infants during the first fortnight of life is very small, and although cognizant of several instances where citrated undiluted milk has proved quite satisfactory even at this early age, I should not now be disposed to employ it. In the breast, as Dr. Pritchard [12] has pointed out, " the transition from colostrum to coagulable milk is relatively slow; it may be ten days or more before the mammary secretion assumes the characters of true milk. During this time the stomach has been functionally developing, acquiring tolerance to the presence of coagulated casein, and learning to peptonise or liquefy the clot soon after its formation. There can be no evasion or short-circuiting of this route." Dr. Dicks, who has been employing citrated whole milk for babies born in a London infirmary and has been following the scheme given above, tells me that although many new-born infants flourish on it, others are not so successful. The material, however, which he has had to deal with is of the poorest class. Under these circumstances, and having in view the nature of the mammary secretion at this period, the safest plan is to give whey during the first fortnight of life, rather than risk promoting early indigestion, which will want combating later. The objections to citrated milk in the first two weeks apply, of course, with greater weight to milk which has been simply diluted.

Having decided on the amount and frequency of the feeds, I then prescribe to each ounce of milk ordered 2 gr. of citrate of soda, and this is dispensed in the form of a watery solution, one drachm of which has to be added to each feed ; for example, if 4 oz. of milk are given every three hours, the prescription is thus :—

Sod. citratis	... ...	8 gr.
Aquam	... ...	ad 1 dr.

Every three hours.

The mother is instructed to bring the milk to the boil and then to add one teaspoonful of the solution to each meal. If preferred, the sodium citrate may be added to the milk scalded in bulk, and this is more convenient in private practice. A good plan in private practice is to order always 20 gr. of sodium citrate to the drachm of water. A drachm of the solution is then needed for each half pint of milk used, and a 6-oz.

bottle of the solution will last a fortnight or so, depending of course on the amount of milk given.

By bringing the milk to the boil, we render innocuous the living pathogenic micro-organisms, not present as spores, and guard against infection with tubercle bacilli. Recent investigations have shown that this is more than a little important, since we now know that bovine tuberculosis may infect the human being, and also that tubercle bacilli are found in anything from 7 per cent. to 23 per cent. of samples of milk collected. I have found also that the clot *in vitro* of citrated milk which has been brought to the boil is finer than that which has been pasteurized or merely warmed.

The mother is directed to bring the child again, at first weekly, afterwards every fortnight or three weeks. The baby is weighed at each attendance, and the amount of milk adjusted to its needs as shown by the scales, the general condition, and the mother's account. The mother's opinion as to whether the baby is satisfied or requires more is not to be lightly disregarded, and affords valuable help in deciding as to the amounts to be given. Citration is gradually lessened at about five months, and can usually be omitted altogether at about six months, when the arrival of teeth permits of unaltered milk and a more extensive dietary. Occasionally, however, in particularly delicate babies, citration has to be continued beyond the usual period.

Advantages.

One of the chief advantages of citrated whole milk is its simplicity. The mother cannot easily make a mistake. All that is necessary is to add a teaspoonful of the solution to a bottle of milk. Thus, much manipulation, with its attendant risks and mistakes, is avoided. This, of course, applies most forcibly to the hospital class of cases, for whom, too, it is especially suitable on account of its cheapness. The disadvantages of dilution, the bulkiness of the feed, the complexity of the monthly variations, the dangers of artificially preserved or thickened cream, or of giving a food containing too little fat, are all evaded. There is no danger of scurvy. In my cases I have often been struck by the firmness of the muscles in babies fed by this method, as contrasted with others—a better guide to the state of nutrition than is the weight. It affords the advantage of easy digestibility, which condensed and desiccated milks possess, without their disadvantages. That it more readily agrees with the infant's powers of digestion than diluted milk

has been noticeable frequently in my cases, and a return to simple dilution has often been attended by failure to gain weight, if not by disaster. A few examples may not be out of place:—

Case 39.—Becky L., aged 6 weeks, was brought for wasting, screaming, and constipation. She then weighed 6 lb. 2 oz. only, and showed the usual signs of marasmus. She had been fed on cows' milk and barley water in correct proportions; 2 oz. of citrated milk were ordered, and 1 lb. 4 oz. was gained in the first fortnight. The mother then gave cows' milk and barley water again

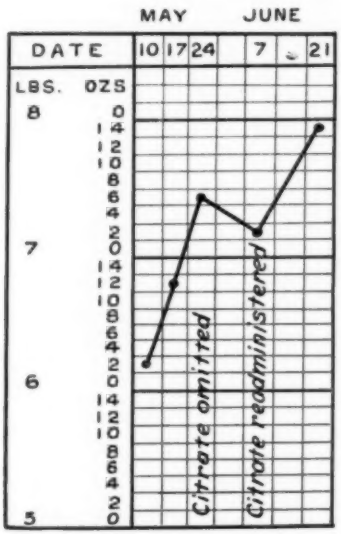


CHART I.

Case 29. B. L.

on her own responsibility, and during the next fortnight the weight dropped by 4 oz. Citration was restarted, when 7 oz. were added in each of the subsequent two weeks. (*Vide* Weight Chart I.)

Case 72.—Christopher C., aged 4 months, was brought for marasmus. He had been fed on cows' milk and barley water and weighed 9 lb. 8 oz. He gained satisfactorily with citrated whole milk until aged 8 months, when his weight was 13 lb. 10 oz. Citration was then stopped, and during the next four weeks 3 oz. were lost. Citration was restarted, and coincided with increase in weight again, 1 lb. 10 oz. being steadily gained in the following six weeks. (*Vide* Weight Chart II.)

Case 61.—Alfred R. first attended when aged 3 months. He then weighed 8 lb. 10 oz. and was brought for wasting. He was breast-fed, and this was continued with the addition of cod-liver oil, but the wasting continued. Citrated whole milk was started, and continued for four weeks, during which 1 lb. 6 oz. was gained. The mother then stayed away, and gave the baby equal parts of cows' milk and barley water, 2 oz. of each for five weeks. During that time the weight only increased 1 oz. Citration was restarted and 6 oz. was gained in each of the successive weeks. (*Vide* Weight Chart III.)

Another advantage of less importance is the absence of that frequency of micturition which is so troublesome in the case of the baby fed on diluted milk.

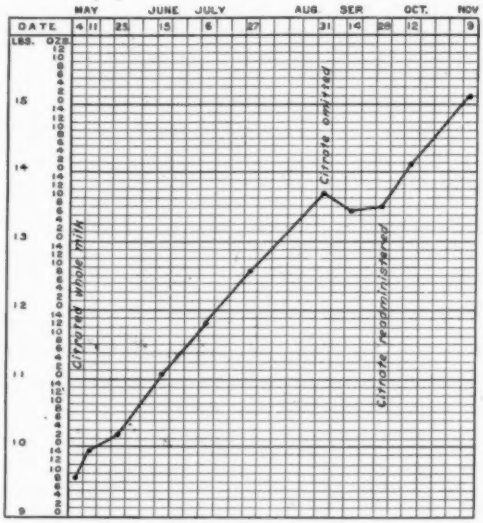


CHART II.

Case 72. C. C.

No better testimony to the value of citrated whole milk could be adduced than that of the favour which it has met with at the hands of the sisters in the children's wards at St. Mary's Hospital. In both the medical and surgical wards at that institution, for about the last three years it has been the custom for the infants to be fed in this way, unless explicit orders for special feeding are given. Both the sisters tell me they prefer whole citrated milk to any other method of infant feeding, not only because of the little trouble which it entails, but because of the

way in which the babies are satisfied, the little discomfort they have, and the way in which they progress. Both are enthusiastic in their opinion, and say that they are quite sure that the wards are quieter and the babies sleep better and do better generally than in the old days, when diluted milk and cream were given as a routine.

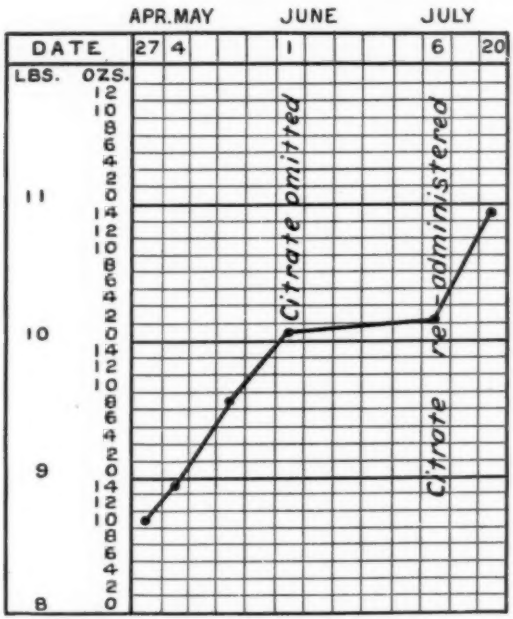


CHART III.

Case 61. A. R.

DISADVANTAGES AND LIMITATIONS.

The chief objection to whole milk is the *excess of proteid* which it contains, but this difficulty is more apparent than real, for the excess is simply passed by the bowel. That it is the curd and not merely the excess of protein which matters is fully shown by the experience of Professor Budin [3] and his disciples, and in the cases about to be recorded. Another disadvantage which has been advanced is the likelihood for *constipation* to be produced. Many of my cases were constipated, but they were so when first brought and before citration had

been started. I have not been able to satisfy myself that it is more common than with other methods of feeding. *Thirst* may be apparent, but may easily be treated by teaspoonfuls of warm water between the feeds if necessary. It is quenched much better thus than by diluting the milk.

A danger which has to be mentioned is the likelihood of producing general œdema by decreasing the coagulability of the blood. I can only say that in my experience, amounting to about 150 cases, I have never yet seen either œdema or urticaria. In the one or two cases of which I have heard the citrate had been given in far too large doses. It is a complication which need not be considered if 2 gr. only are added to 1 oz. of milk.

If the solution is kept too long in a warm place, a fungus is apt to grow on its surface; but this may be avoided by the addition of a few drops of chloroform water. With regard to the *limitations* of whole citrated milk, I need scarcely mention that it is not suitable for those rare cases where milk in any form is not tolerated, but they are very rare. In hypertrophic stenosis or other obvious organic cause of malassimilation this method fares no better than others. In epidemic diarrhœa it is, of course, unsuitable, as is milk in any form.

Occasionally *acid dyspepsia* is the cause of want of growth and development, and of obvious distress after food. Under these circumstances sodium bicarbonate should be given also, to neutralize the excess of acid. The following case is an example:—

Fred R. first attended when aged 3 months. He had been fed on the breast entirely, but was losing weight, jaundiced, very constipated, and screaming after food. Attention to the bowels did not relieve the indigestion or increase the weight. Citrated whole milk was given at first, in addition to the breast, and afterwards alone, as the breast milk stopped. There was a little improvement and gain in weight, but screaming and discomfort continued after meals, with some abdominal distension. Two grains of bicarbonate of soda were now added to each feed, and this was later increased to 4 gr. added to 3 oz. of milk. The indigestion was relieved, comfort was obtained after food, and about 7 oz. were gained weekly. He is now seven months old, healthy, and being fed on unmodified milk, with additional fat and carbohydrate.

Analysis of Cases.

Eighty consecutive cases are comprised in this paper. This does not include all those treated, but such only as have been accessible enough to permit of regular weighings. No selection has been made. It must be

acknowledged that several cases not included only attended for once or twice, as is usual in hospital practice, but nearly all those who came twice showed already an increase in weight before they were lost sight of. Of the eighty cases, forty-one are girls and thirty-nine boys, and the ages at the first attendances vary from three weeks to four months.

Previous Feeding.

When first seen thirty were being fed on cows' milk and barley water, and four on cows' milk and boiled water. It is significant that in only two of these cases was any attempt made to add extra fat, in one in the form of cream in quite inadequate amounts, and in the other as cod-liver oil. In a third case maltine had been added. One was receiving milk, lime water, and cream. In one case, the milk was being peptonized, and in another humanized. Seven were being fed on condensed milk, in one case supplemented by Sanatogen, and in another by Empress wheaten flour. Patent foods were being given in eleven instances and had previously been given in seven more. Altogether seventeen varieties of patent foods had been tried in these cases. In one instance albumin water only was being given. Twenty-one were on the breast, in one case supplemented by condensed milk and in another by nursery biscuits.

Weights on First Attendance.

The poor state of nutrition of the cases when first seen can be best gauged by the weights at that time. Four weighed under 5lb., their ages being 3, 4, 8, and 10 weeks respectively. Ten others weighed 6 lb. or under, their ages varying from 5 weeks to 3 months. The lightest baby weighed 4 lb., and was 3 weeks old. All weighed below the average and had been wasting when first given whole citrated milk. Three, according to the mother's statements, were brought because they were given up by their doctors, and in one case a death certificate had already been promised.

Results.

The results of this method of feeding have been very gratifying. All gained in weight. This was especially surprising seeing that previously all had been wasting, and on very many kinds of foods. I have knowledge of only one fatality. This was that of a very

ill-developed undersized child who first attended, aged 2 months, and weighing 8 lb. 2 oz. For fifteen weeks he gained an average of 3·53 oz. weekly, but died of epidemic diarrhœa in September. Other members of the family had diarrhœa also.

In one case the citrated whole milk was supplanted by another preparation. According to the mother it was being pumped up whilst condensed milk was retained, and as the child was not progressing I allowed the condensed milk to be given temporarily, and afterwards lost sight of the patient. It is important that the presence of thrush was coincident with the want of progress.

In a third case the citrated milk was given as a means of assisting breast feeding, and after about a month was unnecessary.

Three were the subjects of rickets. One of these had rickets before coming under treatment; one had as well as rickets, congenital syphilis, hernia, phimosis, and bronchitis. The remaining seventy-four cases were all well and healthy when last seen. Five developed diarrhœa and vomiting in the summer months, from which they recovered. There was no case of gastro-intestinal disturbance apart from zymotic diarrhœa sufficiently severe to produce loss of weight.

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Case	Name	Date of first attendance	Age	Previous feeding	Reason for attendance	Weight at first attendance	Weight when citrate stopped	Average gain per week	Duration of citrate feeding	Remarks
1	Doris N.	April 8, 1907	11 weeks	Cows' milk and water	Losing weight	10 lb. 6 oz.	15 lb. 12 oz.	3·7 oz.	22 weeks	Seen again at 14 months; healthy
2	Gladys B.	May 27, 1907	4 mos.	Cows' milk and barley water; Ol. morrhuæ	Wasting and incessant crying	10 lb. 2 oz.	16 lb. 10 oz.	7 oz.	15 weeks	Mixed diet at 8 months; healthy
3	Fred B.	May 3, 1907	10 weeks	Breast	Wasting, constipation; breast milk stopped	7 lb. 10 oz.	14 lb. 7 oz.	5·7 oz.	19 weeks	Healthy when attendance ceased
4	John H.	May 10, 1907	6 weeks	Breast one month, then cows' milk and barley water	Wasting, vomiting everything	5 lb. 10 oz.	13 lb. 4 oz.	5 oz.	24 weeks	Healthy at 1 year
5	Adeleine L.	Sept. 13, 1907	3 mos.	Breast and Robb's biscuits	Not getting on	6 lb. 9 oz.	13 lb. 10 oz.	6·27 oz.	18 weeks	Lost weight on breast only; healthy when discharged and seen at 2¾ years; strong and healthy
6	Edith W.	Oct. 18, 1907	6 weeks	Cows' milk and barley water	Wasting since birth	5 lb. 6 oz.	13 lb. 2 oz.	4·42 oz.	28 weeks	Seen when aged 2 years; healthy
7	Jane E.	Jan. 10, 1908	1 month	Allenbury's No. 1	Getting smaller since birth	8 lb.	17 lb. 3 oz.	5·25 oz.	28 weeks	Seen at 9 months; healthy
8	William S.	Jan. 24, 1908	3 mos.	Cows' milk and barley water	"Shrivelling"; given up by doctor	6 lb.	8 lb. 4 oz.	5·75 oz.	8 weeks	Certificate of death had been promised by doctor; at end of eight weeks took unaltered cows' milk
9	William B.	Jan. 24, 1908	2 mos.	Cows' milk ½ oz., barley water 1 oz., water 1 oz.	"Ravenous"; always vomits food	8 lb. 7 oz.	15 lb. 8 oz.	8 oz.	14 weeks	Healthy when citration ceased
10	Frank L.	Jan. 31, 1908	3 mos.	Breast	Wasting and "pain in stomach"	6 lb.	8 lb. 4 oz.	7·2 oz.	5 weeks	Premature birth (7 months); no gain on breast only, nor when Ol. morrhuæ added; vomited when citrate omitted for one week
11	A. M.	Feb. 28, 1908	9 weeks	Breast	Constipated, and smaller than at birth	6 lb. 6 oz.	7 lb. 4 oz.	2·2 oz.	6 weeks	Twin; alternately with breast for two weeks without gain; then citrated milk, but no gain in weight, although digestion normal, until cod-liver oil added; probably due to poor milk

12	Florence S.	April 6, 1908	3 mos.	Cows' milk and barley water	Not getting on; motions always green	7 lb.	12 lb. 14 oz.	5·2 oz.	18 weeks	Developed diarrhœa and vomiting in August, and lost weight; afterward citrated milk; started again with rapid regain
13	Alfred H.	May 25, 1908	7 weeks	Nestlé's milk, then cows' milk and barley water	Wasting, constipation	8 lb.	14 lb. 9 oz.	4 oz.	26 weeks	Spare, delicate baby; some rickets
14	Ethel B.	May 4, 1908	2 mos.	Milk, barley water, cream	Wasting and vomiting	4 lb. 10 oz.	15 lb. 7 oz.	7·52 oz.	23 weeks	Premature baby (7 months), healthy
15	Rose M.	Aug. 28, 1908	4 mos.	Cows' milk and barley water	Wasting and diarrhœa	9 lb. 12 oz.	15 lb. 6 oz.	7 oz.	13 weeks	Stopped away for six weeks and given milk and barley water; during this time weight stationary and developed diarrhœa and vomiting
16	Kathleen B.	June 26, 1908	3 mos.	Mellin's food; Savory and Moore's food; cows' milk and barley water	Wasting and vomiting	6 lb. 8 oz.	17 lb. 9 oz.	7 oz.	25 weeks	Healthy when seen at 14 months
17	Cissie E.	July 27, 1908	4 mos.	Breast	Losing weight	10 lb. 10 oz.	16 lb. 2 oz.	5·5 oz.	16 weeks	Tried for one week on breast and lost 6 oz.; healthy when citration stopped
18	Margaret D.	July 24, 1908	2 mos.	Cows' milk and barley water	Vomiting and constipation; losing weight	6 lb. 8 oz.	14 lb. 5 oz.	4·63 oz.	27 weeks	Had bronchitis, during which weight remained stationary for about one month; healthy
19	Harold C.	Sept. 7, 1908	2 mos.	Breast	Crying after breast; wasting	8 lb. 4 oz.	14 lb. 14 oz.	6 oz.	18 weeks	Tried breast for one week, reinforced by Ol. morrhuæ; did not gain; healthy and strong when discharged
20	Susie B.	Sept. 7, 1908	2 mos.	Milo food; Allenbury; Nestlé's milk; Sanatogen	Wasting, diarrhœa, occasional vomiting	7 lb. 12 oz.	11 lb. 12 oz.	5·8 oz.	11 weeks	Healthy when ceased attending
21	Elizabeth C.	Oct. 19, 1908	6 weeks	Nestlé's milk; Allenbury's No. 1	Screaming; constipation	6 lb. 10 oz.	13 lb. 12 oz.	5·42 oz.	21 weeks	Healthy when citration stopped
22	Thomas H.	Nov. 2, 1908	3 mos.	Breast	Wasting	7 lb. 12 oz.	16 lb. 5 oz.	8 oz.	17 weeks	Breast milk insufficient, also with Ol. morrhuæ; discharged healthy
23	William M.	Nov. 30, 1908	2 mos.	Breast, then cows' milk and barley water	Wasting, vomiting	5 lb. 14 oz.	15 lb. 6 oz.	4·34 oz.	35 weeks	Twin; very weakly child; mother told by doctor would not live two days; also suffered from congenital syphilis, hernia, phimosis, bronchitis; undersized, and some rickets

Date	Name	Date of first attendance	Age	Previous feeding	Reason for attendance	Weight at first attendance	Weight when citrate stopped	Average gain per week	Duration of citrate feeding	Remarks
24	FlorenceY.	Dec. 7, 1908	7 weeks	Cows' milk and barley water	Wasting, vomiting, constipation	5 lb. 6 oz.	7 lb. 2 oz.	3·5 oz.	8 weeks	More comfortable from the first, but did not gain weight for first five weeks, then rapidly
25	William D. M.	Dec. 18, 1908	3 mos.	Allenbury's No. 1; barley water and milk 2 : 1; then lime water and milk 2 : 1; cream	Vomiting everything, wasting, constipation	6 lb. 4 oz.	12 lb. 8 oz.	3·3 oz.	30 weeks	Twin; given up by two doctors; delicate baby; developed diarrhœa and vomiting in June for one week and temporarily lost weight; normal but small when discharged; seen at 1 year and 5 months; healthy
26	Rose W.	Jan. 4, 1909	2 mos.	Breast	Wasting, milk failing, constipation	8 lb.	15 lb.	6·2 oz.	18 weeks	Lost weight with partial breast-feeding; healthy when discharged
27	Alfred F.	Jan. 24, 1909	2 mos.	Nestlé's milk	Wasting	8 lb. 14 oz.	10 lb. 6 oz.	3 oz.	8 weeks	Twin; delicate baby; developed retropharyngeal abscess, and transferred to surgical side
28	Jane F.	Feb. 15, 1909	3 mos.	Allenbury's No. 1	Vomiting	9 lb. 4 oz.	11 lb.	2·8 oz.	10 weeks	Started on very small quantity (1 oz. 2-hourly) because of vomiting, and so no increase until 3 oz. given six weeks later; then gained 8 oz. per week; healthy at 1 year
29	Ellen H.	Jan. 18, 1909	3 weeks	Breast	Wasting	4 lb.	11 lb. 14 oz.	5·72 oz.	22 weeks	Congenital syphilis; breast given as well at first, but milk went
30	John W.	Feb. 15, 1909	4 mos.	Breast three months; milk and barley water; infantina	Wasting, undigested motions	8 lb.	15 lb. 2 oz.	4·38 oz.	26 weeks	Developed epidemic diarrhœa in August, 1909, at 10 months; citration had been reduced to 1 gr. to the ounce of milk; seen at 15 months; healthy
31	Lily C.	Feb. 19, 1909	5 weeks	Nestlé's milk	Wasting	5 lb. 12 oz.	12 lb. 12 oz.	3·4 oz.	30 weeks	Developed bronchitis, accompanied by offensive motions, at 5 months; mixed diet at 9 months; healthy
32	Winifred D.	Feb. 26, 1909	3 mos.	Cows' milk and water	Vomiting, thrush, wasting	5lb. 10 oz.	8 lb. 12 oz.	7 oz.	7 weeks	"8 months' baby"; one tablespoonful of water given with each feed; healthy when discharged on whole milk

33	Winifred H.	Mar. 8, 1909	4 mos.	Breast fed, then milk and water, equal parts; then Savory and Moore's food	Losing weight; pain	6 lb. 10 oz.	11 lb. 4 oz.	6 oz.	14 weeks	Improved more rapidly when lime-water also added; healthy when ceased attendance
34	Hilda L.	Mar. 15, 1909	2 mos.	Breast fed	Wasting; milk going	5 lb. 12 oz.	17 lb.	6 oz.	30 weeks	Breast given twice daily at first; value of change shown by three weeks in country, when 1 lb. per week gained; bronchitis at 9 months, otherwise healthy; seen at 14 months; strong and healthy
35	Ruby W.	April 19, 1909	2 mos.	Breast fed, then cows' milk and barley water	Vomiting, constipation	8 lb. 5 oz.	9 lb. 10 oz.	5·25 oz.	4 weeks	Failed to attend after 1 month; healthy when last seen
36	Fred. W.	April 26, 1909	7 weeks	Cows' milk and barley water; breast for one month	Not satisfied; wasting	8 lb. 6 oz.	15 lb. 10 oz.	5·5 oz.	21 weeks	Discharged healthy
37	Edward P.	May 3, 1909	3 mos.	Nestlé's milk, then cows' milk and water	Wasting, vomiting	8 lb. 12 oz.	10 lb. 12 oz.	11 oz.	3 weeks	Discharged healthy
38	John S.	May 3, 1909	3 mos.	Cows' milk and barley water	Wasting, constipated	9 lb. 4 oz.	13 lb.	5 oz.	12 weeks	Discharged healthy on unmodified milk
39	Becky L.	May 10, 1909	6 weeks	Cows' milk and barley water	Wasting, screaming, constipation	6 lb. 2 oz.	7 lb. 14 oz.	4·6 oz.	6 weeks	Went without citrate for one week, and lost consequently 4 oz.; not seen after 3 months old
40	Marion Y.	May 10, 1909	1 month	Cows' milk and barley water	Wasting	4 lb. 2 oz.	8 lb. 8 oz.	5·4 oz.	13 weeks	Expected to die at first; after bad progressed favourably, developed whooping-cough, followed by diarrhœa and vomiting; lost sight of
41	Eva K.	May 17, 1909	2 mos.	Cows' milk and barley water	Screams, vomiting	9 lb.	14 lb. 14 oz.	4·47 oz.	21 weeks	Discharged healthy
42	George L.	June 11, 1909	11 weeks	Breast fed; milk gone	Thrush, wasting	8 lb. 8 oz.	11 lb. 10 oz.	3·57 oz.	14 weeks	Stationary for first three weeks, due to thrush; discharged healthy
43	Alfred B.	June 14, 1909	5 weeks	Breast	Wasting	7 lb.	12 lb. 14 oz.	5·2 oz.	18 weeks	Mother given extra milk at first and cod-liver oil given to baby, but lost 8 oz. and weaning imperative; discharged healthy
44	May A.	June 25, 1909	3 mos.	Thomson's malted food	Wasting, thrush	8 lb. 10 lb.	10 lb. 2 oz.	4·4 oz.	7 weeks	Irregular in attendance; gaining well when last seen
45	Maurice F.	July 5, 1909	1 month	Milk and barley water	Screaming, vomiting, wasting	6 lb. 2 oz.	8 lb. 10 oz.	5 oz.	8 weeks	Developed zymotic diarrhœa in September; discharged healthy

Case	Name	Date of first attendance	Age	Previous feeding	Reason for attendance	Weight at first attendance	Weight when citrate stopped	Average gain per week	Duration of citrate feeding	Remarks
46	Fred L.	July 12, 1909	7 weeks	Allinson's food	Wasting, constipation	8 lb. 6 oz.	18 lb.	7 oz.	22 weeks	Seen at 9 months; healthy
47	Leslie G.	Nov. 8, 1909	10 weeks	Breast two months; then Savory and Moore's food, then cows' milk and water and maltine	Wasting, vomiting; masses of curd	9 lb. 12 oz.	12 lb. 6 oz.	5·25 oz.	8 weeks	Healthy when last seen
48	Blanche C.	Nov. 19, 1909	2 mos.	Empress wheatened flour, with Nestlé's milk	Wasting, constipation	8 lb. 8 oz.	10 lb. 8 oz.	8 oz.	4 weeks	Healthy when last seen
49	Alfred R.	Nov. 22, 1909	4 mos.	Breast fed for six weeks, then Nestlé's milk	Losing weight; screaming, constipation	10 lb. 3 oz.	14 lb.	10 oz.	6 weeks	Healthy when last seen
50	Charles C.	Nov. 29, 1909	3 mos.	Breast two months, then Ridge's food	Screaming and diarrhœa	11 lb. 4 oz.	13 lb. 8 oz.	7·20 oz.	5 weeks	Healthy when last seen
51	Eva C.	Nov. 5, 1909	10 weeks	Breast fed and Nestlé's milk	Wasting and constipation	6 lb. 8 oz.	7 lb.	1·6 oz.	5 weeks	Mother saw that the citrated milk was pumped up but Nestle's retained; as not progressing well, Nestle's allowed; thrush also coincident with want of improvement
52	Frank B.	Nov. 29, 1909	4 mos.	Cows' milk and barley water, Nestlé's milk, Horlick's malted milk	Wasting	8 lb. 12 oz.	10 lb. 10 oz.	4·28 oz.	7 weeks	Improving; still under care
53	Amy A.	Dec. 13, 1909	10 weeks	Breast fed	Wasting	4 lb. 13 oz.	8 lb. 6 oz.	5·18 oz.	11 weeks	Breast continued for 1 week and lost 2 oz.; then citration started; still under observation; improving
54	Wm. T.	May 31, 1907	2 mos.	?	Wasting	8 lb. 2 oz.	11 lb. 7 oz.	3·53 oz.	15 weeks	Very undeveloped undersized child; developed epidemic diarrhœa in September together with other members of family; died
55	George K.	Jan. 18, 1908	3 weeks	Breast fed	Wasting	7 lb.	8 lb. 14 oz.	3·3 oz.	9 weeks	Stationary for 1 month because of bronchitis; then gained well
56	Kate M.	Jan. 8, 1909	4 mos.	Cows' milk and barley water	Wasting, vomiting	11 lb. 2 oz.	14 lb.	7·3 oz.	6 weeks	Healthy when discharged

57	Gladys B.	May 27 1907	4 mos.	Cows' milk and barley water	Not getting on	10 lb. 2 oz.	16 lb. 10 oz.	7·42 oz.	14 weeks	Healthy when seen at 8 months
58	Alice C.	April 6, 1908	4 mos.	Cows' milk and barley water	Wasting	8 lb. 4 oz.	12 lb.	4 oz.	15 weeks	Developed measles; seen at 1 year; strong and healthy
59	David P.	May 10, 1909	3 mos.	Cows' milk and barley water	Wasting, vomiting	9 lb. 8 oz.	13 lb. 6 oz.	5·63 oz.	11 weeks	Healthy when discharged
60	Agnes J.	Feb. 22, 1909	2 mos.	Breast	Wasting	7 lb. 8 oz.	9 lb. 4 oz.	9·3 oz.	3 weeks	Breast milk and cod-liver oil failed to increase weight; then citration started
61	Alfred R.	April 27, 1909	3 mos.	Breast	Wasting	8 lb. 10 oz.	10 lb. 4 oz.	5·14 oz.	7 weeks	Breast milk and cod-liver oil tried but lost weight; left off; citration for one month, when weight remained stationary; citration restarted and 12 oz. gained in next fortnight; healthy
62	John O'C.	June 17, 1909	4 mos.	Cows' milk and barley water	Wasting	6 lb. 8 oz.	11 lb. 6 oz.	6 oz.	13 weeks	? Healthy, except developed otorrhœa
63	John L.	May 3, 1907	10 weeks	Cows' milk and barley water	Wasting	8 lb. 9 oz.	14 lb. 13 oz.	8·3 oz.	12 weeks	Healthy when discharged
64	Thos. H.	April 22, 1907	3 mos.	?	Wasting	7 lb. 9 oz.	10 lb. 7 oz.	7·3 oz.	6 weeks	Healthy when last seen
65	Bella T.	May 6, 1907	11 weeks	?	Wasting	7 lb. 2 oz.	9 lb.	3 oz.	10 weeks	Did not gain for one month; then improvement
66	Edna Q.	May 6, 1907	6 mos.	Cows' milk and barley water	Wasting	12 lb. 9 oz.	16 lb. 2 oz.	4 oz.	14 weeks	Lost weight for one week, due to gooseberries; healthy when last seen
67	George T.	Sept. 23, 1907	1 month	Breast only	—	—	—	—	—	Given citrated milk, to assist breast feeding; and breast found necessary after about one month
68	Alice B.	Mar. 9, 1908	2 mos.	Breast	Wasting	7 lb. 2 oz.	9 lb.	7·5 oz.	4 weeks	Breast continued at first and cod-liver oil given, but lost weight until citrated milk started
69	May G.	May 10, 1907	4 mos.	Cows' milk and barley water	Wasting	7 lb. 2 oz.	9 lb. 10 oz.	6·6 oz.	6 weeks	Healthy
70	Thomas S.	Feb. 3, 1908	8 mos.	Nestlé's milk	Wasting; "won't take food"	9 lb. 2 oz.	10 lb. 13 oz.	5·4 oz.	5 weeks	Rickets when first seen; improved
71	Prudence H.	Jan. 13, 1908	3 weeks	Cows' milk and barley water; then Dunhardt's food	Vomiting; wasting	6 lb. 12 oz.	12 lb. 6 oz.	4·7 oz.	19 weeks	Developed broncho-pneumonia and away for 3 weeks on whole uncitrated milk; returned, having lost 1 lb. 2 oz., but soon regained it; discharged healthy

Case	Name	Date of first attendance	Age	Previous feeding	Reason for attendance	Weight at first attendance	Weight when citrate stopped	Average gain per week	Duration of citrate feeding	Remarks
72	Christopher C.	May 4, 1908	4 mos.	Cows' milk and barley water	Wasting	9 lb. 8 oz.	15 lb. 14 oz.	3·6 oz.	24 weeks	Gained weight rapidly with citrated milk; then mother stayed away, and uncitrated milk given; weight remained stationary and citration had to be restarted, when rapid gain again
73	George M.	July 6, 1908	2 mos.	Cows' milk and water, then Nestlé's milk	Motions offensive; wasting	9 lb. 4 oz.	11 lb. 2 oz.	10 oz.	3	Then ceased attendance
74	Walter F.	May 13, 1907	10 weeks	Nestlé's milk	Wasting	7 lb. 9 oz.	13 lb. 10 oz.	6 oz.	16 weeks	Healthy when last seen, aged 6½ months; had some diarrhœa and vomiting for 1 week; cause not ascertained
75	Cyril H.	June 3, 1907	10 weeks	Cows' milk and barley water, then Nestlé's milk, then peptonised milk	Wasting, vomiting everything, screaming	9 lb. 6 oz.	15 lb. 8 oz.	4·9 oz.	20 weeks	Healthy when seen, aged 9 months
76	Eliz. L.	Sept. 25, 1907	2 mos.	Breast fed	Wasting, constipated	5 lb. 12 oz.	8 lb. 8 oz.	4 oz.	11 weeks	Healthy when last seen
77	Edith P.	Sept. 2, 1907	2 mos.	Cows' milk and barley water, virol, then albumin water only	Persistent vomiting; "given up by doctor"	6 lb. 13 oz.	15 lb. 8 oz.	5 oz.	28 weeks	Citration stopped at six months, but gain also ceased, so citration continued until ninth month; discharged healthy
78	Ernest H.	June 28, 1907	3 mos.	Cows' milk and barley water, then Horlick's malted milk, then "Dr." Clarke's food	Wasting, screaming after food	8 lb. 14 oz.	16 lb. 2 oz.	9 oz.	13 weeks	Discharged healthy
79	Walter J.	Dec. 13, 1909	5 weeks	Breast	Wasting	9 lb. 9 oz.	11 lb. 4 oz.	7 oz.	4 weeks	Attempted to continue breast, by giving extra milk to mother; steady loss in weight, however, until citrated milk started; still under treatment
80	Bertie G.	Feb. 7, 1910	3 mos.	Humanized milk	Motions like chalk; diarrhœa; not gaining	10 lb. 10 oz.	11 lb. 10 oz.	5·3 oz.	3 weeks	Still under treatment

DISCUSSION.

Dr. SPRIGGS said that Dr. Langmead was to be heartily congratulated on having put together such a series of cases. When a new method of treatment was introduced it was essential that such collections should be made, in order that it might take a place, or lose one, as the case might be, in the armoury of the doctor. He wished to ask whether the cases, of which charts were shown, were having the same amount of milk independent of dilution during the middle period of ordinary feeding as they were during the fore and after periods on citrated undiluted milk. To make it a clean experiment they should have had at least as much milk in the diluted form. Also, when the children were discharged, was there any instruction to go on with the ordinary milk, or was the mother told to keep on with the citrated milk? Dr. Langmead said the excess of proteid matter was not absorbed. He (the speaker) presumed that that statement had been established by careful experiment, because otherwise he would have thought it unlikely, from experimental results, that a large quantity of protein would be passed through unabsorbed over any prolonged period. He had used citrated milk for children in the wards, suffering from severe marasmus, and had been favourably impressed by it. Dr. Langmead referred to œdema as a result of diminished coagulability of the blood, but he (the speaker) thought it might be due to a retention of sodium in the tissues which the kidney could not turn out rapidly enough. A similar œdema was liable to occur from the prolonged use of sodium bicarbonate in diabetes.

Dr. POYNTON congratulated Dr. Langmead on his paper. The idea was due to Professor Wright, but he (Dr. Poynton) took some credit for having dug a note of Professor Wright's out of the *Lancet*[1] and applied it practically. The principle had been a great help in the feeding of children. Speaking generally, citrated milk was valuable. Though American physicians had accused him (Dr. Poynton) of causing in a fortnight or a month acute rickets by his method, he had not himself seen anything of that sort. There was no harm in citrated milk, properly used. He had often noticed that doctors increased the milk, but did not increase the citrate accordingly. That was not a fair way of putting it to the test. Citrated milk did not suit all children: some at once got troublesome diarrhœa, and, on being taken off it, had gone on well enough. The great point raised in Dr. Langmead's paper was, ought undiluted milk to be used? Originally he did not dare to use it, but used slightly diluted milk, and pushed it forward as quickly as possible to undiluted. Knowing how uncertain children were, he did not like to publish broadcast that they should be put on undiluted milk, because if a child went wrong on cows' milk one did not know how much trouble might result. If it went wrong on full milk, it did so badly.

[1] *Lancet*, 1904, ii, p. 433.

It might require weeks to get right again. Therefore, when dealing with small babies he preferred to start with diluted and citrated milk, and increase the strength of the milk and the proportion of citrate of soda. Still, he would give the method of using whole milk a good trial to see how it worked.

Dr. RONALD CARTER, who read a paper on the subject last January, said that for the last two years he had been recommending citrated milk at his infant consultations at North Kensington, and had had great success with it, especially in the case of wasted infants, some of whom had been having the ordinary diluted milk. On being put on citrated milk their weight had gone up remarkably, and all the colic ceased and their general condition improved. Dr. Langmead had been using 2 gr. to the ounce, whereas he (Dr. Carter) used 1 gr. to the ounce.

Dr. LANGMEAD replied that in the children in whom the citrated milk was left off it was not discontinued intentionally, and therefore it was scarcely the same as an exact experiment. Mothers left it off by not bringing the infants. But they had the usual amounts recommended for that period: a child 4 months old had four tablespoons of milk. When he left off the treatment, he did so gradually, and tried to stop it altogether before losing sight of the child. Ordinary milk was used afterwards. He expected to meet with children for whom citrated milk seemed unsuitable, but so far he had not done so. He knew that Dr. Carter had had a large experience in the feeding of infants with undiluted citrated milk, and had written a valuable paper giving his experiences on the matter in North Kensington. In answer to Dr. Miller, he had not noticed any untoward results in connexion with Walker Gordon milk, where that particular milk was used.

Three Cases of Oxycephaly.

By R. Hutchison, M.D.

Case I.—V. J., female, aged 1 year. First child, full time; at birth of child mother was aged 25. Oxycephalic-shaped head (fig. 1); circumference 16½ in., coronal measurement 11½ in. Suture between frontal bones open down to root of nose; coronal and sagittal sutures closed. Eyes wide apart and divergent; slight nystagmus, both rotary and lateral; disks pale, ? atrophic. Hands show webbing between ring and middle fingers on left side, and to less degree on right; phalanges normal. Toes rather irregularly arranged. Some evidence of adenoids. Viscera healthy. Mental condition doubtful.

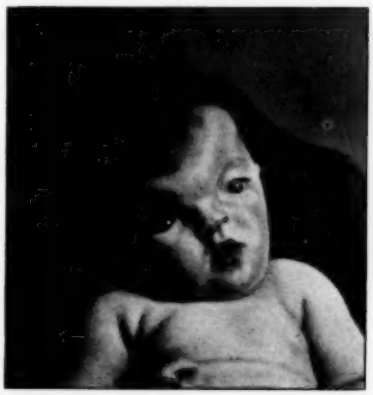


Fig. 1.

Case II.—H. F., female, aged 5 years. Youngest of seven; others normal; two died in infancy. Three miscarriages; one a monster with "head like a cow." Full-time child; had always been healthy. Oxycephalic head with flat occiput and ridge up centre of forehead. Exophthalmos; eyes directed outwards (fig. 2); pupils excentric upwards and inwards. Right responds well to light, left sluggish; disks atrophic; sight defective; adenoid facies; high-arched palate. Hands show short (? bifid) thumbs, only two phalanges in left index; one long and a short terminal. Toes webbed, and great toes very short. Viscera normal. Mental condition seems normal.



Fig. 2.



Fig. 3.

Case III.—S. F., female (not shown). This patient was seen in 1901; she was then aged 4 years. The ninth of ten children. Full time; rather difficult labour; mother had a severe fright at sixth week of pregnancy. Oxycephalic head; circumference 17½ in., coronal 12½ in. Exophthalmos with divergent eyes, left turned slightly down and out (fig. 3). Constant, irregular, chiefly lateral movements. Pupils react sluggishly to light; disks show well-marked atrophy of secondary type, with white lines along course of vessels; vision much impaired. Adenoid facies; high palate; narrow nasopharynx; tubercle of atlas very prominent. Thumbs very broad; little fingers kinked like those of a Mongol. Viscera normal. Mental development backward.

DISCUSSION.

Dr. MORLEY FLETCHER, referring to the mental condition of patients suffering from this condition, said that in the cases which he had seen the mental state had remained good, except in so far as their education had been defective, owing to their impairment of sight. He asked what were Dr. Hutchison's views concerning the production of optic atrophy. Dr. Fletcher himself regarded it as due to some kinking or pressure in the orbit, or possibly to a narrowing of the foramen. In one case in an adult, whom he had shown, the protrusion of the eyeball had been so great, owing to the shallowness of the orbit, that occasionally one of the eyes became dislocated outside the orbit. He thought the defect was due to an abnormality in the basi-sphenoid and the spheno-maxilla. He also drew attention to the fact that several cases showed malformation of the elbow, which prevented full extension; and this he noticed was present in one of Dr. Hutchison's cases. In his (the speaker's) opinion the condition of oxycephaly was due to a developmental defect involving the base of the skull, and was not secondary to any abnormality of the brain, nor was it due to intra-uterine meningitis. The congenital view was supported by the fact that cases had been described as occurring in the same family, and he had met with cases in a mother and her son. He had made efforts to inspect specimens of dried skulls in the museums, but so far without success.

Dr. SUTHERLAND thought the name oxycephaly was rather unfortunate, in that it laid stress on one symptom of an affection with many manifestations, and that symptom might be very slight or absent. There were other symptoms quite as marked and quite as frequent, and a more generally descriptive name was much needed. As regards the pathology, the question arose as to whether the development of the brain or of the skull was at fault. He thought that the latter view was the more likely, and that most of the changes found could be traced to faults in the development of bone, central or peripheral.

Dr. F. PARKES WEBER asked whether Dr. Hutchison had seen minor forms of oxycephaly, or turriform heads in grown-up people, without any ocular signs (neither proptosis nor optic-nerve change). He specially alluded to cases in which there was a striking recession of the forehead above each eyebrow, and a second type in which the middle of the top of the head projected, and was more or less pointed, like a sugar-loaf (real "acrocephaly"), as in the pictures of *Ally Sloper*. He thought these two types were both occasionally met with in otherwise normal adults.

Dr. G. E. SHUTTLEWORTH referred to a remarkable case of oxycephaly shown by Dr. Potts, at a meeting at Birmingham of the Society for the Study of Disease in Children, of which he exhibited a photograph.[1] In this there was asymmetrical distortion of the cranium, which was much prolonged vertically, but there was no proptosis. As regards the mental condition of such cases, which had for the most part been described from the point of view of ophthalmology, there was seldom marked intellectual defect, and Patry had said there was none in the ten cases described by him from personal observation; in the two cases exhibited there were, however, signs of backwardness, and it seemed probable that examination by a psychological expert might disclose mental abnormality in others, which would not be obvious to the ophthalmologist. Oxycephaly included cases of proptosis, but might exist as a cranial condition without this association.

Dr. HUTCHISON, in reply, said he had always supposed—though it was largely guesswork—that the atrophy was due to pressure on the optic nerve where it entered the orbital cavity; and Dr. Keith thought this view was borne out by the specimen now shown—i.e., that it was a mechanical effect. He agreed with what Dr. Sutherland said about the pathology; he did not regard it as a brain condition, but a bone mal-development. He thought the conception of the condition should be widened. He agreed as to the unfortunate name of the condition; another name which had been given was acrocephaly. Both terms were too limited, for he was sure many people with tower-shaped skulls did not belong to that group at all. The photographs handed round by Dr. Shuttleworth were probably cases of that sort. He had seen such skulls produced after prolonged labour, and they were especially apt to occur in face presentations. The skull was drawn out to a point upwards and backwards, and if it was sufficiently marked the deformity might persist. That was "oxycephaly" in the strict sense of the term; but such cases had not the other features, including changes in the digits, which made up the picture of oxycephaly as exhibited in the present cases. The way to regard the condition was that it was an early developmental defect, and not one primarily affecting the brain. The changes in the digits and some of the skull bones were comparable to those in Mongolism, and the optic atrophy and proptosis were mechanical results of the shallow orbital cavity. What it was that caused the arrested development he did not know.

[1] *Rep. Soc. Study Dis. Child.*, 1908, viii, p. 407.

Case of Speech Defect.

By R. HUTCHISON, M.D.

A. W., AGED 7½ years. Has never talked; hears well; mentally bright. Facies rather suggests congenital syphilis, but family history not obtainable (mother dead). No adenoids.

He is very animated and gesticulates a great deal, but can only utter a few quite simple words and repeat a few letters after one. He is certainly not a deaf mute, nor does he seem mentally deficient; nor is the case quite like one of ordinary idioglossia, as he only utters inarticulate sounds bearing no resemblance to speech.

DISCUSSION.

Dr. HUTCHISON raised the question as to whether the speech defect was the result of deafness, or whether the patient belonged to the group of "idioglossia." He did not think the symptoms corresponded exactly to either. The boy was not profoundly deaf. He did not speak in any language at all, as an idioglossia patient does, but made inarticulate sounds.

Dr. SHUTTLEWORTH said the case was very interesting because it was very difficult to determine how far the boy was deaf, and how far he compensated by the activity of his sight for what might be defective hearing. He was a smart boy, and although he said only a few sounds, he made them more intelligibly when he noticed the lips of the speaker. When Dr. Hutchison hid his lips while speaking, the boy's repetition was far less adequate than when he could see the lips. This case recalled one which he had recently seen from Australia, and had been examined by Dr. Still and himself. Both he and Dr. Still thought the boy was not deaf, but was rather mentally defective and he was sent to a private school for mentally defective children. It was difficult in the first instance to test the boy's hearing, as he had so much mental irritability. The parents were sure he could hear well, because they could get him to do anything they wanted. That was true, for there is often a sort of freemasonry between parent and child; but it was afterwards found, when he had settled down at school, that the boy was depending on his sense of sight, and had unconsciously trained himself to lip-reading. But one could make the greatest noise behind him, such as dropping the fire-irons, and he would not turn round to look. He proposed to have the boy examined by an aural surgeon. No doubt it would be necessary to teach the boy in the same way as a deaf mute. When

the eye excelled the ear in grasping the significance of words (as in "word-deafness), it was generally necessary to adopt some modification of teaching by the oral method.

Dr. LANGMEAD asked whether word-deafness had been considered in the case. He did not know of cases in which the child could not speak at all; but it was a condition in which there was no deafness, and the child was reasonable in every way, but had not the capacity for speech, probably on account of a non-development of the auditory speech-centre of the brain. One came across such cases now and again in the routine examination of school children.

Dr. HUTCHISON replied that he had considered the question of congenital aphasia, but he put it aside in this case—perhaps wrongly—because every now and again the boy said a word. He had supposed that there must either be a proper speech-centre in the brain or none at all; in other words, that if any words were used, the condition was not one of congenital aphasia; but perhaps he was mistaken in this view.

Congenital Scoliosis.

By FREDERICK LANGMEAD, M.D.

THE case is that of a girl, aged 10 months, who was brought to the Paddington Green Children's Hospital for "projection of the back." Examination revealed a pronounced spinal curvature with the convexity to the left. This is most severe in the dorsi-lumbar region. In the upper dorsal and lower cervical region is a compensating curve in the opposite direction. There is no other evidence of disease or malformation. Rickets, the usual cause of scoliosis at this age, can be excluded; there is no evidence of rickets, and the curvature is irreducible. If the baby be suspended by the arms the curve does not alter. Spinal caries is not so easily excluded by ordinary examination, but the complete absence of local tenderness and pain, and of constitutional symptoms, render it extremely unlikely, in spite of the fact that a brother is said to have died of "tuberculosis of the spine and abscesses" when aged 1; that the father has phthisis; and a sister, aged 6, tuberculous glands in the neck.

A skiagram reveals the true nature of the scoliosis. Between the last dorsal and first lumbar vertebræ is an extra bone, which is wedge-shaped, the base of the wedge being to the left. The bone bears two transverse processes at its base and one at its apex, and thus apparently

represents a vertebra and a half. That the twelfth dorsal vertebra is not included in this bone is shown by the fact that twelve ribs can be counted on each side, the lowest of which join the vertebra above. There are five lumbar vertebræ plainly visible below.

Remarks.

Cases of scoliosis due to malformation of the spine are apparently very rare. Mr. Duncan Fitzwilliams [6], on whose paper on this subject these remarks are chiefly founded, could only collect thirteen examples of deformity of the spine of a somewhat similar nature. Four of these were in skeletons, and three in fœtal monsters. In five cases only has the condition been described in living children. Mouchet [8] described a case in 1898 of a girl aged 2, in whom the scoliosis was due to the interposition of an extra half-vertebra between the first and second lumbar segments. Codivilla [3] described the case of a girl, aged 11, in whom an extra half-vertebra was seen between the second and third lumbar vertebræ. In Fleury's [7] case the left half of the body of the twelfth dorsal vertebra was suppressed, and six lumbar vertebræ were present. Corner and Harnett [4] published the account of a boy, aged 3, in whom the curve was **C**-shaped, and was due to the vertebræ being broader on one side than on the other. In Fitzwilliam's own case, that of a girl aged 1 year and 4 months, an extra half-vertebra existed between the last dorsal and first lumbar vertebræ, and there were six lumbar vertebræ and an extra rib. It will be seen that the girl shown to-day resembles in many ways the patients already described. In four of the five cases—as in my own—the error of formation has been situated in the lower dorsal or upper lumbar region, as is true also for four of the remaining eight examples in monsters or skeletons. In three cases the nature of the deformity was not ascertained. In only two cases is the deformity of the spine exactly alike (an extra half-vertebra between the first and second lumbar vertebræ)—both described by Mouchet [8, 9]—one in a girl, aged 2, already referred to, and one in an anencephalic monster. My own case resembles most closely that of Fitzwilliams, for in both cases there is an extra one and half vertebræ; but, though fused in my case, they were separate in his.

Fitzwilliams' paper is too recent for any good purpose to be served by giving the details of the cases described, but a brief comparative summary (p. 132) may not be out of place for purpose of future reference.

No. of case and date	Author	Sex and age	Cause of scoliosis	Other abnormalities
1 (1830)	Rokitansky [10]	M., 70 (skeleton)	Right half of ninth dorsal absent, left half bearing two transverse processes	Last lumbar vertebra incorporated with sacrum; only four sacral vertebræ; fusion of vertebral bodies
2 (1850)	Rokitansky [10]	F., 35 (skeleton)	Twelfth dorsal vertebra in two triangular pieces, joined at apices and united to the first lumbar	*Nil*
3 (1850)	Rokitansky [10]	F. (old) (skeleton)	Duplication of one half of three vertebræ and interposition of an extra half-vertebra	Thirteen dorsal ribs, seventh cervical rib
4 (1884)	Bland-Sutton [1]	F. (adult) (skeleton)	Only eleven dorsal vertebræ, but twelve dorsal spines	Malformation of both upper limbs; left hand and carpus malformed; radius and thumb suppressed
5 (1898)	Coville [5]	M., newly-born	Not ascertained	*Nil*
6 (1898)	Mouchet [8]	F., 2	Extra half-vertebra between first and second lumbars	*Nil*
7 (1898)	Mouchet [9]	F., fœtus	Extra half-vertebra between first and second lumbars	Anencephalic monster; double talipes; malformed left hand and mouth
8 (1901)	Bonnaire [2]	Fœtus	Not ascertained	Hydrocephalus
9 (1901)	Bonnaire [2]	Fœtus	Not ascertained	Omphalocele and malformed pelvis and lower limbs
10 (1901)	Codivilla [3]	F., 11	Extra half-vertebra between second and third lumbars	Median tuft of hair, but no spina bifida
11 (1901)	Fleury [7]	?	Suppression of left half of the body of twelfth dorsal	Six lumbar vertebræ
12 (1905)	Corner and Harnett [4]	M., 3	C-shaped curve, due to wedge-shaped vertebræ	*Nil*
13 (1908)	Fitzwilliams [6]	F., 16 months	Extra half-vertebra between last dorsal and first lumbar vertebræ	Extra rib, six lumbar vertebræ
14 (1910)	Langmead	F., 10 months	Wedge-shaped bone, consisting of extra one and a half vertebræ, between twelfth dorsal and first lumbar	*Nil*

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[1] Bland-Sutton. *Med.-Chir. Soc. Trans.*, Lond., 1884, lxvii, p. 157.
[2] Bonnaire. *Bull. Soc. Obstét. de Paris*, 1901, iv, p. 161.
[3] Codivilla. *Archiv. de Ortep.*, Milano, 1901, p. 65.
[4] Corner and Harnett. *St. Thomas's Hosp. Rep.* (1904), 1905, xxxiii, p. 390.
[5] Coville. *Rev. d'Orthop.*, Par., 1898, 1e sér., ix, p. 301.
[6] Fitzwilliams. *Proc. Roy. Soc. Med.*, 1909, ii (Child. Sect.), p. 25.
[7] Fleury. Thèse Doct. Paris, L. Boyer, 1901.
[8] Mouchet. *Gaz. hebd. de Méd. et Chir.*, Par., 1898, N.S., iii, p. 469.
[9] *Idem. Bull. Soc. Anat. de Paris*, 1899, lxxiv, p. 972.
[10] Rokitansky. "Pathological Anatomy," 1850, iii, p. 230.

Blue Patches on the Buttocks and Lumbar Region.

By Frederick Langmead, M.D.

The baby, aged 12 weeks, shows roughly symmetrical blue patches on the buttocks and lumbar region. These are similar to the racial pigmentation seen in Japanese babies and those of half-castes. The aspect and hair are suggestive of a Japanese ancestry. The mother is English. The father was born in London, of an English mother, but no more of his father's nationality is known than that he was a "foreigner."

DISCUSSION.

Dr. G. Pernet said that such areas of pigmentation in Japanese babies were so well known that it was not necessary to say much about them. According to Ashmead [1] the spots are more or less recognizable in the majority of Euro-Japanese half-breeds. The spots are temporary, and disappear two or three years after birth.

Dr. G. A. Sutherland said he thought the important point was whether this pigmentation of a Mongolian type had occurred in a pure English child or not. At that Section there had been shown a Japanese baby with the typical spots; and Mr. Arthur Edmunds had shown a pure-bred English baby with similar spots. A few cases of the kind had been described among English babies, and some in German children. The point to clear up was whether the present child was pure English-bred or not. It appeared not to be so, as the Japanese characteristics were so marked. He believed it was simply the racial characters of the grandfather coming out in the child, and that the unknown "foreigner" had been a Japanese.

[1] *Journ. of Cut. Dis.*, New York, 1905, xxiii, p. 207.

Dr. MORLEY FLETCHER asked Dr. Langmead, with regard to the blue patches occurring in Mongolians, how many weeks the patches persisted.

Dr. LANGMEAD replied that he did not know the usual duration of the pigmentation. It looked like a Japanese child, but there was some doubt about the ancestry.

Juvenile General Paralysis, with loss of Knee-jerks.

By R. MILLER, M.D.

BOY, aged 14 years. During the last eighteen months he has had several fits, after one of which he temporarily lost the use of his right side. He has always been backward. The family history shows several miscarriages, while eight children have died, many of them in infancy. The boy is fat and of infantile appearance. He is timid, irritable, and shows occasional outbursts of passion. Cerebration is delayed; there is tremor of the tongue and hands; voluntary action of the hands is clumsy so that he cannot feed himself easily; there is occasional incontinence: the pupils are large and react very slightly to light; it is impossible to examine the eye-grounds; his gait is unsteady; the arm-jerks are increased, the knee-jerks are lost, and the abdominal and plantar reflexes are normal; there are no signs of inherited syphilis, but the serum-reaction (Fleming) is positive.

Under treatment by mercury, cerebration has become quicker and the voluntary action of the hands less clumsy. He now feeds himself easily. Otherwise there has been no change. The serum-test remains positive.

DISCUSSION.

Dr. MILLER added that the speech defect was of the type found in an early case of general paralysis of the adult. The boy had had a temporary hemiplegia, which had cleared up completely, while the rest of the child's symptoms seemed to point conclusively to the condition known as juvenile general paralysis.

Dr. POYNTON said such cases were very difficult ones. He had shown before the Clinical Section[1] what he believed to be two cases of the condition. They showed a characteristic symptom, sitting by themselves, talking to themselves, and living in an imaginary world, with remarkable delusions.

[1] *Proc. Roy. Soc. Med.*, 1909, ii (Clin. Sect.), p. 11.

The girl was 11 years age, and she died about a year after he saw her. She was completely paralysed before death. The other case which he showed was a more difficult one. This was a boy with Argyll-Robertson pupils who had outbreaks of cerebral nature, and got very fat. It was suggested at the meeting at which he showed him that there might be a tumour in the pons. He had treated the case with mercury up to the present time—viz., for one-and-a-half years. But there were still the irregular pupils and occasional breakdowns mentally, though, on the whole, there was improvement. He could not make up his mind definitely about the case now any more than he could two years ago, as to whether it was juvenile paralysis or syphilitic brain disease.

Lymphadenoma starting at the Twenty-fifth Month.

By R. MILLER, M.D.

BOY, aged 3 years. Swelling in the right side of the neck was first noticed when the child was twenty-five months of age. Six months ago a gland was removed from the neck for diagnosis. Microscopically it showed "small collections of swollen endothelial and coarse eosinophilous cells, a few plasma cells, and an occasional typical giant cell; no tubercle bacilli were found." Dr. Spilsbury regarded the gland as certainly lymphadenomatous. During the last six months the glands have steadily increased in size, but remain discrete and elastic. The spleen has become palpable, and is now enlarged to 2 in. below the costal margin; otherwise the child is in apparently good health. The white cells in the blood number 6,800, and show a slight excess of lymphocytes.

DISCUSSION.

Dr. R. HUTCHISON said the case was very interesting because the condition had commenced so early. At one time he took a great deal of interest in lymphadenoma in children, and collected a fair number of cases, but he could not recall one which commenced so young as this. He thought there was no doubt about the diagnosis; it ran the typical course; beginning in the neck, and then extending to the spleen. The microscopical section and the blood count were compatible with such a condition. There was not much to be said about treatment. There did not seem to be much to gain by cutting the glands out, though he had often seen it done. Invariably the condition had recurred after operation, either in the same spot or elsewhere. If the spleen was also enlarged, there was nothing to be gained by operation. He had known the glands

subside under arsenic for a time, especially where they were soft, as they were in the present case. X-rays in conjunction might also do good. Such cases usually progressed, and often became secondarily invaded by tubercle.

Dr. POYNTON said there were cases of congenital syphilis which were so closely allied to lymphadenoma that it was not until an autopsy was made that one could distinguish them. That, however, was not likely in the present case. He remembered one case which had an enlarged spleen and glands, and was generally regarded as lymphadenoma; but the post-mortem showed it was multiple visceral syphilis, with gummata in the spleen. It might be well to try mercurials even in the present case before giving it up as hopeless.

DR. MILLER replied that the treatment had consisted of small doses of arsenic continuously for the last six months, except for one or two weeks; but there had been no improvement at all, not even temporary, but the glands had steadily increased during the whole time. The general condition of the patient was remarkably good, but not so good as it was six months ago. The type of disease seemed to be exactly like that seen in older patients; it did not seem to be modified, in this case, by the youthfulness of the patient. He was glad of the suggestion about trying mercury, but Dr. Spilsbury was certain that the gland could be from no other disease than lymphadenoma.

Congenital Acholuric Jaundice.

By L. G. PARSONS, M.D.

THE patient, A. J., a boy aged 8, has been yellow since birth. Sclerotics first noticed yellow at 3 years of age. Colour of skin has varied in degree slightly at different times. When first seen (November, 1909) slight lemon tinting of skin was noticeable, but sclerotics were markedly yellow. December, 1909, vomiting, and pain in region of spleen and jaundice deepened. Second of three children. No other case of jaundice in family or family of either parent. His mother had jaundice (?) after birth of third child. No specific history.

Admitted into Hospital for Sick Children, under Dr. Batten, on December 16, 1909. He was pale, drowsy, and irritable. Skin marked lemon yellow, sclerotics of deeper tint. Spleen uniformly enlarged, anterior border reaching to 1 in. above and to left of umbilicus, firm, slightly tender. Urine sherry-coloured, contained no bile-pigment, colour due to urobilin. Stools were dark-green brown. Temperature 100° F. Blood count showed marked anæmia. Hæmoglobin, 28 per

cent. Red blood corpuscles, 1,212,000 per c.mm.; marked variation in size, many microcytes, a few poikilocytes and nucleated cells; leucocytes, 3,250.

January 12: Blood count, hæmoglobin 55 per cent.; red blood corpuscles, 3,518,000, now show only slight variation in size; no poikilocytes; leucocytes, 9,500. Fragility of corpuscles tested for and found to be increased. No specific agglutinin found while in hospital; stools were never acholic, nor did urine contain bile-pigment. There was mild pyrexia of an irregular type. The spleen did not alter in size, but pain on palpation disappeared. The anæmia improved; the skin became less yellow.

Since his discharge from hospital, the colour of skin has varied in degree slightly. At the beginning of April his blood showed increased fragility of corpuscles to about same degree as previously. His elder brother has a spleen that is just palpable; his blood shows no definite increase in fragility of corpuscles.

DISCUSSION.

Dr. POYNTON said that he thought the observation by Dudgeon and Hawkins as to the presence of a specific agglutinin was an exceptional one. It was not present in three cases he had himself shown at the Medical Society,[1] according to the observations of Dr. Embleton.

Dr. F. PARKES WEBER said that one of the remarkable points in the present case was that the boy soon after admission looked very ill. He (Dr. Weber) saw the boy not long after admission to the hospital under Dr. F. E. Batten. The child about that time had considerable fever and appeared to be so seriously affected that one then felt inclined to doubt whether his condition could be explained as merely due to an exacerbation of the disease such as patients with chronic acholuric jaundice were liable to.

Dr. PARSONS replied that the child was very ill when admitted to the hospital, but it had been pointed out by Dr. Poynton that such cases showed exacerbations, in which there was marked anæmia and irregular pyrexia, and a deepening of the jaundice. He had never had such a serious attack since the one for which he was admitted. He was discovered in the routine examination of school children. He had attacks of jaundice for varying periods, and the parents said the colour of the skin had been present since birth.

[1] *Med. Soc. Trans.*, 1909, xxxii, p. 381.

Congenital Syphilis.

By L. G. Parsons, M.D.

J. Y., female, aged 10, was well till aged 3, then eye symptoms, which lasted two years. Twelve months ago pain and swelling of abdomen ; no vomiting. For last nine months joints have been swollen at times and extremely painful ; painful lump on forehead six months. Third of four children ; mother had one miscarriage between third and fourth children. Admitted to Hospital for Sick Children, under Dr. Batten, on January 21, 1910. Pale child ; no Hutchinson's teeth ; old interstitial keratitis both eyes. Liver enlarged, hard edge, left lobe considerably enlarged and nodular. Spleen reaches to the level of umbilicus and anteriorly almost to middle line. Just below costal margin is a rounded lump on its surface, probably gumma. There is definite periostitis of right radius, ulna and humerus, and over right frontal eminence. No change in fundi oculorum. No nervous phenomena. Treatment has relieved the bone pains, but has made no difference in the character of the splenic enlargement.

Section for the Study of Disease in Children.

May 27, 1910.

Dr. J. W. Carr in the Chair.

Malignant Endocarditis of the Tricuspid Valve in a Child aged 6 years.

By Francis H. Hawkins, M.D.

E. W., aged 6 years, was admitted under my care on March 15, 1910, complaining of abdominal pain, and during the last few days of continuously crying out "Oh, my belly!" The abdominal pain came apparently quite suddenly on March 11. There had not been any vomiting. No blood had been passed by the rectum. The bowels had not been moved since the onset of the illness.

Previous history: The child had not suffered from any recent illness. There was no history of scarlet fever or chorea. At the same time of complaining of abdominal pains the child also complained of pains in the legs.

Condition on admission; Temperature, 104·2° F.; pulse regular, but weak and rapid. The child was lying on her back, the abdomen was much distended, movements with respiration very slight, and the walls were rigid; and whenever touched she cried out "Oh, my belly!" The percussion note was resonant. Nothing abnormal could be felt on rectal examination. On examining the chest no abnormal physical signs were detected. There was no albumin in the urine.

The child had been sent into the hospital as a case of peritonitis, and I asked my surgical colleague to see the patient, and it was ultimately decided to operate; but when the abdomen was opened, beyond noting some meteorism and one or two small, fæcal masses, nothing abnormal was observed. On the following day (March 16) the child still complained of constant abdominal pain, and she cried out very frequently; the legs were now somewhat rigid and no knee reflex could be obtained. On March 17 considerable dyspnœa was present, and on percussion over the thorax the note over both bases was impaired and an area of tubular breathing was noted over the left base. The abdominal pain was still complained of, and on the following day (March 18) the child's face was of a dusky colour. Dyspnœa was much increased, and there was evidence of broncho-pneumonia of both lungs. Later in the day the child died. That was three days after admission. I would just mention that no cardiac murmur was detected at any time.

Autopsy: On opening the abdomen the stomach was found to be distended. The mesenteric glands were enlarged. There was no peritonitis, no collapse of bowel, and no occlusion of the mesenteric vessels noticed. On section of the kidneys a few white infarcts were seen in the cortex; these organs appeared otherwise normal. The liver on section showed a few white infarcts close to the surface. No infarcts were seen in the spleen. Thorax: In the left pleural cavity a small amount of semi-purulent exudation was found. The left lung was somewhat adherent (recent) to the chest-wall and the diaphragm; there was some recent lymph over the surface. The right lung was slightly adherent at the base. On examining the lungs a few white nodules were seen on the surface, and when a section was made these nodules —white infarcts—were seen to be quite at the periphery of the lung substance. Both lungs were congested and had scattered over the surface dark, triangular patches, giving much the appearance of red infarcts. There was one large bronchial gland showing evidence of an old caseous focus in the centre. Heart: The mitral and aortic valves were normal. On the auricular surface of the middle cusp of the tricuspid valve a whitish-grey, rounded nodule about $\frac{1}{8}$ in. in diameter was seen.

Report from the Clinical Research Association.—Nodule on heart-valve was found to be inflammatory in origin and crowded with Gram-staining micrococci. The lesion was very suggestive of an ulcerative endocarditis. The lungs showed areas of broncho-pneumonia surrounded by marked congestion and much leucocytic infiltration. These were

found to be crowded with cocci like those in the heart-valve; they have the appearance of pyæmic foci. The gland (bronchial) showed an old caseous focus containing tubercle bacilli. There was no evidence of tubercle bacilli in the lung.

Remarks.

I bring this case before you as one of malignant or ulcerative endocarditis, as I think the lesions present, as also the report of the Clinical Research Association, justify me in so doing. This form of endocarditis is by all writers admitted to be rare in children. Holt states that malignant or ulcerative endocarditis is a rare disease in children, and he refers to a case reported by Harris which occurred in a boy four years of age and affecting the right side of the heart; it was secondary to a cardiac malformation. Holt states that of the cases thus far reported in early life, about twenty-five in number, the great proportion have been in children over ten years of age.[1] Osler,[2] in his Gulstonian Lectures, states that young children are rarely the victims; there were only three or four instances under ten years of age.

The secondary affections in the case now recorded are of interest: the secondary affection of the lungs which is, I believe, usual when the right side of the heart is affected, and then the presence of white infarcts in the liver and kidneys.

Since the death of this child, and so after the notes had been taken (and I am indebted to Mr. Kebbell, the house-physician, for the notes), I find that the child complained of pains in the legs, but, so far as one can ascertain, this was at the same time as the abdominal pains, but I refer to it in relation to the ætiology. Malignant endocarditis in children seldom occurs as a primary affection; Holt states that it is most frequently secondary to pneumonia, but in my case broncho-pneumonia occurred late in the disease. Next to this he places rheumatism and meningitis, and it is in reference to the question of rheumatism that I allude to the fact of the child having complained of pains in the legs; the joints were not swollen, neither was pain complained of when they were touched, but we know that in children indifferent pains are frequently the precursor of endocarditis. As regards meningitis, clinically there was no evidence of this, and as we were not allowed to open the head, the brain, &c., could not be examined.

[1] Holt (L. E.), "Diseases of Infancy and Childhood," 1903, 2nd ed., p. 622.

[2] *Lancet*, 1885, i, p. 505.

DISCUSSION.

Dr. BELLINGHAM SMITH asked whether there was any embolism of the mesenteric artery which would account for the abdominal pain and meteorism at the same time.

Dr. CAUTLEY said he thought it was infective endocarditis; he did not like the term "ulcerative," because there was not necessarily ulceration. Should the term "malignant endocarditis" be reserved for cases in which there were secondary infarcts, or cases which proved fatal, or be used to differentiate them from those due to rheumatism? In acute rheumatism there might be endocarditis ending fatally, with the formation of infarcts throughout the body, although the infarcts did not proceed to suppuration. Recently he had a case of a child, aged 9, who was admitted with mild rheumatism from the point of view of the joint trouble. But she had myocarditis, endocarditis, and some pericarditis. In a week or two she had chorea and rheumatic nodules, and rapidly died. No infarcts were found, but there was acute endocarditis of practically all the valves of the heart. Infarcts occurred in infective cases in the arterial as well as in the pulmonary system, implying that the infective organism was somewhat virulent, interfering with the pulmonary nutrition, and passed through the lung. In such cases the temperature might be fairly high, higher than in rheumatic endocarditis, and pyæmic in type.

The CHAIRMAN (Dr. Carr) said he thought the case must be accepted as one of infective or malignant endocarditis. Such cases were rare in children. He had performed autopsies on two cases, and in each instance the disease was fatal from cerebral hæmorrhage; there was an aneurysm, which had ruptured. In both cases the children were older than was Dr. Hawkins's patient. Infective endocarditis was relatively more common on the right side of the heart than was the simple variety. He did not know any satisfactory explanation of that fact. He did not think the cause was rheumatism, as it was not likely that rheumatic endocarditis, even if it affected the right side of the heart, would not have affected the valves on the left side also. He asked whether Dr. Hawkins knew what kind of surroundings the child came from and whether there was any sepsis in the family.

Dr. HAWKINS, in reply, said there was no evidence of embolism in a mesenteric vessel, and such an event was somewhat rare; he had seen only three cases in thirteen years. He called it malignant endocarditis because there was no evidence of ulceration, and he did not think that the term "infective" applied exactly to this type, as he was inclined to regard the endocarditis occurring in rheumatic fever as infective. He did not know what kind of house the child lived in. The temperature of this child was high on admission.

Ateleiosis in a Man aged 42; Physical Development said to have been Arrested at about the age of 9 years.

By F. Parkes Weber, M.D.

The patient, F. B. H., aged 42, unmarried, first came under observation on account of slight traumatic synovitis of the left knee. He is an infantile dwarf whose physical development roughly corresponds to that

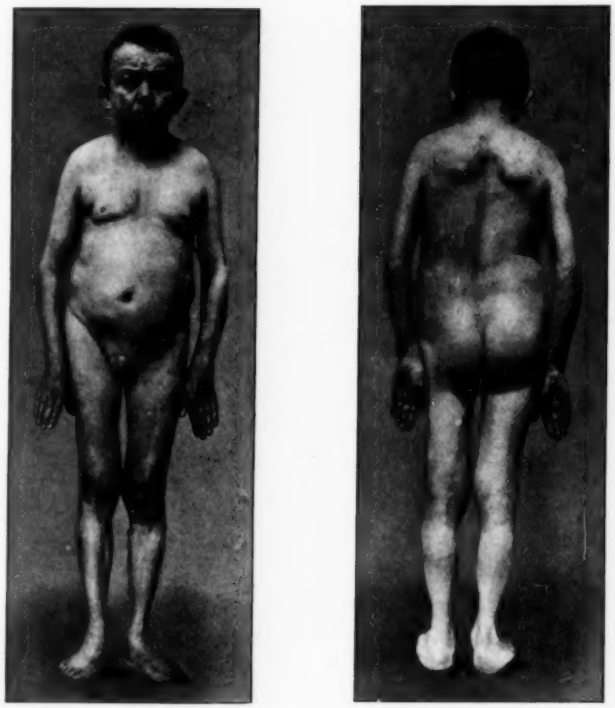


Fig. 1.

Ateleiosis in a man aged 42.

of a boy of nine or ten years; but his expression, the wrinkles on his face, his attitude, his manner of speaking, and his general behaviour are rather more those of an adult. According to the family bible, he

was born in London, April 10, 1868, and he has heard his parents say that he was much like other children up to the age of nine years, but that his growth and development then ceased. He is said to have had "water on the brain" as a baby, and, as a child, to have had two or three falls on the head. He cannot himself remember having had any serious illness. There is no history of chronic diarrhœa or intestinal steatorrhœa. At present he measures 47·7 in. in height, and weighs 4 st. 13 lb. in his clothes. His head is rather large for the diminutive size of his



FIG. 2.

Skiagram of the knees.

body. The shape of his trunk, the undeveloped state of his sexual organs, the appearance of his neck (owing to the want of projection of the "pomum Adami"), and the high pitch of his voice are those of a child. There is no hair on his face or pubes. The penis is exceedingly small. No testicle can be felt on the right side; the left testicle, of about the size of a small cherry, is incompletely descended. Nothing abnormal can be discovered in the heart, lungs or blood-vessels, or in the

abdomen (by palpation) or urine. There is very slight spinal scoliosis. Mentally he seems normal, though perhaps somewhat childish. He cannot read or write, but, apparently "on account of weakness," the doctor would not let him go to school as a child. He earns a little money by light work in a glass factory. Skiagrams (Dr. Finzi) of the extremities show persistence of some of the epiphysial cartilages, but

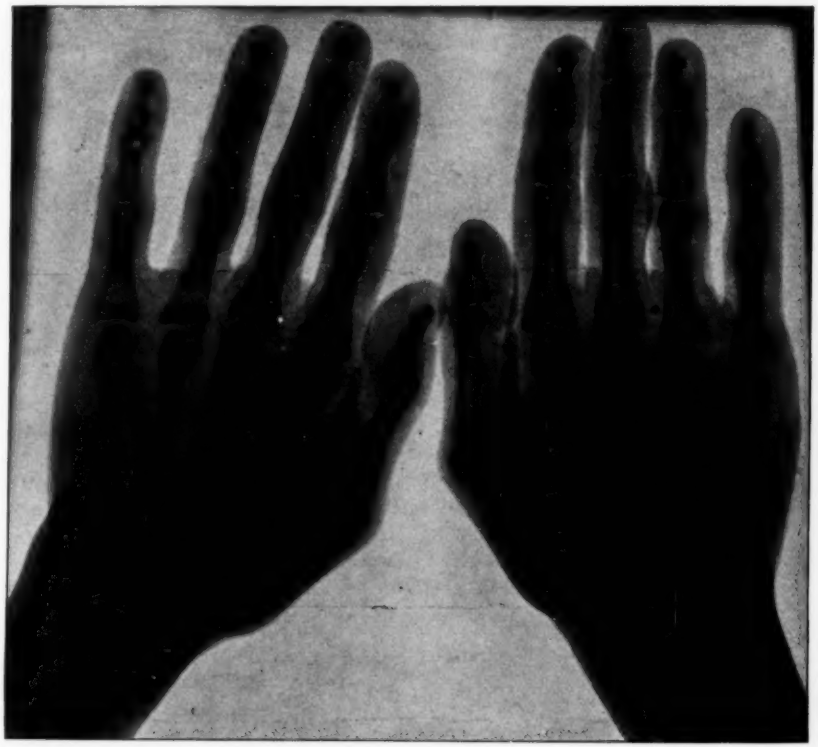


FIG. 3.
Skiagram of the hands.

the amount of union of the epiphyses with the diaphyses varies considerably in different bones. Dr. Finzi points out that, in the skiagrams of the hands, those epiphyses which are not yet joined to the diaphyses are seen to be bordered by a very deep shadow. Skiagrams of the skull show nothing special. The delicate "pink and white" colour of the

patient's face, the extreme fineness of the hairs of his scalp, and the small size of his thyroid gland suggest that there may be an element of myxœdema combined with the ateleiosis, but these features may be merely a part of the general infantilism. His father and mother were both born in 1832, and lived to about the ages of 56 and 50 respectively. There is no history of any other dwarfism or infantilism in the family. "Ateleiosis" (the Greek word "ateleia" means incompleteness) was introduced by Hastings Gilford[1] as a convenient term for conditions of the kind, on account of the incompleteness of development characteristic of all cases. Apparently the epiphysial cartilages may in ateleiosis persist to a considerably more advanced age than in the present patient.[2] Some French authors would probably suggest a theoretical condition of "polyglandular insufficiency" as an explanation for cases of ateleiosis.

DISCUSSION.

Dr. SHUTTLEWORTH said the condition was evidently a rare one, and the physical features did not suggest to him any cretinism; the ateleiosis was the prominent character. Until cases of sporadic cretinism were investigated thoroughly, as had been done in recent years, most cases of arrested physical development were probably lumped together with cases of cretinism; the differences between infantilism and cretinism not being then known. Very little had appeared in English medical literature on this condition, but he heard Mr. Hastings Gilford's paper on it in 1902, that authority pointing out that cases of dwarfism like the present one presented certain analogies to gigantism. The man now shown, aged 42, seemed to exhibit as much intelligence as could be expected considering the fact that during childhood he never went to school. He had some business capacity, as he earned eighteen shillings a week washing and counting bottles.

The CHAIRMAN asked what Dr. Weber suggested was the probable causation of the curious arrest of development in certain directions.

Dr. F. PARKES WEBER replied that he was glad Dr. Shuttleworth confirmed the view that there was no cretinism in the case. He could not give any explanation as to the cause of the arrest of development in ateleiosis.

[1] *Med.-Chir. Trans.*, Lond., 1902, lxxxv, p. 305.

[2] Thus, when the male dwarf whose case is described by Schaaffhausen (quoted by Hastings Gilford, loc. cit., p. 325) died at the age of 61, nearly all the epiphyses were still ununited or could be easily separated. The same characteristic feature was present in the skeleton of Paltauf's case, a male dwarf who died of tuberculosis at the age of 49.

Case of (?) Myositis Fibrosa.

By J. W. Carr, M.D.

Patient is a girl, aged 4½ years; there are three other children in the family, all healthy. No reliable history is obtainable. The child is stated to have begun to walk when sixteen months old; six months later her legs began to get weak, until she became unable to walk or stand. Two years ago it is said that her arms became weak. At present the child cannot fully extend either the hips or the knees, especially on the left side; there is no limitation of flexion, but abduction of the hips is much restricted. The left arm cannot be completely supinated. There is no pain on movement, and no increased mobility under anæsthesia. There is no evidence of disease of the brain or spinal cord. There is no rigidity of the muscles of the affected limbs; they are slightly wasted, probably merely from partial disuse, as they all react normally to faradaism. There is no ankle-clonus; the knee-jerks can just be obtained. Sensation is not affected; the mental condition is normal.

Skiagrams, taken under an anæsthetic, show no definite evidence of bone or joint disease. There is no ossification of the muscles. The spinal column appears to be healthy.

Is the fixation of the joints due to the gradual development of a fibrous myositis, or is it a result of some primary lesion in the joints themselves?

DISCUSSION.

Dr. Carr added that some who had examined the child thought there was some peri-articular thickening. There was slight wasting of the muscles, which he regarded as due merely to disuse. There was no evidence of the cartilages or bones being affected. It was not a primary muscular dystrophy, and there was no indication of myositis ossificans. He did not think the amount of contraction of the muscles was sufficient to account for the fixation of the joints. Under anæsthesia the fixation remained unaltered. A piece of muscle had been removed from the pronator radii teres, and microscopical examination did not show anything abnormal. The question was whether it was primarily a muscle lesion or a joint lesion. For some weeks he had given iodolysin by the mouth up to ℥20, three times a day, and the lower part of the body had been put into a Tallerman bath, up to 150° F., for fifteen minutes at a time daily, and massage and passive movements had been employed afterwards. Some improvement was noticed in the joints on the right side, but not on the left.

Dr. E. CAUTLEY said the absence of any evidence from microscopical examination of a muscle was against it being myositis fibrosa. He thought it analogous to the contracture of limbs which one saw in prolonged toxæmic conditions in patients who were confined to bed. He had seen this in typhoid fever of nine months' duration, the limbs being rigid and fixed in a similar manner. If it were not for the absence of a history of such toxæmia in this case he would have thought that was the explanation.

Mr. LOCKHART MUMMERY agreed with Dr. Cautley that it was not myositis fibrosa; the muscles felt normal. There seemed to be an absence of a satisfactory history. It seemed certain that the child must have suffered from an infective condition. He thought it a case of adhesions round the joints; there was no adhesion inside the joints, and the movements were smooth. The contraction of muscles he regarded as secondary to the limited movements of the joints, and the case coincided with those seen in children who had had improper treatment during a prolonged illness. He recommended that extensions, by means of weights, should be put on both limbs for as long as five or six months; the weights being as much as the child could bear without pain and without the strapping cutting into the skin. The muscles and the fascial planes were shortened. He was not sure that the hips could be got straight. He had tried iodolysin in several cases, and he had not been well pleased with it; there was some improvement for a week or two, and then the improvement stopped; one or two patients seemed to have got ill as the result of the use of fibrolysin and iodolysin.

Dr. CARR, in reply, said the opinions which had been expressed corresponded very nearly with his own. Some of his hospital colleagues favoured the diagnosis of myositis. He did not expect to get further benefit from iodolysin in the case, and he proposed to carry out Mr. Mummery's suggestion as to weight extension. The child's present condition was miserable, and if it remained when she grew up it would be deplorable.

A Case of Amaurotic Family Idiocy.

By E. BELLINGHAM SMITH, M.D.

S. G., FEMALE, aged 11 months, was brought to the Queen's Hospital for blindness, inability to sit up or hold her head up. Patient is the third child of Jewish parents; the mother has had no miscarriages. There are two elder children, both of whom are alive and well. Infant has been always breast-fed. The symptoms complained of have only been noticed for one month previous to date of admission (April 27, 1910).

On examination the child is exceedingly fat and well nourished. The general condition and aspect, however, is one of extreme apathy; no

movements of head or body are attempted, and those of limbs are limited in character. The child is quite unable to sit up without assistance, but if the back be supported she is capable of holding up her head unaided; the lolling movements of early infancy are, however, occasionally present.

A further examination of the limbs reveals a variable degree of rigidity, which is, however, always greater than the normal muscular hypertonicity of an infant. This spasticity is more marked in the legs than arms. The eyes exhibit random, aimless movements, but no true nystagmus is present—no notice is taken of surrounding objects, but there is a definite appreciation of a strong light. An ophthalmoscopic examination reveals changes characteristic of amaurotic family idiocy. These changes are bilateral, and consist of marked pallor of optic disks, amounting to optic atrophy, while in the yellow-spot region there is a greyish-white area about $1\frac{1}{4}$ times the size of the optic disk, surrounding a bright pink spot. The shape of the patch is roughly oval, and the long axis is horizontal.

A Case of Congenital Malformation of the Heart.

By T. R. Whipham, M.D.

The patient is a male child, aged $1\frac{3}{4}$ years. Cyanosis was first observed when he was a fortnight old and has persisted since. It is very marked on the face, especially on the lips, nose, and ears. The extremities are deeply congested, and all the digits are clubbed. The heart is enlarged in all directions. At times there is no murmur and only an impurity of the first sound at the base can be detected, but at others a slight soft systolic bruit is audible to the left of the sternum, while the second sound is a little sharp. The child also presents congenital ptosis of both upper eyelids. The red blood corpuscles numbered 5,514,000 per c.mm. in March, but a recent count shows 9,440,000 and a hæmoglobin value of 170 per cent. Ophthalmoscopic examination shows that the vessels are abnormally large, but there is a distinct difference in the appearances of the veins and arteries. The lesion present is probably some abnormality in the origin of the great vessels from the heart, with deficiencies in the septa dividing the right and left chambers of that organ.

A Case of Congenital Pulmonary Stenosis in a Girl aged 18.

By T. R. WHIPHAM, M.D.

THIS case is shown to compare with the last. The patient, a girl aged 18, has always been cyanosed and has suffered from shortness of breath, but from no other cardiac symptoms. She presents a typical blue appearance and clubbing of the finger-ends. The heart is enlarged, and over the pulmonary area there is a distinct thrill and a typical rough systolic murmur indicative of pulmonary stenosis. Menstruation has occurred once, slightly, about four months ago. The age of the patient is somewhat unusual.

DISCUSSION.

The CHAIRMAN said most cases of congenital heart disease did not live much beyond puberty, but it was a matter of the degree of pulmonary stenosis, which, in this instance, was slight. Satisfactory compensation might be attained and the patient live to a fair age. A man presented himself to him one day and said he would be happy to be of service in connexion with clinical teaching, as he had an interesting heart condition. On examination he was found to have definite pulmonary stenosis, though probably less serious than that of the present girl. The man's age was about 25. He agreed with Dr. Whipham that in the first case there was some extensive abnormality of the heart, though what it was one could not say. The second blood-count was more in accord with what would be expected with such a high degree of cyanosis.

Dr. FRANCIS HAWKINS said the second of the cases was interesting owing to the age of the patient. One such case he exhibited before the Clinical Society; the patient's age was 19, and she was kept in sight until 22 years of age. She was 4 ft. 4 in. high, and did not look more than 12 years of age. At the autopsy the cardiac apex was bifid; the ductus arteriosus was patent, and there was marked pulmonary stenosis; the valves were funnel-shaped with an opening admitting a No. 10 catheter. He also referred to a second case of a boy aged 12, who died when 14 years of age. The heart was on the right side, the other viscera being normal in position; a loud systolic murmur was heard at the level of the second right rib, louder on the right side; pulmonary stenosis was diagnosed. At the autopsy the foramen ovale was patent, and the ventricular septum imperfect. The pulmonary artery came from the left ventricle; the opening was small, a mere slit with no valves. The aorta also came from the left ventricle; the opening was small, and in place of valves

were nodular-like projections. He supposed such cases, with a loud systolic murmur over the pulmonary area, might go on until fairly late in life. Some years ago he saw an officer who had been passed into the Army by a joint certificate of Sir William Gull and Sir William Jenner. He had a pulmonary murmur, and was markedly cyanosed on exertion; yet he had been in the Egyptian war, the Tirah campaign, and South Africa.

Dr. PARKES WEBER thought that in the infant with congenital heart disease and cyanosis a condition of absolute closure of the pulmonary orifice was possibly present, in spite of the fact that a soft systolic bruit had been heard over the pulmonary area.

Dr. E. CAUTLEY asked what Dr. Whipham thought the prognosis was in the case first shown. The diagnosis was probably as nearly correct as could be made—viz., that there was a large patent septum ventriculorum, and that the pulmonary artery was transposed, or that the pulmonary artery and aorta arose from the same ventricle. He did not agree with Dr. Weber's suggestion that there was complete atresia. In his own experience atresia or almost complete atresia of the conus pulmonalis could exist with very little cyanosis.

Dr. WHIPHAM replied that the bruit in the first case varied; he could hear one that day. Its greatest intensity was not over the pulmonary area, but over the third interspace to the left of the sternum. At other times the murmur was either absent or was not more than an impure first sound. He thought the second blood-count was probably correct, though he had no reason to doubt the first; both were made by skilled pathologists. He regarded the prognosis as very bad.

[A further blood-count in the first case, made on June 2, showed 11,740,000 red corpuscles per c.mm., and 180 per cent. of hæmoglobin.]

A Case of Air-swallowing.

By T. R. WHIPHAM, M.D.

THE patient, a boy aged 12, has the peculiar habit of air-swallowing and forced eructations, which are almost continuous. He is said to vomit frequently, but this is probably the result of his abnormal habit. The tongue is clean and the abdomen presents no abnormality, being flat and not unduly tympanitic. It is alleged that, two years ago, he suffered from the same condition, and that he recovered after three weeks' treatment. The present symptoms began suddenly, without any apparent cause, about seven weeks ago. The eructations are persistent, except when the boy is asleep. There is no history of chorea, but the patient is said to have been of a nervous disposition since early childhood.

Dr. F. PARKES WEBER said there was a means of quickly stopping the aërophagy—namely, getting the patient into a hospital and inserting a rubber gag between the jaws, which kept the mouth open. This could be applied for a time, and if the patient was anxious to get rid of his disagreeable belching he could himself apply the instrument at home when he felt an attack coming on. Air could not be drawn into the stomach if the mouth were kept widely open, and, consequently, the belching likewise ceased. The associated abdominal discomfort could be treated by dietetic methods.

Pathological Report on Case of Myelogenous Leukæmia in an Infant aged 18 months.[1]

By T. R. WHIPHAM, M.D.

THE patient died on March 4. The following is the pathological report on the case by Mr. A. N. Leathem, who performed the necropsy.

Post-mortem examination, made about seven hours after death: The body showed great emaciation, weighing only 11 lb. 6 oz. There were no signs of decomposition. Rigor mortis and hypostasis were both absent. The skin was of a brownish tint, and there was an entire absence of subcutaneous fat. About the head and neck were several small ulcers, extending into the subcutaneous tissue, and also subcutaneous hæmorrhages. The blood was thin and watery, and did not coagulate. The brain, meninges, sinuses, and cranial bones appeared normal. There was a little muco-pus in each middle ear. The pericardium was normal. The heart-walls were pale and flabby; the valves were normal, and there was a small quantity of fluid blood in each side of the heart (the pale clots frequently described in cases of leukæmia were not present). The cervical glands were moderately enlarged on both sides, and the tracheal and bronchial glands were also slightly enlarged; all these glands were pale, juicy, and firm, without being tough. The trachea and bronchi contained a small quantity of frothy mucus and appeared normal. The thymus weighed 1·5 grm., and consisted chiefly of a little fibrous connective tissue. There were numerous sub-pleural petechiæ, but apart from these the lungs appeared normal. The peritoneum was normal. The stomach contained ½ oz. of deeply blood-stained mucus, and in the stomach-wall there were

[1] Shown February 25, 1910. *Vide* p. 92.

numerous small, submucous hæmorrhages. The mucous membrane in the small and large intestines appeared normal, though Peyer's patches were a little prominent. The mesenteric glands were not enlarged, and appeared normal. The liver weighed 18½ oz.; the anterior margin was slightly rounded, and the colour was slightly redder than normal; no leukæmic patches were seen. The pancreas appeared normal. The spleen weighed 14 oz.; consistence firm, but not tough; there was a little perisplenitis; the colour was normal; the Malpighian bodies could not be seen. The right kidney showed two small hæmorrhages at the lower end, and a larger one on the anterior surface. The left kidney appeared normal, except for a few very small petechiæ under the capsule. Both kidneys were rather tough. The retroperitoneal glands were enlarged and of a pink colour. The marrow in the ribs and vertebræ was considerably thicker than normal and pink in colour.

Microscopical examination: The lymphatic glands show a diffuse cellular hyperplasia. The lymph follicles in some of the glands remain fairly well marked, but in the retroperitoneal glands they are mostly indistinct. The increase in cells is partly an increase in the normal lymphocytes, but there are also present a great many cells with large nuclei, surrounded by a much greater amount of protoplasm than is seen in the lymphocyte, and many of these cells contain eosinophile granules. The peripheral lymph sinuses are mostly crowded with cells. There is no increase of connective-tissue stroma. The blood-vessels contain very large numbers of leucocytes. In the liver the intercellular capillaries are greatly dilated, and contain large numbers of leucocytes. The liver cells show a good deal of atrophy. The collection of leucocytes in the vessels is general and evenly distributed, and there are no large patches of leucocytic infiltration. The connective tissue of Glisson's capsule shows a slight infiltration with leucocytes. Sections of the spleen show a thickening of the capsule and a uniform diffuse cellular hyperplasia, with no increase of stroma. The Malpighian bodies are almost entirely obliterated. The increase of cells is not an increase of lymphocytes, but is due to enormous numbers of large cells, with big, rather pale nuclei, surrounded by a large quantity of protoplasm. Sections of the right kidney show extensive recent hæmorrhages and large patches of leucocytic infiltration. Sections of the left kidney show only a very few small collections of leucocytes. In the pancreas there are a few very small collections of leucocytes. Films of the marrow from a rib show large numbers of eosinophile cells, some polymorphonuclear and some with large rounded nuclei. Many nucleated red cells

are present; polymorphonuclear neutrophiles and large mononuclear neutrophiles are numerous, but the granules do not stain well. Films of the discharge from the ulcers on the scalp, made before death, show a large number of myelocytes, many of which are in a state of phagocytic activity.

Enormous Sarcomatous Tumour in a Child aged 2 years.

By Hildred B. Carlyll, M.B.

I. G., a little girl aged 2¾ years, was admitted into the East London Children's Hospital, under Dr. Coutts, on March 14, 1910, suffering from an enormously distended abdomen. The child had been well until January, 1910, when she had an attack of diarrhœa and vomiting, and was kept in bed for two weeks. She was ill off and on after this, but her abdomen was not noticed to be distended until six weeks before admission. Vomiting had recently occurred nearly every day. She was her parent's only child.

On admission the child was well nourished, but looked ill. There was no œdema. The eyes were sunken and the veins on the forehead were enlarged. The abdomen was greatly distended, and measured 23 in. round near the umbilicus. The note was dull, except over a small area in the epigastrium. A thrill was obtained, or a sensation which simulated it. There was a very distended vein in the mid-line above the umbilicus.

On March 18 the flanks were explored with a needle, with a negative result. Shortly afterwards Mr. Trotter performed an exploratory laparotomy. On incising the peritoneum a large, smooth, semi-solid tumour was felt, extending wherever the hand could reach. There was a small quantity of free blood in the peritoneal cavity. The wound was closed, and death occurred on March 21, a week after admission. While the child was under observation there was some pyrexia; the respiration was not greatly laboured; the bowels were regular, and vomiting occurred but once; there was no trouble with micturition.

Autopsy: As soon as the peritoneum was incised the tumour slipped out of the abdominal cavity. On separating a few adhesions, which connected it with the bladder only, it was seen that the only connecting

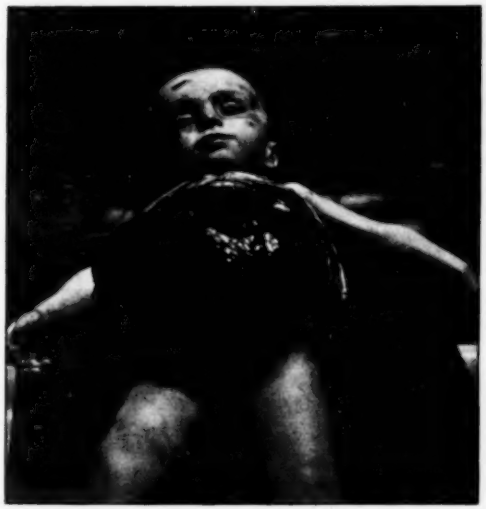


FIG. 1.
Tumour *in situ*.



FIG. 2.
A piece of paper has been placed beneath the arterial attachment of the tumour to the child.

link between the tumour and the child was a branch of the left common iliac artery, with a thin peritoneal covering. The tumour was firm, compact, and gelatinous, and possessed no obvious capsule; it lay apparently entirely within the peritoneal cavity; it was divided into lobes by two clefts. All the organs of the body were normal. The tumour weighed nearly 6½ lb., and sections confirmed the diagnosis of myxo-sarcoma.

DISCUSSION.

The CHAIRMAN asked how the tumour was diagnosed from renal sarcoma before exploration, as that was the chief probability.

Mr. LOCKHART MUMMERY thought it a pity the tumour was not removed in life, as the autopsy showed it could have been, it having been attached only by the large iliac artery. Possibly the patient would have survived.

Dr. CARLYLL replied that the first thing thought of was renal tumour. Other diagnoses were a fluid collection and tuberculous peritonitis. The final idea of the surgeon was that it was a renal tumour. The incision which would have been necessary to remove it would perhaps have been a record one, and it was doubtful whether the patient could have survived. There seemed no anatomical reason against such removal. At the post-mortem no obvious capsule was seen, but now the tumour had partly shrunk and the peritoneal covering was visible in places.

Section for the Study of Disease in Children.[1]

June 11, 1910.

Dr. E. CAUTLEY in the Chair.

Multilocular Cystic Hygroma of Neck.

By CHARLES P. CHILDE, F.R.C.S.

L. D., AGED 4 years. History: Healthy child; has had no children's diseases. A small, soft swelling noticed in the angle between the left clavicle and the anterior border of the trapezius two weeks after birth. This has grown progressively in an upward and forward direction, more rapidly sometimes than at others. It has seemed to swell up at times and then decrease in size again. The last few months it has grown much more rapidly. There is no pain or dyspnœa. A fortnight ago the portion beneath the lower jaw became painful and tender, with a rise of temperature to 100° F. The temperature subsided in a week, leaving this portion of the swelling hard.

Mr. DOUGLAS DREW mentioned a case of extensive cystic hygroma of the chest, axilla, and arm in an infant upon whom he had operated; he had to operate in two stages owing to the extent of the trouble. It was by no means an easy operation, as the vessels and nerves of the axilla passed through the centre of the tumour. After the second operation, when the wound was healed, the child had a very severe attack of inflammation in the remaining part of the tumour situated between the axilla and the elbow, which lasted two or three weeks. He had watched the case for several years since, but the attack of inflammation had not proved curative.

[1] Provincial Meeting held in Portsmouth.

Subperiosteal Resection of the Shaft of the Ulna for Tuberculous Osteomyelitis.

By CHARLES P. CHILDE, F.R.C.S.

D. S., AGED 7 years. History: Measles when aged 3. No family history of tuberculosis. Three brothers in the family, all of whom are quite healthy. No history of injury to the arm. Four and a half months ago began to have pain over the upper end of the right ulna. A swelling appeared later, which was opened; it continued to discharge until admission to hospital nine weeks later. A skiagram exhibited the tuberculous deposit in the ulna. Operation, March, 1910: Complete subperiosteal excision of the shaft of the ulna, the bone being sawn through above, below the coronoid process, and wrenched off the lower epiphysis. The case has progressed satisfactorily, and the new ulna formed from the periosteum can be distinctly traced on the screen.

Partial Tarsectomy for Tuberculous Disease.

By CHARLES P. CHILDE, F.R.C.S.

E. W., AGED 5 years. History: No family history of tuberculosis. No history of injury. A swelling appeared over the dorsum of the right foot when aged 2. This opened and discharged till time of operation (scar on dorsum of foot visible). Operation, October, 1908: Incisions on each side of the foot, and reflection of the tendons. A large portion of the mid-tarsus, including the disease, was removed with the saw. The wound healed satisfactorily, and the child has a very useful foot.

Pseudo-hypertrophic Muscular Paralysis.

By W. CARLING, M.B.

A. B., AGED 8½ years, school-boy. Father: Healthy, and of a healthy, long-lived family; a gunner in the R.M.A. Had a paralysis of one eye fourteen years ago (probably specific). Mother: Healthy. Had two brothers who died of "spinal complaint"; has one brother alive and well, also four sisters well. Has had in family (1) girl, aged 15, well; (2) boy,

who died at four months from whooping-cough and convulsions; (3) girl, aged 10, well; (4) the patient; (5) boy, who died in three days in a convulsion; and since, two miscarriages. Patient: Delicate from birth; cut all his teeth with convulsions; first stood when aged 2. When aged 2 years and 4 months had pneumonia, from which he nearly died, after which he appeared to be very weak and had great difficulty in walking. The muscles of the legs were first noticed to be getting weak, then those of the back, and later those of the arms. The calves are not large, but are very hard. The muscles of axillary fold and arm are atrophied. Stands with body and shoulders thrown backwards, but, on sitting, curve disappears. Sometimes falls down when standing for no apparent reason. When walking has feet well apart, knees turned in, and toes pointing out; rolls body from side to side. Last year was able to walk three-quarters of a mile; this year cannot. Cannot go upstairs; comes down sitting on each step, or if going over one step turns round and passes over backwards. When placed on floor used to climb up himself; now cannot do so. Knee-jerk normal. External squint of left eye. Goes to school and gets on well with his lessons.

Absence of Puncta Lachrymalia (Bilateral).

By L. Cole-Baker, M.D.

R. S., aged 10 years. Suffered from epiphora since birth. An attempt was made to pick up opening in canaliculus in left lower lid without success. The right gland partially removed through incision at outer margin of upper lid. Not succeeding, both were removed through the conjunctiva at outer angle of upper lid.

Case of Syphilitic Dermatitis.

By John T. Leon, M.D.

Girl, aged 7 years. First seen March, 1908. About eight months previously a small papule appeared on the right buttock and gradually spread. When first seen she had a large warty patch on right buttock, partly ulcerated and discharging. There were some isolated raised papules in the groin, and a mass of enlarged glands. Under treatment the lesion nearly healed and the child was lost sight of till early in

the present year, when she reappeared at the out-patient department in a similar condition and in the same place. Under anti-syphilitic treatment it is gradually healing. Calmette's tuberculin test was negative.

Dr. PERNET was of opinion the case was one of lupus vulgaris.

Specimen of Large-celled Sarcoma of Brain.

By JOHN T. LEON, M.D.

THE patient, a boy aged 16 years, was brought to the out-patient department for deafness. On investigation there seemed to be no deficiency in the auditory apparatus, but very slow cerebration. No paralysis or other motor symptoms, except tremors of hands and nystagmus. Sight good, but double optic neuritis. Slight pain in head. No vomiting, except one day. Admitted as an in-patient a few days later, he became progressively more and more drowsy; he could, however, take his food, and no incontinence. About two weeks later sight began to fail rapidly; there was intense optic neuritis in both eyes. An operation was proposed with a view to relieving pressure, but the patient in about twenty-four hours passed into a condition of coma and died from apparently sudden cardiac failure.

Specimen of Peritoneal Growths from a Case of Lymphocythæmia, with Blood-slides.

By JOHN T. LEON, M.D.

GIRL, aged 6½ years, admitted to hospital May 28, 1908, for intense anæmia, enlarged spleen and liver. The mother stated that the child was a very small baby and had never been strong; she was mostly breast-fed till 12 months old. She had whooping-cough when aged 5, and about six months later she began to get progressively weaker and thinner. The child complained of no pain, but some abdominal discomfort on exertion. Blood-count on admission: Red cells, 560,000; white, 17,500. Differential count: Lymphocytes, 80 per cent.; polymorphonuclears, 10 per cent.; normoblasts, 2 per cent. Liver extends 2½ in. below the costal margin, and the spleen is much enlarged. Has signs of fluid in the peritoneal cavity. On June 9: Hæmoglobin, 20 per

cent.; red cells, 800,000; white, 8,000. The abdomen got progressively bigger and the spleen slightly larger. The child died on July 29. The temperature at first was normal, but during the last month of her illness there was some pyrexia with cough, and crepitations at places over both lungs. Enlarged glands were present in both axillæ, groin, and neck. Post-mortem: Lymphoid growths were present in practically every organ. The liver weighed 30½ oz., the kidneys 11 oz. and 9 oz., and the spleen 5½ oz., all these organs being the seat of numerous growths. The temperature during the last two months of illness varied from 98° F. to 100° F.

Intussusception of the Small Intestine, containing a Sarcoma of Intestinal Wall, in a Boy aged 4 years; Enterectomy; Recovery.

By C. A. S. Ridout, M.S.

History: November 14, 1908—Acute abdominal pain in spasms; *nil* found on examination; no vomiting; bowels open. November 18—Similar attack; frequent vomiting this time; temperature 100° F., pulse 120; *nil* on examination of abdomen; bowels open. November 23—Lump felt to right of umbilicus, which disappeared next day. November 28—Another severe attack of pain and vomiting; right rectus abdominis muscle rigid; pulse 132, temperature 98·4° F. Laparotomy, November 28—Under anæsthetic, sausage-shaped mass felt to right of umbilicus. On opening abdomen a congested enteric intussusception was found, with tightly-constricted neck, quite irreducible; a mass of enlarged mesenteric glands also present. The whole of intussusception was resected, and end-to-end anastomosis rapidly performed by suture. Patient made slow recovery, but bowels acted freely the day after operation. Patient was sufficiently recovered to go home in about ten days. Examination of specimen showed that the cause of irreducibility was a puckered, indurated growth of the intestinal wall, which would not pass through the tightly-constricted neck of the intussusception. Microscopical examination showed the growth to be a round-celled sarcoma; it occupied laterally the wall of the intestine, and its circumference was about the size of a halfpenny; total length of gut involved in intussusception, about 14 in. Patient died some months later with secondary deposits in the liver.

A Case of Extensive Injury to the Head in a Boy aged 7 years.

By C. A. S. Ridout, M.S.

January 23, 1905: Patient struck by fall of telephone pole on left side of head and rendered unconscious. On admission to hospital presented the signs of concussion and could be roused on stimulation; also slight paresis of right arm and right facial palsy. A large hæmatoma, with small punctured wound, was present over left parietal region of skull. Operation: Disorganized brain-matter escaped after passage of probe through scalp wound, and on turning down parietal flap lacerated brain-matter and blood escaped through an extensive comminuted fracture, involving the left parietal and temporal bones and left side of frontal. Some portions were removed, others which were depressed were raised; some lacerated cortex of brain was cut away, mostly from neighbourhood of Rolandic area. After-progress: Great cerebral irritation, with slow return of consciousness; marked aphasia, which slowly disappeared; hernia cerebri felt under scalp; marked right facial paralysis, which slowly recovered; weakness of right upper and lower limbs, which to some extent recovered but have remained in a spastic condition, flexor power being much greater than extensor power, and finer movements, such as writing, &c., impossible; athetosis is also present. Further pieces of bone ultimately came away, leaving a large area, including practically the whole of the left Rolandic area, uncovered by bone; the brain pulsates under the scalp, and an aluminium cap is worn as a protection. 1910: Mental power is good, and but for the spasticity mentioned above the patient is able to get about and play with other boys.

Case of Pemphigus.

By Claude C. Claremont, M.D.

D. C., aged 5 years, admitted April 23, 1910. There is nothing in patient's personal or family history to suggest a specific infection. With the exception of measles eighteen months ago, patient has had no previous illness. Present illness dated only a fortnight before admission; it commenced with pimples in the head, which gradually spread over the body. On admission the child was covered with a bullous eruption.

The blebs varied in size from nothing up to a grape or larger. The contents were clear. The blebs were tense and, at places, almost confluent. The scalp and forehead were affected, but the bullæ here were not large, and over the scalp had mostly discharged their contents. The back was very closely covered. The arms were thickly covered except on the inner surfaces. The eruption on the front of the trunk was comparatively sparse except at the lower part. The legs and feet were thickly covered. On the back of the hands the rash was on the point of erupting, the papules being hardly vesicular and quite small. Some of the scabs of the broken vesicles were sanious, probably from violence. Since admission there has been great improvement. The vesicles have dried up, leaving healed and healthy surfaces. The formation of fresh vesicles continues, but they do not now exceed six a day. There have been few on the soles and none on the palms. They have been systematically pricked daily, and the child has been taking Donovan's solution.

Case of Purpura.

By CLAUDE C. CLAREMONT, M.D.

D. H., AGED 2 years and 5 months, admitted May 30, 1910. Has always been a backward child; first teeth appeared at 9 months. Present illness has lasted for four months. On admission, patient presented purpuric patches on the cheeks, forearms, and legs, all dull red, almost yellow in tint. They were said by the mother to be sometimes brighter. Since admission the patches on the cheeks have resumed a bright red colour, and fresh spots have appeared on the buttocks, thighs, and upper arms. The gums round the upper incisor teeth are a little inclined to bleed. The child is rickety, but otherwise, except for the eruption, would appear to be in good health. The coagulability of the blood is deficient.

Dr. PERNET said the case was an unusual one, especially the symmetrical patches on the cheeks. The child was certainly rickety, and moreover the depressed nose and rather prominent frontal bosses pointed to congenital syphilis. He would recommend calcium lactate, as the blood was lessened in coagulability, and also mercury in the form of inunction. He had seen symmetrical infiltrations of the cheeks, though not quite like this—viz., not hæmorrhagic, but yellowish in appearance—disappear under mercury. Although the child had had no hæmorrhages from mucous membranes, he might have

such, and should therefore be kept under observation. Dr. Pernet inquired if there was albumin in the urine. The appearance of the child made him think there might be.

Multiple Exostoses.

By Harold Burrows, F.R.C.S.

F. L., aged 4 years. When 18 months old the patient developed symptoms which were attributed to left hip-disease. He was treated with a Thomas's hip-splint for two and a half years. He was then brought to hospital with a supposed fracture of the forearm. The deformity and limitation of movement at the elbow were found to be due to an exostosis growing from the radius. The patient has numerous exostoses growing from scapula, ribs, and ends of long bones.

Specimen of a Chylous Cyst of the Mesentery associated with, and perhaps due to, Enlarged Glands in the Mesentery. Death from Volvulus.

By Harold Burrows, F.R.C.S.

The patient, who was under the care of Dr. C. H. Thomas, was a boy aged 5 years. He was operated upon for acute abdominal pain and collapse. On opening the abdomen a white cyst was found growing from one side of the mesentery. The cyst contained 15 oz. of fluid. The cyst was hastily emptied, the greater part of it removed, and the abdomen closed. Any elaborate operation was out of the question owing to the extreme collapse. The patient did not rally, but died twelve hours after the operation.

A post-mortem examination revealed a volvulus of the small intestines. The mesentery contained numerous enlarged lymphatic glands. The volvulus was caused probably by a rotation of the cyst, the mesentery being twisted at the same time. The mesentery, being thick and rigid on account of the enlarged glands, neither adapted itself to its new position nor became untwisted. The fluid from the cyst proved to be pure chyle, without any admixture of pus or other cellular elements.

Microscopical examination of an enlarged gland from the mesentery yielded no information on account of putrefactive changes. There were enlarged glands in the transverse mesocolon as well as in the mesentery.

Shortening of Right Humerus and Left Thigh due to Unobserved Fractures in Early Life (Infantile Scurvy ?).

By HAROLD BURROWS, F.R.C.S.

THE child was nursed for three months, and then fed chiefly on Robb's biscuits until aged 1 year and 8 months. Then the child "became paralysed," and screamed whenever he was picked up. The X-ray photographs, which were taken by Dr. Beverley Bird, show that there has been a fracture through the upper part of the shaft of the right humerus, and another fracture through the lower end of the shaft of the left femur.

The Diagnosis and Treatment of Intussusception in Children.

An Address Introductory to a Discussion on the Subject.

By C. P. CHILDE, F.R.C.S.

WE felt some diffidence in suggesting a subject for discussion at this meeting owing to the lack of a special department for the treatment of diseases of children in Portsmouth, for it is in special hospitals or special departments that large opportunities occur for studying particular types of disease, and it is from the experience gained in them that we look for authoritative guidance. I feel, therefore, that I owe the meeting an apology for introducing this subject, as I have had no larger experience in it than falls to a surgeon at a general hospital, and I hope to learn from the discussion that follows more than I can suggest with regard to it.

The subject of intussusception is one of great interest for many practical reasons: first of all, because it concerns the general practitioner equally with the surgeon; secondly, on account of the multiplicity of treatments advocated for it, and the divergences of opinion as to the efficacy or advisability of some of these; and, lastly, by reason of the very high mortality—and, I think, unnecessarily high mortality—which attends it. It may therefore be not unprofitable to introduce it again, though it has been dealt with in many discussions and by many able writers of late years, and though there may be nothing new to be said about it. For if, as I hope to show, the general trend of medical

and surgical opinion as regards its treatment is in a certain direction, it is only by reiterating the reasons for our conclusions, and emphasizing the results which can be attained in that direction, that we can hope to root out other methods which undoubtedly have been attended with a measure of success, and yet attachment to which, I think, can be proved to be the direct cause of its high mortality.

I may say, to begin with, that I do not propose to allude to any of the rarer forms of intussusception, or the rarer complications that may arise in the course of it. I intend to take the typical acute intussusception of young children, which is a straightforward disease, and to deal with it in an entirely practical manner, which is the only way in which I am at all competent to discuss it from my experience as a surgeon. The typical acute intussusception of young children is the ileo-cæcal or cæcal ileo-cæcal variety, the ileo-cæcal aperture or some portion of the cæcal wall immediately contiguous to it forming the apex of the intussusception. According to Von Bergmann, this variety accounts for about 70 per cent. of all cases of infantile intussusception. Of 62 cases of intussusception in children collected by Holt, 74 per cent. were ileo-cæcal. Of 59 cases occurring in the Edinburgh Hospital for Children between the years 1898 and 1908, 76·3 per cent. were ileo-cæcal. That is the form.

Secondly, as to the diagnosis. It may be stated at the outset that it is usually easy. In fact I think it is no exaggeration to say that, with the possible exception of ruptured ectopic gestation, it is the easiest to diagnose of all acute abdominal affections. The diagnosis of many acute abdominal disorders is difficult for the reason that they exhibit in common a group of symptoms and physical signs, while there may be, and frequently is, present no characteristic indication which enables us to discriminate between them.

But in the acute intussusception of children the case is different. We possess here many special aids to diagnosis. First of all, there is the fact that the disease appears at a certain definite period of life in a large majority of instances. This period is the first year. Of 16 cases published by Clogg occurring in children, 12, or 75 per cent., were in babies under 12 months; of the remainder, the oldest was 3 years. Of 123 cases of intussusception at all ages treated in the Glasgow Royal Infirmary between the years 1893 and 1906, 75 per cent. occurred in babies of 12 months or less; and of the remainder, 15 per cent. between the ages of 1 year and 8 years. The next aid in our diagnosis is the sex. The same tables from the Glasgow Royal Infirmary give

89 males to 34 females: in other words, males were attacked two and a half times as frequently as females; and just as in considering the probabilities in other acute abdominal affections (e.g., those of the appendix, gall-bladder, &c.) the sex of the patient is of some presumptive value, so it is of a certain value here. We find, therefore, that the disease usually appears during the first year of life, and most commonly in a male. The attack is ushered in with sudden abdominal pain, to which the patient naturally draws attention by screaming and lying with his legs drawn up. Vomiting, as a rule, soon follows, and collapse is usually present. The initial symptoms in fact are, as might have been expected, those of the acute abdomen, and were it not for the fact that the little patient is unable to describe his symptoms, there would probably be, as a rule, as little doubt that an acute abdominal catastrophe had occurred as in the case of a perforated appendix or gastric ulcer. But the fact that the patient is unable to give his own case-history, coupled with the circumstance that screaming, stomach-ache, and sickness are everyday occurrences in babies, leads to the seriousness of the attack in its early onset being frequently overlooked by the parents or medical attendant. The oversight on the part of parents we are unable to combat; our own shortcomings are less excusable.

When, therefore, summoned to a child suffering with these symptoms, our first responsibility consists in excluding or confirming intussusception before treating the ailment lightly, or prescribing a soothing powder or syrup. How is this to be done? By an abdominal or rectal examination, or both combined. Distension is not present till late in the disease; a tumour, generally quite easily felt, is present early. Greig, who has published an account of 20 cases under his own care at the Dundee Royal Infirmary, writes: "In no early case have I failed to find a tumour." Clogg writes: "The diagnosis depends on the presence of a tumour." Of 16 cases which he has published, a tumour was palpable in 15. The only case in which a tumour was not palpable was one of six days' duration, with great abdominal distension. Clogg felt a tumour quite easily in one case, only two hours after the onset of the attack. In two cases brought to him with suspicious symptoms, no tumour was palpable under an anæsthetic. The patients were not operated upon, and both recovered. In 59 cases occurring at the Edinburgh Hospital for Children, between the years 1898 and 1908, a tumour was palpable in all—in 29 *per rectum*, in the remainder by abdominal palpation.

Either by abdominal palpation or rectal palpation alone, or by these methods combined, and with the help of an anæsthetic if

necessary, of which I shall speak directly, very few tumours in small children can escape detection if distension is absent, as it usually is early in the disease. There are, in addition, other signs frequently but not invariably present which are of great assistance. I mean tenesmus and the passage of mucus and blood *per anum*. It must be borne in mind that the condition is one of acute obstruction, and that the normal intestinal contents (fæces and flatus) will not, as a rule, pass it. Nevertheless, early in the illness the bowel below the obstruction may empty itself, and fæces may be evacuated; also in some cases there may be a pervious way through the intussusception, and fæces and flatus may escape from above through an obstruction which is not complete. With these exceptions, flatus and fæces are not passed. Blood and mucus, on the other hand, poured into the bowel below from the strangled and congested intussusceptum are usually evacuated, accompanied by straining. This sign was only absent in 3 of the 59 cases at the Edinburgh Hospital at some time or other of the disease. When present the diagnosis will have to be made most commonly between ileo-colitis and intussusception. The fact that fever is usually present in inflammatory affections of the intestinal tract and absent in early intussusception (the patient suffering from collapse, with a low temperature), and the fact that in inflammatory conditions of the intestinal tract there are mixed with the blood and mucus the contents of the intestinal canal, whereas in intussusception the condition is one of acute obstruction and blood and mucus are the only material passed (with the exceptions I have just mentioned), will help to a decision. But here, again, it is the abdominal or rectal examination, or both, which reveal the tumour and settle the diagnosis. Such affections as rectal polypus and Henoch's purpura with abdominal crises will be likewise excluded by a competent physical examination.

Of the 8 cases of acute intussusception that have come under my care, 7 were in young children. In none of these was there any doubt about the diagnosis; the only case in which there was any difficulty was that of an adult with the enteric form of the disease. Of course I do not mean to suggest that there may not be great difficulty in arriving at a conclusion, but I am speaking of the common event; and the common event is that acute intussusception in children can be quite easily confirmed or excluded by a careful attention to the symptoms and a competent physical examination. The essential point is to confirm or exclude intussusception in children with acute abdominal symptoms before treating the complaint as a minor ailment.

It forms three-quarters of all acute obstructions in early life. According to Cheyne and Burghard, "acute intestinal obstruction in infants is always due to intussusception, if an external strangulated hernia be excluded."

The necessity for an early diagnosis supplies the key to the treatment. Acute intussusception may be said to be handicapped by a multiplicity of treatments. Different treatments have their advocates, and are attended with success in a certain proportion of cases. Intussusceptions, for instance, have been reduced over and over again by distending the bowel below with gas or water, by inversion, by manipulation through the abdominal wall, or by a combination of these methods, and such measures are countenanced at the present time in most text-books on children's diseases as proper preliminary procedures before resorting to operation. In attempting, however, to arrive at the most hopeful treatment, that which will give the highest aggregate of recoveries as distinct from a brilliantly successful result here and there, I think the following considerations should be kept definitely in view :—

(1) Every case of intussusception is probably reducible by direct manipulation within the abdomen in the early stage, and certainly becomes more difficult to relieve with every hour of delay, chiefly from swelling of the intussusceptum owing to strangulation at its neck.

(2) If the tumour becomes irreducible the patient will lose his life. There is no fact more clearly established from experience of the treatment of this disease than that, if the abdomen is opened and reduction is impossible, whatever operation is performed, whether resection or Barker's operation, or enterostomy, or lateral anastomosis without interference with the tumour, or the various other operative procedures which have been suggested, the child will die. None of them gives the patient a fighting chance. There is no record of an infant having survived any of these operations at our hospital. I have myself resected the intussusception on three occasions and performed Barker's operation twice, with the monotonous result of death every time within twenty-four hours. On one occasion I closed the abdomen, on being unable to reduce the tumour, feeling certain that had I attempted resection the child would not have left the operating table alive. The Edinburgh Hospital for children shows no better results. Of 59 cases of intussusception, 15 were submitted to resection, anastomosis, or colostomy : all of them died. Greig in 20 cases had three resections. There were no recoveries. In the records of the Glasgow Royal Infirmary, of thirty-one operations at all ages, including resections,

anastomoses, and colostomies, there was no recovery in an infant. Two recoveries occurred in patients aged 10 and 17 years respectively. I have myself had one successful case of enterectomy for acute intussusception in an adult. Clubbe has recorded one recovery in a child 11 months old; Frank, one in a child of 9 months; and McDowell, one in a child of 9 months. These are as rare as the auk's egg. It would be easy to give similar results of these operations from the records of any hospital or from the practice of any surgeon. Nor is it to be anticipated that any modification of technique, unless some very much simpler method of dealing with these cases is devised, will give better results in the future. It is well known how serious the operation of resection of intestine is in a healthy adult in the presence of acute obstruction. How is it to be expected that babies will stand such an operation, however skilfully performed? Resection or other operations for irreducible intussusception, as at present known and performed, are not therefore worth serious consideration as matters of practical treatment. They will probably continue to be performed as a forlorn hope, and possibly rightly; but they cannot be considered in any but the light of fatal operations, and closing the abdomen on the off-chance of Nature effecting a cure by casting off the intussusceptum would probably give no worse results.

(3) Infants attacked with intussusception are mostly strong, healthy children. Of the 59 cases before referred to at the Edinburgh Hospital, in only 3 were the patients pronounced delicate; nearly all were strong, healthy children. Of Clogg's 16 cases, 15 were stated to be perfectly healthy children.

(4) Strong, healthy children will stand laparotomy and reduction of the intussusception perfectly well if done early enough to make it an easy and rapid operation. This is amply shown by statistics. Dunbar has reported 7 cases in which laparotomy and reduction was performed with six recoveries; Clogg had 14 cases of laparotomy and reduction with only two deaths, exactly the same proportion—*i.e.*, nearly 86 per cent. of recoveries; Wiggin had 77 per cent. of recoveries from early laparotomy and reduction; Marnoch reports 4 cases in which laparotomy and reduction was performed within fifteen and a half hours of the onset. All recovered.

(5) Whatever the treatment adopted, the time intervening between the onset of the attack and the relief of the same is the main factor determining the result. As I have shown above, if that time is sufficient to allow the intussusception to become irreducible, the patient

almost invariably dies. But even if on opening the abdomen the intussusception is reducible, the time at which it is reduced has an unmistakable bearing on the result. For instance, in 12 cases reported by Greig there were 6 of laparotomy and reduction followed by recovery. The average duration of these was twenty-seven hours. There were, on the other hand, 6 cases of laparotomy and reduction which proved fatal. The average duration of these was fifty-one hours. Of 123 cases at the Glasgow Royal Infirmary, 60 per cent. of recoveries took place in cases treated within six hours of the onset, 73·3 per cent. in those treated within twelve hours, 57·5 per cent. in those treated within twenty-four hours, 26·6 per cent. in those within forty-eight hours. According to McGregor, who has investigated these cases, there are 61 per cent. of recoveries within twenty-four hours; after forty-eight hours all treatment is futile. Of Clogg's two deaths in 14 cases of reduction, in one the duration had been four days, in the other seventeen hours. In Marnoch's 8 cases there were, as I have stated before, four recoveries in cases treated within fifteen and a half hours, four deaths in cases treated beyond that time. Greig's tables show the average duration of recoveries to be twenty hours, the average of deaths fifty-eight hours. In 10 cases of three days' duration there were nine deaths, in 10 of four days' duration eight deaths, in 7 of seven days' duration seven deaths; in other words, there were only three recoveries in 27 cases after forty-eight hours. Figures of this kind could be easily multiplied from the literature. They all lead to the inevitable conclusion that the time intervening between the onset of the attack and the relief of the intussusception, by whatever means that relief is obtained, is the main factor influencing the mortality.

(6) The measures of reducing intussusceptions without opening the abdomen by inflation, irrigation, inversion, or manipulation through the abdominal wall, though successful at times, are open to many obvious objections. First of all they are crude and unscientific, in that they are used in complete ignorance of the condition of the intussusception if reduced, and employ forces which cannot be accurately gauged. Secondly, they leave uncertainty whether the apex of the intussusception, the most difficult part to unfold, has been reduced at all, and many cases have been reported in which the palpable tumour apparently disappeared, yet which subsequent laparotomy revealed not to have been reduced entirely. Thirdly, they leave it uncertain whether intussusception will not immediately recur. Fourthly, they are open to the risk of reducing intestine which is incapable of regaining its vitality. Fifthly,

they are open to the risk of rupturing the sheath in the process, seven cases of which have been collected and reported by Lawford Knaggs. These objections are not theoretical, but are founded on what has actually occurred over and over again in cases treated by these measures. They more than offset the risk of opening the abdomen early in a healthy child, which has been proved by experience to be small. But the greatest of all the objections to these procedures is that they mean delay and hesitancy in the treatment of this grave abdominal disorder. They constitute an alternative treatment which is sure to appeal to parents if discussed side by side with operation, and lead to pressure being brought to bear on the medical attendant to try them and persevere with them before resorting to the surgeon. Meanwhile, when unsuccessful, as they only too frequently are, the patient's powers of endurance have been diminished, the difficulties of reduction have been increased, and the opportunity of an early, short, and easy unfolding of the intussusception within the abdomen, which is *the* operation, and the only operation, that young children will stand, has been lost.

If these considerations in dealing with intussusception have been fairly stated, and are not wide of the truth, there is only one method which gives confidence of materially reducing its mortality—immediate laparotomy, with the expectation of an easy and scientific reduction under the eye of the operator. If the tumour through delay is permitted to become irreducible the case is practically hopeless. Nature's cure, the casting off of the intussusception, according to Wiggin, occurred once in 103 cases—a desperate chance, and one not worth considering. To sum up the matter and reduce it to actual practice. In all cases in which the symptoms detailed above are present, intussusception should be the first consideration in the practitioner's mind, and a careful abdominal and rectal examination should be made for a tumour. If this cannot be palpated, and symptoms are suspicious, the patient should be examined under an anæsthetic, preparation having been previously made for operation, so as to avoid a double anæsthesia. On diagnosis the abdomen should be opened and the intussusception should be reduced. If irreducible, the surgeon may resort to any operation he prefers; but the patient will die whatever operation he performs, and a hopeless prognosis should be given. The methods of irrigation, inflation, and inversion may possibly be of use after the abdomen has been opened as aids to reduction; apart from this, they should be reserved for those cases only in which either surgical assistance is not procurable or the opinion is held that the patient is unable to stand a laparotomy.

Twenty-five years ago much the same views were entertained with regard to strangulated hernia, a condition in many respects comparable with intussusception. There were all sorts of preliminary treatments in vogue—enemata, hot baths, ice to the tumour, taxis, taxis under anæsthesia, &c.; sometimes successful, yet all of them contributory causes, through the delay entailed, of the high mortality when the patient came to operation. Time is not wasted with measures of this kind nowadays. Immediate operation is recognized as the most scientific, safest, and best all-round procedure, and simplifies the matter by eliminating all doubt or hesitancy about what should be done. There should be the same clear understanding with regard to intussusception. There are greater risks in delay and preliminary measures than in early operation, and, however successful the former may be in individual cases, the high mortality at present existing for intussusception is directly traceable to their remaining recognized methods of treatment.

Mr. Childe exhibited a young adult, together with specimen, in whom 31 in. of small intestine had been excised for an irreducible enteric intussusception, with closure of both ends, and subsequent lateral anastomosis. The patient eventually recovered after a large empyema had been evacuated from the right pleural cavity.

DISCUSSION.

Dr. CAUTLEY agreed with Mr. Childe as to the extreme importance of operative treatment at the earliest possible time after the diagnosis was made. Nevertheless, it was important to remember that an intussusception undergoes reduction spontaneously in a few instances, and evidence thereof had been found at operation for its relief. Sometimes there was great difficulty in diagnosis, especially in the early stages, at the time when a general practitioner first saw the patient. Thus the symptoms might be limited to severe collapse, without screaming or vomiting. Occasionally, serious hæmorrhage from the bowel was alone present, and in one such case no tumour was found on examination under anæsthesia. Henoch's purpura was rare, and usually diagnosed with ease, but there were cases on record in which it had been combined with intussusception.

Mr. DOUGLAS DREW thought that Mr. Childe took too pessimistic a view of those cases in which it was necessary to resect the intussusception. Mr. Drew had performed resection in four cases, all of which were fatal, and while admitting that these cases were well nigh desperate, yet from time to time successes were recorded. There had been two at the Queen's Hospital for Children—one in a child of 4 years, and another in an infant under 2 years. He had also read of more than one successful case in infants under

12 months. While recognizing that the secret of success was early operation, Mr. Drew had met with several cases in which the symptoms had been present several days, but which recovered after operation. Mr. Drew did not agree that the diagnosis was always easy in the early stages of the disease; he pointed out that the surgeon had the great advantage of examining the child under chloroform before operating, which rendered the diagnosis easy, as the detection of a tumour was almost certain in the vast majority of cases. He had only met with one case in which a tumour could not be felt under the anæsthetic, but in which an intussusception was present. He had, however, made the mistake of operating on a case of Henoch's purpura. Mr. Drew was under the impression that inflation and injection had long since been abandoned as unsatisfactory; he had never employed either method.

Mr. DUNCAN C. L. FITZWILLIAMS: I should also like to express the appreciation which I know we must all have felt in listening to Mr. Childe's paper. All surgeons, nowadays, I am sure, are ready to endorse the views Mr. Childe has just expressed. The Chairman stated that he thought surgeons found the diagnosis easier than the physician or general practitioner, because the surgeon saw the case at a later stage; but I cannot altogether agree with this. Pain, screaming, vomiting, passage of blood, symptoms all found quite early, point then as clearly to the cause as they do later; it should not need their repetition to direct attention to the true nature of the cause. If these symptoms are present, as they are in the majority of cases, from near the onset, diagnosis should be as easy then as later, where the symptoms have not changed in number but have done so in severity. I met with a very good example of this not long since. A child, who was being brought to see me periodically on account of rickety deformity, was brought as usual one morning, and, after hearing from the mother of the satisfactory progress and seeing the legs, I was about to dismiss her when the collapsed appearance of the sleeping child struck my attention. Though no word had been mentioned as to anything unusual being the matter, on inquiry I heard that just before starting for the hospital, some three to four hours previously, the child had passed a motion, and then began to have severe pain. She had vomited twice and had an intense desire to stool, but only once had been able to pass anything, and that was slightly blood-stained. Placing the hand on the abdomen, the tumour was easily felt, and the child sent up for admission. I do not think many cases are seen at an earlier date than this, and yet if this one had been overlooked the reason would certainly have been want of careful examination, and not difficulty in diagnosis. With regard to the tumour being palpable, it may be said that in the early stages over 90 per cent. of cases are palpable. In the later stages the tumour is masked by the general peritonitis. The tumour is twice as often felt on the left as on the right side; if in the mid-line, it is usually above or about the level of the umbilicus. The most difficult position in which to feel it is near the hepatic flexure, where it may lie under and be shielded by the edge of the liver. Here it may lie absolutely concealed, as in one case in my own experience, where, with a lax abdomen and under an anæsthetic, no tumour

could be demonstrated until the operation revealed it. The fatality which accompanies the irreducible cases is only too familiar to all, but I think lately a distinct advance has been made by the application of spinal anæsthesia to these hitherto hopeless cases. To Mr. Tyrrell Gray, late Resident Medical Officer at Great Ormond Street Hospital, this credit is due; and Mr. Fairbank lately has been able by this procedure to save the life of an infant only 7 months old, in which resection and anastomosis were necessary. This, I believe, is the first success yet obtained at such an early age. With regard to the various methods of reduction, we should keep in mind the general well-proved principles of abdominal surgery; and if a departure from them is made in the case of children, it should be made in response to some very urgent need. In intestinal surgery it is a principle that the gut should be emptied as completely as possible before operation. What, then, can be the excuse of those who fill it with fluids as a preliminary measure? The pressure which is used with safety is so low that one cannot reasonably expect reduction in any case where reduction would not be much more easily effected by the fingers. I have repeatedly seen attempts at reduction by fluids fail, and reduction easily obtained by operation. That being so, septic fluid contents can only add greatly to the risk from leakage through already ruptured gut, or during a possible resection. If the gut is to be filled with anything, air is infinitely less risky; but even this should be a method of treatment only in those cases which are more than six hours from the help of surgery—a position occupied by very few places in the British Isles at the present day.

Mr. C. A. S. RIDOUT agreed with the Chairman as to the difficulty of diagnosis, emphasized the necessity of speed in operating, and mentioned particulars of a case of his own in which he removed 14 in. of enteric intussusception containing a sarcomatous growth with recovery, and in this case an end-to-end anastomosis was rapidly done. The patient was a boy, aged 4½ years.

Mr. GEORGE MORGAN quoted from a paper he had recently read before the Brighton and Sussex Medical Society a record of his last twelve cases. Of these twelve cases, eleven were under 1 year, and the twelfth only 1 year and 8 months. Of the twelve, four were "too-late cases" and so died; the other eight recovered. As regards diagnosis, he emphasized the value of a history of screaming followed by some shock, and accompanied by tenesmus and retching, but recognized the fact that the onset might be so gradual and imperceptible as to be overlooked, or, on the other hand, so sudden and the shock so severe as to destroy life in four hours. In eleven out of the twelve cases a tumour could be felt before anæsthesia; in the twelfth the tumour was tucked under the edge of the liver, the most likely position for it to be overlooked. He hoped in the future no physician or surgeon would lose valuable time by trying inflation, as it was quite impossible for anyone to say if the whole of the intussusception had been reduced. In one of his cases—a baby with whooping-cough—on the eighth day during a violent fit of coughing and retching the stitches gave way, and the intestines escaped into the bed. It was in the night, but the intelligent

nurse on duty wrapped the bowels up in hot cloths until he reached the hospital. The child was in a state of severe shock, but made a good recovery. The intestines were sponged with hot saline solution, and some of the solution left in the peritoneal cavity. Finally, he remarked on the high temperature that might follow an operation for intussusception, rising up to 104° F. or even 107° F., due to the escape of the imprisoned blood, now probably loaded with toxins, into the general circulation, and so causing fever. He had never lost a case from this cause.

Mr. LOCKHART MUMMERY thanked Mr. Childe for his paper. He did not consider the prognosis, after the condition had existed for more than forty-eight hours, as anything like so bad as Mr. Childe had stated. He quite agreed that speed in performing the operation was of the utmost importance. His own view was that twenty minutes was the very maximum time which could safely be taken in performing an operation upon any child under 2 years. He mentioned two cases upon which he had operated within the last month: one was an infant aged 9 months, in whom the condition had existed five days; the other child was aged 1½ year, and the symptoms in that case also had existed for five days. In both patients the condition was successfully reduced, and recovery occurred.

Mr. HAROLD BURROWS referred to cases of mistaken diagnosis in Henoch's purpura. Instances of this were not infrequent in which a child had acute abdominal pain of a colicky nature with tenesmus and passage of blood and mucus by the bowel. These symptoms might be due to a hæmorrhage into the wall of the intestine or to an intussusception, and a differential diagnosis was most difficult. Another error which had occurred on more than one occasion was to mistake the ileo-colic prolapse, remaining after spontaneous reduction of an intussusception, for a tumour, and to perform excision of the involved portion of bowel. He had seen a case in which, a diagnosis of intussusception having been made, the abdomen was opened and no intussusception was found. The lowest part of the ileum, however, was congested, and a tumour, about ½ in. in diameter, could be felt projecting into the cæcum from the ileo-cæcal valve. The surgeon excised the affected portion of the bowel, being under the impression that the symptoms had been caused by the tumour. The child died, and on examination of the supposed tumour it was found to be merely œdematous mucous membrane prolapsed through the ileo-cæcal valve; the case being one of spontaneous reduction of an intussusception. After an intussusception had been reduced by operation this œdematous mucous membrane could always be felt.

Dr. PERNET desired to refer to one point only, and that was Henoch's purpura, which had been mentioned by several speakers. He would like to know what they meant by Henoch's purpura. Henoch had described a purpura fulminaris (fatal in from twenty-four hours to four days) that went by his name, but Dr. Pernet did not consider that form had anything to do with intussusception, nor did Henoch [1] say anything as to the latter condition in this connexion.

[1] Henoch's "Lectures on Children's Diseases," ii, p. 379; *see* also "Intussusception," op. cit., ii, p. 65.

Mr. W. J. MIDELTON said he had listened with great interest to the paper and remarks of subsequent speakers. He agreed with the speaker who emphasized the importance of general practitioners attending meetings. In his own experience as a general practitioner, extending over twenty years, he tried inflation in one of his early cases; the child died. Since then he had always advised operation. None of the speakers had mentioned bleeding from hæmorrhoids as causing a difficulty in diagnosis. In one case that had come under his notice the child passed a good motion twenty-four hours after intussusception had been suspected, and operation was not performed until twenty-four hours after that; the child recovered.

Dr. L. MAYBURY sent the following communication: Unfortunately I was not present when Mr. Childe read his paper, but I would like to make a few remarks on the subject under discussion. I have had a good many cases of intussusception in children, and I cannot remember having lost one. The diagnosis is not difficult, I think, if one goes carefully into the history of the case—the usual sudden onset, vomiting, pain, straining, and the passage of blood and mucus; sometimes at first fæces, but when obstruction is complete, then never. The blood is at first bright and then of a darker colour, quite different from colitis. There is, I think, nearly always to be felt an abdominal tumour by palpation of the belly and by digital examination *per anum*. With a stalled finger it is sometimes not easy to feel the descending part, which in many cases is hard, but in others is soft, so that the finger easily passes between it and the ensheathing bowel. It all depends upon whether the patient is seen early or late—whether reduction is possible without an operation or not. In my cases I have been fortunate in seeing them early, and though in some cases there has been difficulty in reducing the intussusception, yet they have made good recoveries without operation. One is not very likely to overlook a case of intussusception amongst the better classes or in hospital practice, but amongst the poor there is no doubt such cases are occasionally not diagnosed early. Although two of the surgeons present have condemned inflation and irrigation, I have used inflation in all my cases, and with satisfactory results. A soda-water syphon inverted, and the nozzle attached to a rectal tube, is a very good means of obtaining gas. It matters very little what position the patient is in, but I think the dorsal horizontal is the best. Inflation with abdominal manipulation under an anæsthetic has proved successful in my cases. Should the intussusception have been present any length of time, then the knife is clearly the best treatment. I always administer opium after the reduction to keep the gut quiet and prevent peristalsis.

Mr. CHILDE, in reply, said that some of the speakers were of opinion that the various methods of reduction without opening the abdomen had become obsolete; but a reference to recent literature on the subject, as well as the remarks of some of those present, had shown that such was not the case. It was also objected that the diagnosis of intussusception was not, as a matter of fact, so simple as he had indicated, and that the reason the surgeon usually found the diagnosis easy was that only the later stages of the disease came

under his notice. He quite admitted, and had stated in his paper, that there might be difficulties in the early diagnosis, but he did not believe that this was the main reason of the frequent oversight of the disease. He attributed it to the fact that stomach-ache, screaming, and vomiting, which were the initial symptoms of an acute abdominal crisis in a baby, were of such everyday occurrence at this period of their existence that frequently, and very naturally, little significance was attached to them by the parent or medical attendant. He believed that if intussusception were more often kept in mind when these symptoms were present, and the determination was uppermost to confirm or exclude it, this could be accomplished in a large majority of instances by examination, as abdominal distension was absent early, as a tumour was usually present early, and as by bimanual examination, under anæsthesia if necessary, very few tumours could escape detection in infants. Mr. Drew thought that the speaker was too pessimistic with regard to the results of resection, and mentioned two cases at the Queen's Square Hospital for Children which had recovered. Mr. Ridout also mentioned a case of successful resection. These successes, however, occurred in rather older children, and Mr. Drew admitted that they were the only recorded cases of recovery. While, therefore, these results were very satisfactory and a source of congratulation to the operators, they merely furnished the exceptions that proved the rule, that any operation for intussusception in infants, except laparotomy and easy reduction, is fatal. Several speakers had laid stress on the need for rapidity in operating, and he thought the revival of Murphy's button for this operation, as being absolutely the most rapid method of anastomosis, might be worth consideration, though he had never himself used it. Apart from all theoretical considerations, and judging from experience alone, the rapid operation which babies could tolerate, and the *only* operation they could tolerate, was laparotomy and easy reduction; and the treatment of cases of intussusception in babies in this way, and during the stage of the disease when this was possible, furnished the only hopeful outlook of a material reduction in its at present high mortality. Children did not always die of shock after the various operations; they sometimes succumbed to septic peritonitis or toxæmia due to absorption from the infected intestinal contents.

A Case of Idiopathic Dilatation of the Colon.

By Claude C. Claremont, M.D.

D. W., a girl, aged 13 years, was admitted into the Royal Portsmouth Hospital on March 3, 1909. The mother gave the following account of her medical history: At the age of 2 years she had whooping-cough and bronchitis; when aged 4 years she had inflammation of the bowels—namely, vomiting, diarrhœa, fever, distension and abdominal

pain. She recovered, but had a similar attack about a year subsequently, the swelling of the belly having in the interval diminished; but it increased again during the second attack. After this the belly became smaller, but it has never regained a natural size. Two or three years afterwards she had a "stoppage," during which there was no action of the bowel for three weeks. There was no vomiting, but there were attacks of pain which were relieved by passing flatus in large quantities and of offensive odour. After the bowels had acted she was taken to the Royal Hants Infirmary at Winchester, but no operation was proposed. Since this time patient has been going to school and has been in fair general health, but the abdomen has never been a natural size, and there has been a tendency to constipation. She was brought to the Portsmouth Hospital because her general health was failing.

The patient was a fairly well nourished, healthy-looking child, remarkable only for the size of her belly, which was striking. Her girth at the level of the umbilicus varied from 25 in. to 30 in., being evidently determined by a varying degree of flatulent distension. The belly was especially full on the flanks and appeared to be chiefly occupied by dilated ascending and descending colon, with a definite sulcus in the middle line. I could never satisfy myself as to the level of the transverse colon—a circumstance which was explained at the operation. The diaphragm was raised; the belly was everywhere resonant. Examination under an anæsthetic revealed nothing further. The pelvic viscera were normal. Nothing abnormal could be found in the rectum or neighbourhood of the sigmoid colon. She was tested with Calmette's ophthalmo-tuberculin on two or three occasions with a negative result. During her stay in the hospital her health, generally speaking, was fair, but deteriorated rather than otherwise. The constipation increased and required the systematic use of enemata, which acted better than aperient medicines. The temperature rose above normal for short periods. On several of these occasions the rise was accompanied by vomiting and constipation, and the patient was on the verge of intestinal obstruction.

Accordingly I asked my colleague, Mr. Childe, under whom the patient had been originally admitted, to explore the abdomen, which he did on July 5. On the belly being opened in the middle line it was found that the greater part was occupied by the large intestine, the transverse colon being folded on itself. It was directed downwards from the hepatic flexure almost to the pubes, and then upwards to the

splenic flexure, the descending portion of this bend being to the right and the ascending portion to the left of the middle line, thus accounting for the central vertical sulcus which the belly presented before operation, and for the difficulty in appreciating the direction of the peristaltic action which was sometimes visible. The whole colon was much dilated, but the transverse portion and the upper half of the descending colon were enormously so. The extreme dilatation ceased abruptly half way down the descending colon, but the remaining portion of the descending and the whole of the ascending colon were decidedly dilated. The iliac and pelvic colon and the rectum were normal. The whole of the large gut seemed thickened and hypertrophied. The small intestines were also dilated and apparently hypertrophied, but to a much less degree. After consultation it was decided to remove the transverse and the upper part of the descending colon. This Mr. Childe accordingly did, closing the cut ends of the colon and making a lateral anastomosis. The patient bore the operation well. No mechanical cause of dilatation or obstruction could be found, nor was there any sign of tuberculous or other past inflammation. The patient did well until the evening of July 8, when she complained of pain in the abdomen. She died the same evening.

Post mortem it was found that the anastomosis had given way at one point, and fæces had escaped into the peritoneal cavity. This appeared to have been due to some hard fæces which had worked its way up the ascending colon to the anastomotic opening.

I think there can be no doubt that the case I have narrated is an example of so-called idiopathic dilatation of the colon, or Hirschsprung's disease. These cases are rare. The dilatation and hypertrophy are not secondary to any recognizable mechanical obstruction, and persistent spasm or paralysis of the lower end of the gut has been invoked as a cause, but does not seem to have been demonstrated. A history of constipation can always be traced back to early childhood, if not to birth. Most of the patients die in childhood, but some reach adult and even later middle life. Although they may live on the verge of intestinal obstruction, this seldom actually occurs. The danger which is always hanging over them, and to which most succumb, is toxæmia from intestinal absorption. This is characterized by profound collapse, associated with high temperature (in some cases reaching 108° F. or 109° F.) and rapidly leading to a fatal termination. There is variation in the part of the bowel affected. In this respect the present case is exceptional. It is more common for the sigmoid flexure to be most conspicuously

dilated, forming a bag which may occupy the front of the belly, reaching to the umbilicus, or even to the epigastrium or the right hypochondrium. Fullness and impaired resonance in the left lower quadrant of the belly are significant from a diagnostic point of view. Also, although greatly distended, the belly is soft in contrast with the tension of chronic intestinal obstruction.

There is little to be said about the medical treatment of these cases. Various surgical procedures have been adopted. In Dr. Morley Fletcher's case[1] Mr. Betham Robinson opened the abdomen, and from within squeezed out the contents of the colon through the anus, and then closed the wound. Fifteen months later the patient was in good health, and presented no abnormal distension of the belly, but the rectum was ballooned. A few months later he died of toxæmia after a short illness, and after death the rectum and sigmoid loop were found enormously distended, and the rest of the colon considerably so.

I regret that our attention had not been called to this case before our operation. If the same procedure had been carried out previously to the excision, in all probability the child's life would have been saved and the operation would have been successful. The procedure obviously of itself fulfils a therapeutic indication, and I would suggest that it should form a routine part of all operations for the condition under consideration.

Other surgical operations have been tried.[2] In one of Osler's cases colotomy was followed by a good result. Treves excised the descending colon, the sigmoid, and the upper part of the rectum; and effected a junction between the left end of the transverse colon and the anus. The patient was completely restored to health. When the dilatation has been chiefly confined to the sigmoid flexure it has been excised, and the descending colon joined to the rectum, so as to form a straight line.

It is a remarkable circumstance that in some of these cases the gut at the point of junction has subsequently undergone elongation and dilatation, so as to form a new sigmoid loop which has required another operation. In some cases the surgeon has been content to make an anastomosis between the two ends of the pelvic loop with temporary success, but the patient has subsequently suffered from volvulus of the loop, and it has been necessary to excise it. One patient was operated

[1] *Brit. Med. Journ.*, 1907, i, p. 370.

[2] Hawkins, *Brit. Med. Journ.*, 1907, i, p. 477.

on twice for volvulus, first of the natural loop, and after its excision of the loop which developed at the site of the junction of the gut. Even when these excisions have been successfully carried out, the patients have not always been cured of the constipation.

Remembering the temporary success which followed Mr. Betham Robinson's operation, it occurs to one that it might be worth while, before submitting a patient to a formidable operation, to try the effect of emptying the gut as he did, and of systematically flushing it through an appendicostomy wound. I notice that Dr. Cautley, in his recently published book, indorses this suggestion, remarking that systematic lavage in cases of colitis tends to produce contraction of the colon.

DISCUSSION.

Mr. LOCKHART MUMMERY said that in his opinion it was unwise to attempt removal of part of the colon in these cases, and better to remove the entire colon and anastomose the ileum to the rectum. Colotomy was a most fatal operation in these cases owing to the bowel usually tearing away. In one case he had performed appendicostomy for this condition with the object of preventing accumulation in the enormous colon. He had been pleased with the result, as the patient, who had previously had frequent attacks of intestinal obstruction, had since the operation—a period of nearly three years—been free from these attacks.

Mr. CHILDE said he was greatly disappointed at the result of the operation. The diagnosis was not made beforehand. When the abdomen was opened the ascending, transverse, and descending colons were turned out and looked like three large footballs. The ascending colon was the least dilated, and the dilatation suddenly ceased at the ilio-pelvic colon. He excised the most dilated portion, consisting of the transverse and descending colons, measuring 36 in. in length and 12 in. in circumference. The ends were closed and a lateral anastomosis effected between the ascending and the ilio-pelvic colon. The girl at first did well, but subsequently developed peritonitis and died. A post-mortem examination revealed the fact that a hard scybalous mass from the cæcum had become impacted in the anastomotic opening, and had caused ulceration and perforation at one point. He felt that if the cæcum had been sacrificed as well at the time of the operation, and the whole of the large intestine to the ilio-pelvic colon had been excised, this accident would not have happened and the patient would in all probability have recovered.

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VOLUME THE THIRD

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1909-10

CLINICAL SECTION



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1910

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Clinical Section.

October 8, 1909.

Mr. A. Pearce Gould, President of the Section, in the Chair.

Further Report on a Case of Carcinoma of the Breast treated by Radium.

By N. S. Finzi, M.B., and Ernest H. Shaw.

The specimens exhibited this evening are from a case of carcinoma of the breast shown to this Society on May 14, 1909.[1] The patient was a woman, aged 78, whose breast was removed by operation in 1906. The disease recurred beneath the scar, and the axillary glands became enlarged; it was then treated with X-rays, with temporary improvement. On March 10 and March 12, 1909, she was given two long exposures to the rays of 50 mg. of radium bromide filtered through a thin silver tube. This was followed by another exposure on April 29, 1909, a screen of lead, 1 mm. in thickness, being used in addition to the silver. The first two exposures were excessive and caused small but troublesome burns. The tumour and enlarged gland, however, disappeared, and at the time of her death were not palpable. She was admitted to the German Hospital with a strangulated umbilical hernia, and died on July 13, 1909.

At the necropsy the whole of the mammary and axillary subcutaneous tissues was removed, together with the portion of skin affected by the burns. The sites of the mammary tumour and axillary swelling were occupied by two thin, irregular sheets of fibrous tissue; no definite nodule of growth could be distinguished in the fibrous masses, but at the edge of the larger mass two small ovoid bodies, about 1 cm. in length,

[1] *Proc. Roy. Soc. Med.*, 1909, ii (Clin. Sec.), p. 226.

were found, which felt like pieces of hardened fat. There was no sign of any growth in the mediastinum, but there were several nodules, about 1·5 cm. in diameter, in the liver, and two whitish raised patches, about 1 cm. in diameter, in the mucous membrane of the stomach. The kidneys, spleen, and other organs appeared to be normal.

Microscopic Sections.—(1) A section through the burn in the skin and the underlying tissues of the mammary region shows that the true skin is much thickened by fibrous tissue, in which there are collections of inflammatory cells and numerous blood-vessels. The epithelium ends at the edge of the ulcer, the base of which is fibrous and vascular. A thick band of fibrous tissue, free from growth, passes down from the skin into the fat and joins an irregular star-shaped mass of fibrous tissue, about 1·5 cm. from the surface; this fibrous tissue is well formed, and numerous fibrous blood-vessels are seen in it. The whole mass is densely packed with small spheroidal cells (spheroidal-celled carcinoma); the cells are mostly arranged in closely-packed rows. At the margin the carcinomatous cells pass along the fibrous prolongations into the surrounding fat. The fat-cells are separated in many parts by the cells of the growth. Blood-pigment is seen in the fibrous masses. (2) A section of another portion of the fibrous sheet mentioned above is composed of well-formed fibrous tissue, and is free from malignant growth. (3) Sections of two axillary glands: one is almost entirely occupied by growth, the cells are small and round, and the connective-tissue matrix is very scanty; the other gland is very vascular, but does not show any definite growth. (4) A section of one of the two small nodules found at the margin of the fibrous sheet shows dense fibrous tissue infiltrated with carcinomatous cells which are also passing into the surrounding fat. (5) Nodule of growth in the liver; it shows the same type of cell and a rather scanty fibrous matrix. A portion of the growth is necrotic. (6) A patch of growth in the mucous membrane of the stomach; the cells infiltrate the gland-tissue. At one place several collections of round cells are seen in spaces. (7) Site of the axillary radium burn; this had completely healed over, but the delicate epithelium was rubbed off after death. The true skin is fibrous, infiltrated with small, round inflammatory cells, and very vascular. There is well-marked hyaline thickening of the walls of the sweat-glands.

Two interpretations of the above results are possible: (*a*) That most of the cells of the growth disappeared as a result of the radium treatment, but a few remained and afterwards began to multiply, some of them being carried on to the lymphatic glands and liver. (*b*) That the

growth was still being absorbed locally at the time of the patient's death. In favour of the first suggestion it may be urged that the carcinomatous cells, which are thickly packed in the fibrous tissue, are well stained and are "creeping" along, as it were, the fibrous septa into the surrounding fat, where they are not surrounded by fibrous tissue; that the growth in the lymphatic gland is apparently recent and free from fibrosis; and that in the nodule in the liver there is very little fibrous tissue between the cells. As regards the second suggestion, we have seen in another case, shown to the Society at the same time as this one, malignant cells which stained well and did not show any obvious degenerative changes. We are inclined to support the first view and believe that if further treatment had been given it might have completed the process of absorption of the growth. The condition of the skin due to the burns, however, rendered further treatment inadvisable.

The PRESIDENT (Mr. Pearce Gould) said members would condole with Dr. Finzi and Mr. Shaw in the misfortune which overtook the patient in having a strangulated umbilical hernia. It was evident from what had been heard and the description of the slides that there was considerable fibrosis of the secondary deposits, but it was equally clear that there was still active carcinoma in the patient, which had reached the internal organs. The case, of course, would always remain an "incomplete" one.

Demonstration by the Ultra-microscope of living Treponema pallidum and various Spirochætes.

By H. W. BAYLY.

IN France, for at least a year, the ultra-microscope has been used for demonstrating in a rapid, easy, and certain manner the presence of the living *Treponema pallidum* in a papule or sore which it would be impossible on clinical grounds alone to diagnose as unquestionably syphilitic; and few, if any, Parisian syphilologists or dermatologists would venture to give an opinion on a doubtful primary or secondary syphilitic lesion without previously examining a scraping for the specific organism with the ultra-microscope. Probably this method is the most practical and certain available at the present moment for ascertaining whether a lesion of the skin or mucous membrane is syphilitic or not.

But, as with most pathological investigations, the positive result

is much more conclusive than the negative, and syphilis cannot be excluded because the *Treponema pallidum* cannot be detected on one examination. By means of the paraboloidal immersion-condenser, popularly called the ultra-microscope, the rays of light are deflected so that they converge obliquely on the object examined, which appears as a bright refractive body on a dark background. In this way very transparent objects, which are invisible by direct illumination, are easily seen and their shape and movements studied. Dr. Paul Gastou, the Director of the Central Laboratory of the St. Louis Hospital, Paris, first, I believe, pointed out the value of the ultra-microscope from a clinical apart from the purely biological point of view. For the last two years he has both advocated and practised this method for the early and certain diagnosis of syphilis, and it is largely due to his efforts that the help afforded by the ultra-microscope is now appreciated and employed by so many clinicians in Paris. I have only this week received from Dr. Gastou a little book published by him, entitled "L'ultra-microscope dans le diagnostic clinique et les recherches de laboratoire," and I have pleasure in referring to this book anyone who wishes for greater details on this subject than time permits me to give here.

The *Treponema pallidum* may be recognized by its size, shape, and movements. It appears as an extremely fine silvery spiral from 6—15 μ in length, with very regular or closely-set spirals (about seven to the diameter of a red blood-corpuscle). The spirals vary in number from ten to twenty-six, and the extremities are pointed. If the organism be so focused that only the summits of the spirals are illuminated, the treponema appears as a series of brightly illuminated dots not unlike a chain of streptococci. It is very feebly motile, its movements consisting of rotations round its long axis, backward and forward movements, and bending movements which are the most marked. It preserves its spirals during rest.

The *Spirochæta buccalis*, *Spirochæta refringens*, *Spirochæta balanitidis* are much larger, with wider and more open spirals, very much more active in their movements, and are quite unlike the *Treponema pallidum*. The only spirochætes very like the specific organism of syphilis are: (1) *Spirochæta dentium*, found in carious teeth, which is shorter (5—10 μ), five to fifteen spirals, the wave-length the same as *T. pallidum*, but depth of wave is considerably less; (2) *Spirochæta pertenuis* (yaws), Castellani; (3) *Spirochæta pseudo-pallida*, Lœwenthal (ulcerated cancers). In the last two the spirals are not quite so

deep or regular, and in the case of *Spirochæta pertenuis* the ends are often twisted into rings or loops.

The *Treponema pallidum* is found below the surface, and should be sought at the margin of the lesion. It cannot be detected in the centre of an ulcerated or necrosed area where the saprophytic spirochætes may be seen in large numbers. The organism is found in the largest numbers in mucous plaques, is constantly present in varying numbers in primary untreated chancres, and is usually detected in the papular syphilide and in scrapings from a recently removed enlarged syphilitic lymphatic gland.

The margin of the chancre, papule, or mucous plaque should be gently scraped till blood just begins to exude. The surface is now dried with a swab of plain sterile gauze, and then a little blood or serum expressed. A small drop of this is removed with a platinum needle and mixed with a drop of distilled water on a thin glass-slide. A thin cover-glass is now pressed down firmly, so that only a thin layer of fluid remains between the slide and cover-glass. A drop of immersion-oil is now placed below the slide and on the cover-glass. The condenser must first be accurately centred. This can easily be done with a low objective (1 in. or $\frac{2}{3}$ in.) by means of concentric rings scratched on the surface of the condenser.

Any artificial light can be used—electric (arc or Nernst), gas (incandescent), or even an oil lamp. Probably the Nernst is the most convenient. A bull's-eye condenser must be arranged so as to concentrate the light to the centre of the microscope mirror. After the slide has been placed in position so that there is a layer of oil between the ultra-microscope and the under surface of slide, and after the object is focused, the ultra-microscope must be racked up or down and the mirror adjusted till bright illumination with a dark background is obtained.

General or local treatment has a marked effect on the number of treponemata found, and the organisms tend to disappear after a few weeks from the site of the primary inoculation, even without treatment.

As the chief value of this method of examination is for early diagnosis (I have found numerous treponemata in a small sore with only three days' history), the effect of general treatment is not of much moment; but if the sore has been present for over six weeks, or a local antiseptic has recently been applied, no reliance can be placed on a negative result. In the absence of local treatment, a sore the

nature of which is clinically doubtful can in a few moments be proved to be syphilitic, and so much valuable time may be saved and the annoyance and discomfort of secondary manifestations prevented.

I am indebted to Dr. Gastou for the diagrams of scrapings from chancres, papules, and mucous plaques from which the lantern-slides were kindly prepared by Dr. Kerr in the Pierse-Duncombe Laboratory at St. George's Hospital; and also to Mr. J. W. Ogilvie for his kindness in lending me two complete outfits for the ultra-microscope and for coming here to-night to help me.

Case of Mycosis Fungoides.

By J. L. Bunch, M.D.

The patient, a man aged 50, noticed in October, 1908, some scaly patches on his limbs. These patches, which were itchy and irritable, increased in size and resembled eczema, except that they were very sharply defined. The whole of the trunk and limbs became gradually involved in a diffuse superficial infiltration, the pruritus increased, and softish granulomatous tumours appeared. The first one was noticed in March, on the front of the left leg, and increased gradually in size until, according to the patient's account, it became as large as a cricket-ball. Numerous other tumours appeared, the largest being on the limbs. When he first came to University College Hospital, on July 23, 1909, the trunk, arms, and legs presented numerous well-defined infiltrated patches, varying in size up to that of a hand or larger, and of a reddish colour. These patches were dry and very scaly; so scaly, in fact, that by moderate friction it was possible to collect a small pile of scales. These lesions resembled a dry eczema, except for this very sharp definition. There was very troublesome pruritus in the region of these patches. There were numerous tumours on the limbs, flattish, convex in shape, reddish in tint, softish to the feel, slightly raised above the surface, and of various sizes up to a diameter of about 3 in. Some of these tumours were breaking down in the centre, and one on the back of the left arm had a typical fungating, mushroom-like appearance. This tumour was about $1\frac{1}{2}$ in. in diameter and projected $\frac{1}{2}$ in. from the skin, spreading out at the top from its pedicle, which was of smaller diameter. The secretion from the ulcerated surface was of a yellowish, purulent nature, but not particularly offensive. The trunk did not show any definite tumours,

but numerous softish infiltrations of a reddish or purplish tint. The primary tumour on the left leg, which was said to have been the size of a cricket-ball, had apparently undergone some involution, for it did not then project more than ½ in. from the skin, and was flattish rather than round. The patient said that since the onset of the disease he had been very nervous, could not sleep or work, and was very tremulous. On examination, the hands and tongue showed marked tremor; but the knee-jerks were present, his pupils reacted, and there seemed to be no loss of sensation. The nervous symptoms appeared to be functional, but it was not clear whether they could be ascribed to his work. For twenty-five years he had been employed in the generating station of a large electric-light plant, but stated he had



Mycosis fungoides.

never had a severe shock from contact with a dynamo or brush. In spite of this, however, he was now nervous of handling the machinery.

His father is alive, aged 82, and his mother died when 78 years old.

He was admitted to University College Hospital on July 28, 1909. The fungating tumour was treated with boracic fomentations, and he was ordered alkaline baths three times a week and a calamine cream to be applied continuously to the infiltrated areas. Intramuscular injections of arsacetin have been given during the whole of his stay in hospital, at first in doses of 7 gr. per injection, and afterwards in doses of 3½ gr. These injections were given three times a week, and he has had in all 87½ gr. injected without causing anything more than

slight discomfort at the time of injection. He has also had regular treatment by X-rays and the constant current applied occasionally to his spine.

Within a month of his admission to hospital the tumours showed considerable diminution in size; they became flatter, and, as involution proceeded, the skin covering them became more deeply pigmented, until it became a dark brown as the tumours finally disappeared. The limbs now show many darkly pigmented areas, marking the sites where tumours have been. The fungating tumour on the left arm also became gradually smaller, until it was finally level with the skin, and the ulcer healed up, leaving a well-marked white scar surrounded by a narrow chocolate-coloured circular band. About six weeks ago the moustache area became erythematous, infiltrated, scaly, and so irritable that it prevented sleep. A tumour then developed, about the size of a hazel-nut, an inch above the left angle of the mouth; but this is now much smaller, being almost level with the skin, and is darkly pigmented. Two recent tumours of smaller size are also present at the back of the neck; they are still red, but are getting smaller. A month ago some follicular lesions appeared on the scrotum, spreading irregularly down the thighs, and also on the wrists. These lesions differed entirely from the original mycosis tumours in that many of them developed yellow heads, were obviously pustular, and on culture showed the presence of *Staphylococcus aureus*.

Sections of the tumours showed hyperacanthosis with elongation of the papillæ, infiltration of the papillary body with lymphocytes and some small plasma-cells in a fine connective-tissue network. It is noteworthy that sections of apparently healthy skin taken at some distance from the infiltrated areas also showed these changes, though in a less marked degree. The histological appearances point rather to the lesions being of the nature of a cutaneous lymphadenitis than a variety of sarcoma.

Dr. F. DE HAVILLAND HALL in 1895 had shown at the Clinical Society[1] a case in which the tumours had undergone very extensive ulceration, and did not respond to any treatment. The pharynx and larynx were affected; this, he thought, was unusual in mycosis fungoides. The patient, who attended the hospital, drifted away and was lost sight of; when he left the hospital, he seemed to be going downhill. The sections of the tumours were made by Dr. Hebb. No animal or vegetable organism was found.

[1] *Trans. Clin. Soc. Lond.*, 1895, xxviii, p. 268.

Pigmentation of the Circum-oral Skin and of the Buccal Mucosa in Pernicious Anæmia.

By H. D. Rolleston, M.D.

Male, aged 25, but looking much less, was admitted July 19, 1909, for diarrhœa, anæmia, and enlargement of the liver and spleen. There was not any evidence of congenital syphilis, and Wassermann's reaction (Dr. H. W. Bayly) was negative. There was marked freckling of the lower half of the face, especially around the mouth. The lower lip presented inky pigmentation along the line of contact towards the angles of the mouth. On the oral surface of the right cheek there were one large and four small pigmented patches, and on the mucosa of the left cheek there were two similar pigmented patches. Inquiry showed that the patient had not taken arsenic before admission; this supports Dr. Hale White's contention that oral pigmentation may occur in pernicious anæmia independently of arsenical treatment.[1] The patient has greatly improved since he was put on arsenic.

Blood.—The number of red blood-corpuscles has varied from 1,515,000 to 3,015,000, the percentage of hæmoglobin from 40 to 76, the colour-index from 1·17 to 1·5, the number of leucocytes from 2,960 to 4,000 (polymorphonuclears 40 to 64, lymphocytes 34 to 56). Only one nucleated red corpuscle has been seen. The arterial blood-pressure is 100 Hg.

The question arises whether the spots of pigmentation on the buccal mucosa are of the same nature as the freckles around the mouth, as in the case of the twins figured by Sir J. Hutchinson,[2] to which my attention has been drawn by Dr. Galloway, or whether the pigmented spots on the buccal mucosa are associated with the anæmia. The first alternative seems the more probable. There is no reason to think that the patient has Addison's disease.

DISCUSSION.

Dr. Parkes Weber suggested that the condition might be one of incomplete xerodermia pigmentosa. The distribution of the pigment on the hands and face was very striking, and the skin on the hands had an atrophic appearance.

[1] *Clin. Journ.*, Lond., 1907-8, xxxi, p. 355.

[2] *Arch. Surg.*, Lond., 1896, vii, p. 290.

Sir DYCE DUCKWORTH, in answer to the President, said he had not been familiar with pigmented patches on the buccal membrane in pernicious anæmia. He was not aware of it having been described in any text-book, and he could not recall an example of it. He thought that in this case there must be some relationship between the peculiar freckling and the pigmentation of the buccal membrane.

Excision of both Maxillæ.

By ALBERT CARLESS, M.S.

T. B., AGED 49, was admitted into King's College Hospital on June 15, 1908. No family or personal history of importance. He had complained of inability to breathe properly through the left nostril for some years, but had only noticed a swelling on the left side of the face for three months. There was no pain, and the swelling increased in size pretty rapidly. Latterly he had complained of overflow of tears from the left eye. He looked robust and healthy. The left cheek was much swollen, and on examination a tumour could be felt overlapping the lower border of the orbit, extending behind to the malar bone and masseter, and involving the alveolus and palate. The teeth on this side were bad and freely movable. The swelling was firm and elastic, not adherent to the skin, not tender or painful, and without pulsation or fluctuation. The palate was depressed on the left side and elastic to the touch. The posterior nares appeared to be blocked. There was not any glandular enlargement. Dr. StClair Thomson reported as follows: "The left nostril is quite occluded, but after the use of cocaine no growth or polypus is visible. There is no growth in the post-nasal space; but traces of pus appear in the left choana. He complains of occasional cacosmia. Transillumination shows neither side very translucent; but the left is the darker, and whilst there is some subjective sensation of light to the right eye, there is none to the left." A diagnosis of tumour of the superior maxilla growing from the upper part was made, and it was thought to be probably simple in nature. The left superior maxilla was removed on June 19; this operation included a portion of the malar bone, the floor of the orbit, the nasal process of the bone, and ramifications of the growth which had extended in various directions. The mass was practically devoid of bony tissue; the antrum was nearly obliterated. On microscopic examination it was found to consist of columns of epithelial cells

with commencing foci of liquefaction; in other words, it was the so-called fibro-cystic disease, or epithelial odontoma.

The patient did well, but returned in November of the same year, and it was then found that the other side was becoming similarly involved, especially the palate and alveolus. The right maxilla was removed on November 6, but the orbital plate was left. Since that date the patient has continued well and free from any signs of recurrence. He has been fitted with an artificial palate by Mr. Alfred Hart. The appliance consists of an aluminium plate carrying teeth, and secured to the tissues in the roof of the nose by three rubber suckers. Springs from small tooth-plates carried by the mandible also assist in keeping it in place. The patient can masticate soft food, such as bread and butter; can smoke a pipe, and complains of but little inconvenience, except from the diplopia which results from the dropping of the left eyeball.

DISCUSSION.

The PRESIDENT (Mr. Pearce Gould) said he had not had such a case, but some years ago Mr. Farnell, of Eastbourne, showed a patient from whom he had removed both superior maxillæ for sarcomatous disease, with a very admirable result as regards the appearance of the patient. He had not heard what was the ultimate issue of the case. The deformity resulting from such an operation was decidedly small. The congratulations of the Section were due to Mr. Carless, and also to Mr. Hart for his skill in fitting a denture to such a difficult case. The pathological aspect of the case was interesting in the different structure of the tumour on the two sides. He hoped Mr. Carless would keep in touch with the patient in view of possible further developments.

Mr. W. G. SPENCER said he had had a patient who lost both sides of his upper jaw from syphilis, and it was very difficult to get an artificial upper plate to keep in position, but finally, by means of a curved spring between it and the lower plate on each side, the man was made comfortable.

Meckel's Diverticulum containing Calculi and producing Colic.

By JAMES SHERREN, F.R.C.S.

F. T., A BUTCHER, aged 38, was admitted to the medical side of the London Hospital on March 22, 1907, with the following history: For two and a half years he had suffered from attacks of pain in the right iliac region, griping in character and of sudden onset. At first these

were considered to be "appendicular" colic, but in November, 1906, after a severe attack of pain which confined him to bed for the first time, he passed "gall-stones in his motions," and the diagnosis was changed to "biliary" colic. Seven days before admission he had another severe attack, accompanied for the first time by vomiting. On admission there was slight rigidity and definite deep tenderness in the right iliac region; his temperature and pulse-rate were normal. The diagnosis of subacute appendicitis was made, and he was discharged to convalescent home fourteen days later.

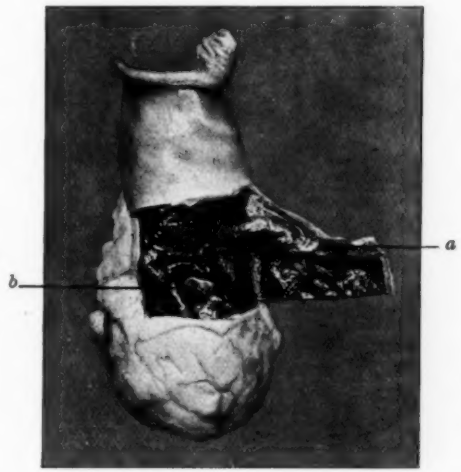
He was admitted under my care on May 9, 1907, after several severe attacks of pain. Ignoring the history of the passage of gall-stones, I considered the attacks due to disease of the appendix. There were no dyspeptic symptoms, the pain had never been above the level of the umbilicus, had never passed to the back or to the right shoulder, but had been localized to the right iliac region and had been accompanied by rigidity of the muscles of this region and deep tenderness.

I operated on May 16. On opening the abdomen, I felt what appeared to be an enlarged gall-bladder full of stones. On bringing it to the surface its correct nature at once became evident. It sprang from the antimesenteric border of the ileum about three feet from the ileo-cæcal valve, at its commencement being of the same size as the gut from which it sprang, gradually dilating to its extremity. Its outer surface was smooth and free from adhesions. The termination of the superior mesenteric artery ran across the ileum in a fold of peritoneum to end on the diverticulum. I removed the diverticulum and sutured the opening in the intestine. The patient left the hospital on June 6, and I saw him in perfect health two years later.

The specimen, which is in the London Hospital Museum, measured in the recent state 4½ in. in length (*see* fig.). One inch from its attached end its lumen was almost obliterated by a fold of mucous membrane which surrounded it; the opening would just admit the tip of my index-finger. Externally there was no constriction or sign of its presence. Below the valve it was closely packed with brittle, blackish calculi. Mr. Candy, analyst to the Hospital, reported that a specimen consisted of cholesterin, calcium oxalate, and a trace of bile-pigments.

After a careful search, I have been unable to find a record of a similar case. Calculi are rarely met with in Meckel's diverticulum. I have been able to find four cases recorded in which the diverticulum contained calculi; these were called "gall-stones" in two of the cases. Beale [1] at the necropsy of a boy, who had died of peritonitis, found a perforated

diverticulum of the ileum containing "a cherry-stone, the coriaceous covering of several orange-pips, and two masses of very irregular form and tough consistence, resembling the concretions often found in the intestines of horses." Galeazzi [2] found a concretion in the purulent contents of an inflamed diverticulum. Hollander [3] reported the case of a man operated on ten hours after the onset of what was supposed to be appendicitis. A gangrenous Meckel's diverticulum was removed filled with gall-stones, faceted and composed of cholesterin. The patient made a good recovery. Znojemsky [4] operated on a man, aged 18, three days



Photograph of Meckel's diverticulum, with window to show valve and calculi. *a*, valvular fold of mucous membrane surrounding lumen; *b*, calculi.

after the onset of acute intestinal obstruction, and found a Meckel's diverticulum containing a "gall-stone" incarcerated in the outer retrocæcal recess.

Hollander's is the only case in which we are definitely told the composition of the calculi, and in this, as in Znojemsky's case, I do not know on what grounds the calculi are called gall-stones. Both in Hollander's and my case the calculi were composed of cholesterin. In my case, I believe that the stones were formed in the diverticulum as the result of inflammation of its mucous membrane. They could not have reached it

from the gut, as the opening was too small to admit of the passage of most of the calculi.

In conclusion, I think it would be quite possible to diagnose a future case of this nature. Symptoms of abdominal colic, the pain and rigidity being situated in the right iliac region, followed by the passage of calculi in the motions can be considered as pointing to the presence of a Meckel's diverticulum filled with stones, and should lead to operation without delay.

REFERENCES.

[1] Beale. *Trans. Path. Soc.*, Lond., 1850-52, iii, p. 366.
[2] Forgue et Riche. "Le Diverticule de Meckel," Par., 1907, p. 188 (original paper unavailable).
[3] Hollander. Loc. cit., p. 188 (original reference not given).
[4] Znojemsky. *Zentralbl. f. Chir.*, Leipz., 1907, xxxiv, p. 1107.

DISCUSSION.

Mr. Carless reported a somewhat similar case in a young man, about 23 years of age, whom he had operated on a few months back. He was a clerk, and had never had any illness of importance—certainly nothing abdominal. He was seized with severe abdominal pain about midday, referred to the right side, and was subsequently sick and collapsed. He was admitted to hospital and operated on within eighteen hours of the onset. The abdomen on examination was held rigid and tense, but less so on the left side than on the right. The point of maximum tenderness was somewhat above McBurney's spot, and it was supposed that there was either a perforated stomach or appendix. On making the usual appendix incision and displacing the omentum, a red swelling as large as a hen's egg was encountered, lying in front of the ascending meso-colon. One end was practically free; the other was connected by a narrow neck to a diverticulum originating from the ileum. The diverticulum was about 1 in. long; the distal inflamed pouch was $2\frac{1}{2}$ in. in length and about $1\frac{1}{2}$ in. in breadth. There was a good deal of inflammatory trouble around, and therefore the pouch was removed on the proximal side of the constriction and the more normal portion of the diverticulum left—i.e., a portion about $\frac{3}{4}$ in. long. A wick of gauze for drainage was introduced, and the abdominal wall above and below it closed. The patient did perfectly well, and is now back at work. The interior of the pouch was lined with mucous membrane in a state of intense inflammation, and indeed was almost gangrenous. Within the pouch was a calculus, looking like a mulberry calculus of the urinary tract, but quite light in weight. It and the pouch were shown, and Mr. Carless promised to have a chemical examination of the calculus made, so as to ascertain whether or not it was of biliary origin.

The PRESIDENT (Mr. Pearce Gould) said the condition must be very rare, otherwise there would have been more than the few cases Mr. Sherren had been able to find recorded out of the very large number of "appendicitis" cases which had been operated upon in the last twenty years. It was interesting that Mr. Carless's case resembled Mr. Sherren's in the narrow communication between the proximal part of the diverticulum and the pouch in which the stones were found. In neither case was there a demonstration of the condition of the gall-bladder, so that there was no information as to whether there was a gall-stone in it, nor was the nature of the concretions within the diverticulum definitely ascertained.

Dr. GARROD thought that it should be possible to ascertain, by examination of the calculi, whether or no they were gall-stones. The appearance of the calculus in Mr. Carless's case certainly suggested that it was a gall-stone, and if its colour were due to bilirubin-calcium, it could hardly be of any other nature. On the other hand, the trace of bile-pigment in Mr. Sherren's case might well occur in an intestinal concretion. The formation of a cholesterin calculus in an occluded or nearly occluded Meckel's diverticulum was not at all improbable; for collections of cholesterin may form in any cavity lined with mucous membrane, when it is the seat of chronic inflammation.

Mr. SHERREN replied that he explored the gall-bladder and found nothing the matter with it; there was no evidence of stone.

A Case of Rheumatoid Arthritis with Enlargement of Lymphatic Glands and Spleen.

By E. I. SPRIGGS, M.D.

THE patient is a lad aged 16, a farm labourer. One of his sisters died of whooping-cough; all the other members of the family are alive and well. The patient has not had any other illness. When he was aged about 10 his mother noticed that the proximal joints of the fingers began to enlarge; the ankles, elbows, and wrists became swollen later. He says that he did not feel any pain or discomfort until two years after the appearance of the disease, when he was 12 years old. The condition became gradually worse, but did not prevent him from going to school or work, and a doctor was not consulted until ten weeks ago. He then complained of pain in all the affected joints, especially in the feet and left elbow. The pain was sometimes darting, sometimes aching, and was worse at night. After a fortnight in bed it was very greatly

relieved. The patient was admitted to hospital twenty-five days ago, complaining of the pains; he was flushed, and did not look well.

The pulse-rate has usually been between 92 and 104 in the last three weeks, having been lower than 92 on four occasions only. The temperature is frequently 99° to 99·5° F. in the evening. There is uniform enlargement of the phalangeal joints of the fingers and toes, including the thumbs, the distal joints being affected as well as the proximal.



Fig. 1.

Redness was present on admission, but has now disappeared. The elbows, wrists, knees, and ankles are uniformly swollen (fig. 1). The vertebral, temporo-maxillary, and sterno-clavicular joints have escaped. The lymphatic glands of the axilla are enlarged, soft, discrete, and not painful. Each epitrochlear gland is of the size of a large hazel-nut, and soft. The glands in the groins are smaller, about the size of a small hazel-nut, and harder. Some small glands may be felt deeply

placed above the clavicles. The spleen can be felt an inch below the costal margin when the abdomen is relaxed. Its area of dullness is increased, extending forward beyond the anterior axillary line and upward to the seventh rib. The urine on admission contained 0·5

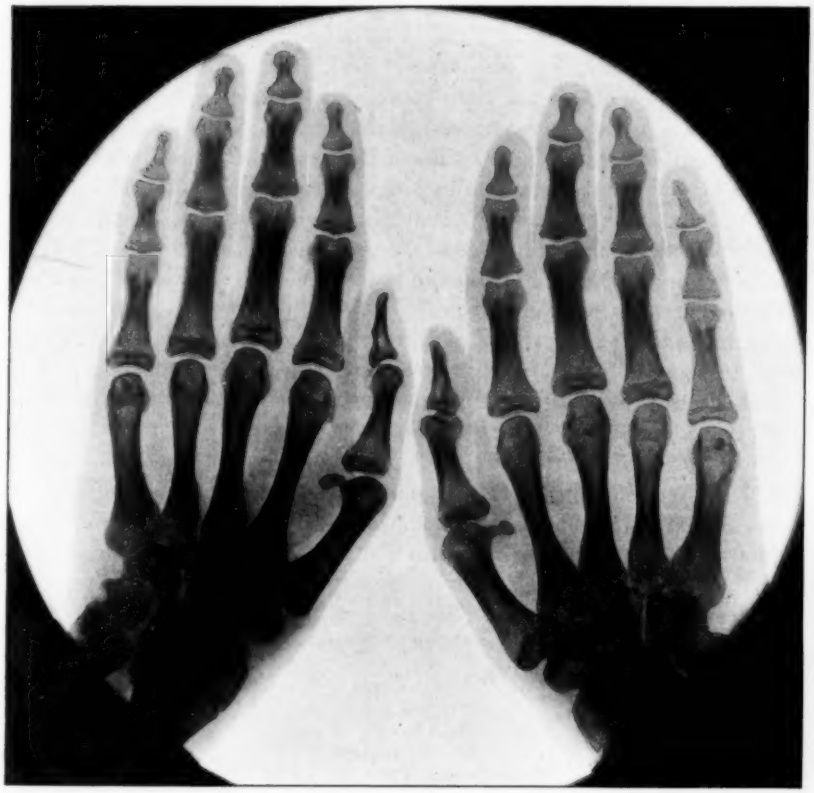


Fig. 2.

per mg. of albumin. The other abdominal viscera, and those in the thorax, present no abnormal signs. The bowels were open daily without medicine when he was not in bed; no mucus has been noted in the stools. Some teeth are decayed. Mr. Frank Morley, however, reports that there is not sufficient evidence that the arthritis is due to septic

infection from the mouth. The examination of the blood (Dr. Spitta) gave 4,660,000 reds, 7,600 whites, hæmoglobin 66 per cent. The differential count showed 59 per cent. of polymorphonuclears, 29·5 per cent. of lymphocytes, 2·5 per cent. of mononuclear cells, and 9 per cent. of eosinophile cells. The opsonic index for tubercle was 1·04, to the pneumococcus 1·14, to the *Streptococcus pyogenes* 1·09. Calmette's and von Pirquet's tests were negative. The joints were examined by X-rays. Dr. Simmons reports that "the only noticeable peculiarity about the joints is their abnormal translucency, due probably to a diminution in the percentage of opaque bone salts. Such an appearance is seen in cases in which for any reason the limbs are not used. There are also some small excavations at the sides of the bases of the metacarpal bones. These are frequently met with in both osteo-arthritis and gout" (fig. 2).

The treatment in the hospital has consisted so far of rest in bed and a mixture containing iodide of iron and nux vomica. There is definite improvement, the pain and redness having disappeared, and the urine being now free from albumin.

The case is a characteristic one of rheumatoid arthritis showing the enlargement of glands which is frequently associated with this condition in children (Still), but which has been recorded in many cases in adults (McCrae, Garrod, Llewellyn Jones), including one of a man aged 25, whom I brought before the Section last year,[1] who was also the subject of exophthalmic goitre. Enlargement of the spleen is frequent in children (Still), but in rare instances has been observed in adults (McCrae). As is well known, children and young people respond more easily to infective processes by enlargement of the glands and spleen than is the case in adults. The case is therefore an example of an intermediate stage between chronic infective arthritis as met with in childhood and in adult life.

DISCUSSION.

Dr. GARROD expressed the belief that there was not a definite disease confined to children which might be called Still's disease, and another disease, with similar articular lesions which was developed only in adult life, but that, as Dr. Spriggs suggested, the enlargement of the spleen and glands, which was occasionally seen in adults as well as in children, was an example of the influence of the age of the patient upon the clinical features of a malady. In true rheumatism such an influence was very conspicuous. His own experience

[1] Vide *Proc. Roy. Soc. Med.*, 1909, ii (Clin. Sect.), p. 49.

led him to think that when larger numbers of cases were studied the enlargement of the spleen and glands was not so common in child sufferers, even during exacerbations of the disease, as was the case in the examples on which Dr. Still based his most valuable paper.

Dr. ROLLESTON said that twelve years ago, at Cambridge, he saw a case of the same nature in a man aged about 30. Before Dr. Still's paper was read before the Medical and Chirurgical Society the same condition of chronic infective arthritis, with enlargement of the corresponding lymphatic glands, had been described in adults by Chauffard and Ramond,[1] who obtained from the joints and from the glands a diplo-bacillus which they regarded as being both the cause of the peri-articular trouble and the gland enlargement. The term "Still's disease" was applicable to the infantile or childish form.

Sir DYCE DUCKWORTH said he had always considered the cases described by Dr. Still as presenting two separate diathetic conditions—a blend or association of struma with the arthritic or rheumatic habit of body. Blends frequently occurred, and one expected to meet with them more frequently than one did.

Dr. SPRIGGS replied that some active treatment in the shape of radiant light or heat had been suggested, but the patient was doing so well that this had not been begun yet. In answer to Sir Dyce Duckworth's remarks, he (Dr. Spriggs) was inclined to regard the enlargement of glands as directly connected with the joint condition, as the epitrochlear glands especially were markedly enlarged, and this was an uncommon situation for glandular enlargement associated with tuberculosis.

Case of Flexor Spasm.

By G. A. SUTHERLAND, M.D.

FEMALE, aged 5. In June last the left hand was noticed to be held in a cramped position and not used. Two days later the child had a sort of fit, becoming faint and of a dusky colour, but did not lose consciousness. There was found on examination to be flexion of the fingers of the left hand into the palm. The ring and middle fingers were chiefly affected; the others could be easily extended, and even hyper-extended, by passive movement. If these two fingers were straightened out forcibly the wrist at once became strongly flexed; if the fingers and wrist were straightened, the elbow became flexed. The

[1] *Rev. de méd.*, Par., 1896, xvi, p. 345.

hand was usually deviated to the ulnar side. The grasp of the left hand is good. No alteration in the reflexes or electrical reaction. The condition has persisted unchanged. The contraction is not absent during sleep or under an anæsthetic.

Detachment of the Tendon of the Subscapularis Muscle for Recurring Dislocation of the Humerus.

By W. G. Spencer, M.S.

Male, aged 24, employed in a boot store, has had epileptic fits from childhood; always whilst asleep, without any aura, but has malaise on awaking. Since the middle of 1908 he has had recurring dislocation of right humerus into axilla; found dislocation on awaking, and had to seek doctor to reduce it. The left shoulder has been dislocated once or twice. In April, 1909, an anterior incision was made into the right shoulder-joint; nothing abnormal was discovered; tendon of subscapularis cut through at its insertion. Healing without any diminution of shoulder movement. Had a short, severe fit on three nights after operation; nurse had to insert gag. After leaving hospital has had a fit about once a fortnight. No dislocation of right shoulder since operation. Left shoulder has been dislocated twice. He is in regular light work.

Case of Schlatter's Disease.

By Gordon Taylor, M.S.

G. S., female, aged 13, attended the out-patient department of the Middlesex Hospital for a painful swelling at the upper end of the right tibia. For twelve months she had been conscious of an undue sensitiveness at the upper end of this bone, and she was unable to kneel upon this leg. She could walk, but she had pain when she bent her knee to a right angle. On examination it was found that she had a very tender swelling over the tubercle of the tibia at the point of insertion of the ligamentum patellæ. She had been treated at a dispensary with

blisters, iodine and friction, but unavailingly, and the patient thought that the swelling was larger and certainly more painful. A skiagram showed a widening of the space between the descending tongue-like process of the tibial epiphysis and the shaft of the bone. In addition, the anterior surface of the process appeared to be irregular, as if due to chronic periostitis. Treatment by fixation of the knee in plaster was proposed.

A Case of Gout, with large Tophaceous Deposits, in a Mahommedan.

By Sir DYCE DUCKWORTH, Bt., M.D.

THE notes of this remarkable case have been kindly furnished to me by Captain H. H. G. Knapp, M.B. Oxon, Indian Medical Service, as reported by Dr. Harilal Basu, assistant surgeon, General Hospital, Mandalay.

Shiek Abdool, aged ? 30, Mahommedan, a cook, was admitted to hospital in May, 1907, suffering from hard, nodular swellings in both hands and feet, which began about a year previously. His father was a jemadar in a native regiment at Cawnpore. Both parents died during his early years. He had four sisters; he left his family to earn his living when aged 17. He began his career as a cook to a European gentleman, was married when aged 18, and has worked as a cook ever since.

His diet consisted of wheat-flour chupattees, rice, fish, mutton, potatoes, green vegetables, and dhall. He became addicted to various liquors—rum, whisky, and beer—mostly rum. When aged 18 or 19 he suffered from pain in the wrist-joints, unattended with fever or swelling, lasting for one or two weeks at a time. These attacks occurred every two or three months; other joints subsequently became involved. Various native remedies were employed without any benefit, and he then sought advice from various Burmese practitioners. While under their care, and, as the patient believed, as a result of their treatment, the swellings on the hands and feet appeared, and continued to enlarge up to the time of admission to the hospital. These were found to consist of uratic tophi. Many deposits of urates were met with over the elbow-joints, in the skin of the thighs and calves, and on the dorsum of both feet, some being large and movable. The largest mass was on the right

forefinger, and one was over the tuberosity of the left tibia. The elbow-joints were enlarged, and movements of both arms were limited. The ankle-joints were also enlarged, and both feet were everted. The hands were much crippled, both by changes in the joints and masses of uratic deposit. The urine was highly albuminous. He was treated with iodide of potassium and salicylate of sodium.



Tophaceous gout in a Mahommedan.

This condition is certainly rarely met with in Oriental practice. Gout in any form is seldom witnessed amongst Mahommedans anywhere, and probably only in those who break the laws of their religion in respect of diet. This patient had probably been a large meat-eater and a free drinker of alcohol. He appeared to have suffered more pain than is usual in cases where the tophaceous deposit is large; some of these cases occur without any of the ordinary pains of acute gout, even when

enormous masses of urate-deposit are thrown out. They are classed amongst examples of atonic gout, and may occur in early life leading to a feeble and cachectic state. The urine is sometimes fairly natural in such cases, but may contain sugar, as happened in one instance which came under my observation, and in which a fatal issue followed a large anthrax on the nape of the neck.

A Case of Gout, with Uratic Tophi, in a Boy aged 14.

By Sir Dyce Duckworth, Bt., M.D.

I am indebted to Dr. H. Aldersmith, Medical Officer of Christ's Hospital, Horsham, for the particulars of this noteworthy case.

A. B., aged 14, was admitted to the sick ward on January 30, 1909, for pain in his left great-toe-joint, which was swollen, hot, and tender. On January 31 the right great-toe-joint was also painful but natural in appearance; temperature, 101·8° F.; pain was worse at night. February 1: In same condition; temperature, 102° F.; both great-toe-joints swelled. February 2: Temperature, 102° F.; typical gout. February 3 and 4: Joints red, shining, veins full; temperature, 100° F., and never rose, even at night, subsequently. February 6 to 11: Swelling continued, especially under the ball of each toe, angry-looking, dark red in colour. February 14: Pain passed off, both feet tender on standing; feeling quite well. February 19: Skin desquamating. February 21: The right toe-joint more swollen, red and tender, with turgid veins. February 25: Swelling increasing under the right toe. February 27: Fluctuation felt, fluid appearing to point, suggestive of pus. March 4: After a consultation, it was decided to puncture this swelling and let out this fluid with all aseptic precautions. March 5: 2 dr. or 3 dr. of straw-coloured fluid were removed, no pus, but consisting of a saturated solution of sodium urate. The wound was closed, but continued to discharge a little into the dressings till March 15, when a little yellow substance was observed in the wound; this was now opened up, and a small mass of sodium urate was removed; fluid still continued to exude, and the swelling subsided, the parts resuming a natural appearance. Two weeks later a small tophus was noticed in the left foot; this caused some pain. A small aperture over the mass gave vent to pus,

so the tophus was cut out and the wound washed out and plugged with gauze.

This boy's great-grandfather was a so-called "martyr to gout." There was no history of any previous gouty attacks, and there had not been any injury to either foot.

The school dietary allows of one meat meal only each day, and no articles likely to produce gout.

Dr. Aldersmith has, he tells me, had from 700 to 800 boys annually under his care for thirty-nine years, and never met with a similar case. He has met with one case of gout in a boy and one in a young girl many years ago.

The question arises in a case of this kind as to the best diet to enjoin for the future. I should not hesitate to recommend a wholesome mixed diet, with a view to secure the highest level of general nutrition.

DISCUSSION.

Dr. PHILIP G. LEE (Cork) said he could recall six cases of acute gout in boys between the ages of 9 and 12 years in Ireland, the patients being poor and therefore not too well fed.

Sir DYCE DUCKWORTH, in reply, said he thought it was well known that true gout was almost unknown among the peasantry and the lower orders in Ireland. He had himself lived in Ireland a good deal and studied the habits of the people there, and had found that gout was not known. It was at one time seen among the butlers of wealthy people in Dublin, but even in Dublin the condition was very rare. The Professor of Pathology in Vienna, a good many years ago, wrote to the Professor of Pathology in Dublin asking him to send some specimens of bones which were encrusted with urates, as there were none in Vienna; but the Dublin Professor replied that that was the last city to send to for such specimens—that they were much more likely to be found in London. When it was said that gout was met with in young Irish children, he could only reply that he was sceptical, and thought some mistake in diagnosis must have been made. The consumption of porter was, however, now very large amongst the lower orders in Ireland, and it was not unlikely that in course of time gout might become prevalent amongst them.

Two Cases of Lobar Pneumonia, with considerable Displacement of the Heart.

By W. P. Herringham, M.D.

We diagnose pneumonia partly by the physical signs and partly by the symptoms which it presents. If these are typical the diagnosis is easy, but in a large number of cases they are not. It is the physical signs which vary most, and in consequence the diagnosis has frequently to be made chiefly on the symptoms. But though it is comparatively common for the vocal vibrations to be absent, for the percussion note to be almost, if not quite, as flat as that of pleural effusion, and for the breath-sounds to be weakened instead of undergoing the changes typical of pneumonia, it had never been my lot until this year to observe along with these variations from the type such a displacement of the heart as would confirm the diagnosis of pleural effusion. Such displacement is not of course unknown; it is mentioned by Musser and Norris in their article in Osler and McCrae's "System" (1907, vol. ii, p. 624). But, judging from my own experience and that of others to whom I have spoken, it must be so uncommon that marked instances of it may be worthy of record.

Case I.—H. B., aged 41, a big van-driver, was admitted on February 13, 1909. He had had pneumonia fourteen years ago; he had been a heavy drinker of beer. He was admitted on the sixth day after a typical onset. On admission he was flushed but not cyanotic. He had typical pneumonic sputum. The signs of pneumonia were present over the whole of the left lung, and the only variation from the type was that in the axilla; the breath-sounds were weak. The heart's apex was ½ in. outside the left nipple line, and the right border 1½ in. to the right of the middle line. The leucocyte-count showed only 5,000. February 16: The left side was bulging; vocal vibration absent at the base, the percussion note quite flat, and the breath-sounds absent there. At the apex of the lower lobe there were bronchial breathing and bronchophony. The heart's apex was ½ in. inside the nipple line, and the right border ½ in. internal to the right nipple. The sounds were clear; I explored, but found no fluid. He died the next day. At the necropsy, for the notes of which I am indebted to Dr. Langdon Brown, the heart (14½ oz.) was found to be pushed over so far that the apex was almost

in the middle line. The muscular tissue was good and the valves natural. There was slight dilatation of the right side. The left lung weighed 93 oz. The upper lobe was emphysematous, the lower lobe massively consolidated, with red hepatization in the deeper and grey in the superficial parts. There was a thick layer of lymph over this lobe, but no effusion. The right lung weighed 24 oz. It was emphysematous and engorged.

Case II.—W. M., aged 25, was admitted on April 25, 1909. He had had no previous illness. He was a heavy drinker of beer. He was admitted on the fourth day after a typical onset. On admission he was pale and looked very ill. He had characteristic pneumonic sputum. There were typical signs of pneumonia over the right lower lobe. From carelessness the position of the heart's apex on admission was not written down at the time. April 28: Over the right lower lobe vocal vibration was not to be felt; the breath-sounds were very weak everywhere except at the apex of the lobe, where they were bronchial. May 1: The heart's apex was ½ in. outside the nipple line. The breath-sounds in the axilla were almost inaudible. May 3: The leucocytes were 16,000. The heart's apex was 1½ in. outside the nipple line. Over the whole of the right back, and over the front up to the third interspace, vocal vibration was absent, the percussion note was stony, and the breath-sounds inaudible. A paravertebral triangle of dullness was made out on the opposite side, whose base was 2 in. wide, but whose upper limit could not be exactly defined. I explored in the seventh space and withdrew a little viscid fluid, which showed pus-cells under the microscope. This, as appeared from the event, was from the lung itself. Under a local anæsthetic an incision was made. No pus could be found, but at the end of his finger the operator felt what he took to be a thick pleura with fluid on the other side of it. This was, as was afterwards found, the lung itself, but it did not feel like a solid body. He accordingly pushed his knife into it, in hopes of reaching fluid beyond. The patient was none the worse for the operation, but had been desperately ill before, and died the next day. At the necropsy, for the notes of which I am indebted to Dr. Horder, it was found that there was a marked skoliosis, the thoracic curve being to the left with its maximum point opposite the tenth dorsal vertebra. The heart (12 oz.) lay almost wholly to the left of the middle line. All its chambers were dilated. The myocardium and the valves were natural. There was actually a superficial dent made by the heart upon the soft tissues of the thoracic wall. The right lung (83 oz.) was extremely massive, distending the

right side of the thorax, pressing down the liver, and pushing the mediastinum over to the left. It was consolidated throughout, grey on section, and very soft. A thick, grumous fluid exuded from the bronchi, which were full of this material and considerably dilated. The left lung (19 oz.) was emphysematous and engorged. In addition, the right kidney was rudimentary, and its place was occupied by the left kidney, which weighed 18½ oz. The left ureter crossed the lower lumbar vertebræ diagonally.

The patient was always too ill to sit up, and the skoliosis had not been noticed during life. Possibly the presence of Grocco's sign depended upon the deformity. But I think it more probable that the semi-diffluent lung bulged over to the left behind when the patient lay on that side, much as it bulged in front.

Both cases, it will be noticed, occurred in drunkards.

DISCUSSION.

Dr. WILLIAM EWART looked for some explanation for the strange circumstance that a condition which, judging from Dr. Herringham's experience, ought to be fairly common, should have hitherto escaped the notice of careful observers. A distended appearance of the hepatized lung grooved by the ribs was familiar to all; but he did not remember any instance of this encroaching hypertrophy. He was therefore led to inquire as to the state of the opposite lung in these two cases. It was possible that any condition hampering the respiratory expansions of the sound lung might favour distension and encroachment of the pneumonic lung by withdrawing from it the check of lateral pressure. Skoliosis, which was present in one of the two cases, was apt to interfere with the size and the function of the lungs. Perhaps some analogous influence might have existed in the other case. In connexion with the paravertebral triangle of dullness which had been described, it would be desirable to know whether the postural "test" and "counter-test" had been applied in proof of its genuineness.

Dr. HERRINGHAM, in reply, said that in the first case there was no condition which prevented the right lung from expanding. It was engorged, and weighed 24 oz., which was the full size. There was nothing else in the chest. He did not try the test to which Dr. Ewart had referred. If he had, he might have corrected his physical signs; but the patient was too ill to move about.

Further Notes on a Case of Neuritis of Brachial Plexus.[1]

By W. P. Herringham, M.D.

October 12, 1909: Patient seen again. The deltoids of the two sides show no difference, and he can hold the arms horizontal against the strongest pressure that I can exert simultaneously upon them. In the upper arm the muscles of the right side are slightly the smaller; but again he can keep the elbow flexed or extended against my most powerful effort. In the forearm there is considerable wasting on the right side, and the right grasp is decidedly weaker. No visible wasting in the hands. He says there is slight numbness over the right thumb, but the slightest touch with cotton-wool can be felt.

[1] *Proc. Roy. Soc. Med.*, 1908, i (Clin. Sec.), p. 31.

Clinical Section.

November 12, 1909.

Mr. A. Pearce Gould, President of the Section, in the Chair.

Death of Mr. H. H. Clutton.

The President (Mr. Pearce Gould) said he wished to interrupt the ordinary proceedings for a few moments in order to refer to the lamented death of their late President, Mr. Clutton. Mr. Clutton was, as Members were aware, associated with the late Clinical Society for a great number of years. He was the last President of that Society, and he presided at the first meeting of the Clinical Section of the Royal Society of Medicine. So that there were some very special reasons why some comment should now be made on his very lamentable decease. Mr. Clutton was a very distinguished surgeon, he had very great clinical gifts, and those who had the pleasure of attending his demonstrations in the wards of St. Thomas's Hospital would bear him out in the statement that surgery in London had lost one of its brightest ornaments by the decease of Mr. Clutton. He begged to move the following resolution, and suggested that it be sent to Mrs. Clutton :—

> That this meeting of the Clinical Section of the Royal Society of Medicine desires to place on record the great regret with which it has heard of the death of Mr. H. H. Clutton. It recalls that Mr. Clutton was the last President of the Clinical Society, and that he filled the office with marked success and distinction, and he took an active share in the negotiations which resulted in the formation of the Royal Society of Medicine. By his death surgery has lost a distinguished exponent and this Society a valued Member who took the deepest interest in its welfare.

Mr. Charles Symonds, in seconding the vote of condolence, said he was sure it was one to which everybody would agree. It was difficult at a moment's notice to express a true appreciation of a man so highly esteemed by all who knew him. As a surgeon he had a distinguished position; as a teacher there were few who equalled him in his clinical methods; at the Council of the College of Surgeons he was one of the most valued members, and, moreover, represented his college on the Senate of the University of London. The impressive gathering that day at the memorial service in Wells Street—one of the largest he had ever attended—showed the appreciation in which Mr. Clutton was held. Those who had the privilege of his personal friendship will long miss his warm-hearted sympathy.

Report on Intestinal Concretion found in a Meckel's Diverticulum.[1]

By A. Carless, M.S.

The concretion, which has been divided in two, is clearly laminated; in the centre is a nucleus of uncertain origin, and surrounding it the substance of the calculus is of dark-brown colour and almost equally pigmented throughout. It has been submitted for chemical examination to Dr. Emery, who reports that it consists apparently of fæcal and fatty material. Alcohol dissolves out nothing but a little fat and cholesterin; chloroform has the same effect. Hot ether dissolves out a good deal of fat. Alkalis dissolve out colouring matter, but no uric acid; in acids it gives off a little gas, indicating a trace of carbonate. The residue consists of amorphous pigmented material, granular debris, and short hairs, the origin of which is uncertain. Obviously the calculus is of intestinal, and not biliary, origin.

[1] Described at last meeting, *see* p. 14.

Removal of Vesical Calculus Weighing Eight Ounces.

By A. Pearce Gould, M.S.

The patient from whom this calculus was removed first came under my care in May, 1903, suffering from an enlarged prostate. I enucleated a large adenoma of the prostate by the suprapubic route and removed seven small faceted uric-acid stones which lay behind it. The tumour weighed 7¼ oz. He made a good recovery and continued well until the winter of 1905-6, when he caught a chill in Rome, and the urine, which up to that time had been clear, suddenly became "thick" and alkaline. He came to me for the second time in July, 1906, and I found that he had a large stone in his bladder, which I removed by suprapubic cystotomy; it consisted of a large, firm, uric-acid nucleus, with a very thick coating of phosphates. He made a good recovery, and left the Home passing clear and acid urine. On January 5, 1907, he consulted me because he was not passing his urine so freely; I sounded him and failed to detect any stone. In June, 1908, he came to me again because he had had one or two attacks of cystitis; I did not detect a stone on sounding, and after a short course of treatment in a Home his urine became clear. In October, 1909, he returned complaining of the old symptoms of frequency of micturition, pain, and alkaline turbid urine. The sound at once struck a large stone. This was easily felt by bimanual examination, and found to be of unusual size. On October 18 I opened his bladder for the third time above the pubes, and with some difficulty removed a stone which weighed when fresh 8 oz. During its extraction the reflection of the peritoneum on the top of the bladder was torn; this I sutured up before I closed the upper part of the abdominal wound. The patient, who is now aged 71, is making a good recovery, he passes several ounces of urine *per urethram*, but there is still some escape of urine from the wound.

The special interest in this case is in the great rapidity with which the stone formed in the bladder.

Mr. Gould added that, even assuming that at the previous operation a small stone was missed, the case was remarkable as showing how rapidly phosphatic calculi might form in the bladder. An analysis of the calculus showed it to consist of ammonio-magnesium phosphate, mixed with calcium phosphate; it fuses under the blow-pipe.

Sclerodermia with Sclerodactyly.

By H. D. Rolleston, M.D., and G. D. H. Carpenter, M.B.

A woman, now aged 30, was shown at the Clinical Society in 1901, as generalized sclerodermia with Raynaud's disease.[1]

History: She had pneumonia, measles, and chicken-pox when a child; in 1896 she had rheumatic fever with endocarditis, as shown by a systolic mitral murmur which subsequently disappeared. In 1897 she had gastric ulcer and hæmatemesis, and in March, 1900, appendicitis, for which she underwent appendectomy. Symptoms of pain and numbness in the hands and, to a less extent, in the feet began in 1895. When first seen in 1899 she had sclerodermia of the face, hands, and feet. In 1900 the skin over the tips of the first three fingers of both hands began to ulcerate, but healed under treatment; the feet and legs also showed sclerodermia. In 1901 she had, in addition, patches of sclerodermia in both inframammary regions and on the right back and flank; these have now disappeared. At this time the fingers were kept in a semi-flexed position and their movements were considerably restricted.

Present condition: The sclerodermia over the face, arms, and hands has advanced, whilst the feet and legs have recovered. The whole of the face is affected, and the skin round the mouth is radially puckered, so that the oral aperture is much diminished. The hands show considerable ulnar deflexion, are flexed, claw-like, and on admission (October 4) were almost immovable. The terminal phalanges are atrophied, and the nails very small and much over-curved. The skin over the elbows is red, dry, and harsh, while the skin of the fingers, hands, and forearms up to the elbows is dry, shiny, and tense. Attacks of local syncope and local asphyxia still occur in the hands.

Treatment: During the last ten years she has had thyroid, iodides, and various vasodilators without any relief. She has always received benefit from electrical treatment, usually the constant current. During the last month she has been treated by Dr. Allpress Simmons, first with combined faradism and galvanism, and subsequently with galvanism alone. As the result of this and of massage and passive movements of the fingers there has been considerable improvement.

[1] *Trans. Clin. Soc.*, Lond., 1901, xxxiv, p. 215.

Sequel of a Case of Sclerodermia of the Extremities (Sclerodactylia, Acroteric Sclerodermia of Hutchinson) associated with a previous and family history of Angioneurotic Œdema.[1]

By T. D. Savill, M.D.

The patient, a married woman (Mrs. Lloyd) aged 56, had had attacks of angioneurotic œdema at intervals throughout life. The patient's mother and one of her two daughters also suffered from attacks of angioneurotic œdema throughout life. The patient's attacks of angioneurotic œdema ceased two and a half years ago, when the hands and feet first began to get hard and stiff.

When she was first shown to the Section in April last she could walk easily, but now only with the greatest difficulty. The thickening of the skin has extended up the limbs and has grown progressively worse, and the joints have become stiffer and more creaky, showing that changes are taking place within the joint structures as well as in the skin around.

Various methods of treatment have been adopted. Among the drugs employed may be mentioned: Extract of ergot, 4 gr. thrice daily, February 18, for two weeks; calcium lactate, 15 gr. thrice daily for two weeks; thyroid extract, 5 gr. thrice daily for two weeks. In May she was admitted to the West End Hospital for Diseases of the Nervous System, where she had fibrolysin injections, 1 c.c. daily. In June massage and galvanism were administered to the hands, arms, and feet. Subsequently extract of pituitary body was tried. At different times in July sauerin tabloids, sodium salicylate, and castor oil at night were given, with the object of improving the condition of the intestinal tract. Recently phosphoric acid has been prescribed. In spite of careful diet and all this medicinal and electrical treatment she has become progressively worse.

The exhibitor asks for opinions on the associated joint changes and for suggestions as to treatment by organic extracts or other measures.

[1] Exhibited at the Clinical Section on April 30, 1909, see *Proceedings*, 1909, ii (Clin. Sect.), p. 200.

DISCUSSION.

Dr. J. GALLOWAY said such cases as those under discussion were unusual ones, and the experience of any single individual in their treatment must be small, while the difficulty of explaining their ætiology and pathology was so great that any attempt in the way of treatment must be tentative. He could not add much to the methods of treatment already tried by Dr. Rolleston and Dr. Savill, except by saying that in such cases as had come under his notice he had obtained by far the best results, especially in cases involving the neighbourhood of joints, by careful, continuous and patient massage. He had not been able to get much benefit from the other methods of treatment suggested by Dr. Savill.

Dr. HERRINGHAM said he had had two such cases, one of which he had shown before the Society. One of the cases was remarkable for the result. He had been in St. Bartholomew's Hospital for several weeks, but remained practically the same. Then he went to Bath, and while undergoing a course of treatment there he suddenly died, without obvious cause. A post-mortem was declined, so no exact cause could be assigned. There appeared to have been no visceral disease likely to cause death. The other man was in St. Bartholomew's Hospital for several weeks, and went out apparently in the same state as he came in. Both were treated by electricity, and one, and he believed the other also, by thyroid, but no drug or dietetic treatment seemed to do them good. It seemed to him to be misleading to look upon it as a disease of the skin; it was a disease of all the deep structures, as far, at any rate, as the intermuscular planes, and in many cases it seemed to involve the muscles themselves. A very good paper on the subject had been published by Thibierge, and it seemed that the condition was a general fibrosis of the limb, much like cirrhosis of the kidney squeezing the parenchymatous tissue. What that process could be caused by he did not know. Dr. Nixon wrote two papers in the *Bristol Medical and Chirurgical Journal*, in which he said he was strongly of the opinion that it was a chronic infection. But Dr. Herringham did not think anyone had found the infecting agent. He, however, felt sure it was a very deeply seated and deeply growing complaint, and was inclined to connect it with myositis ossificans, a disease of the mesoblastic tissues through the whole series.

Dr. ROLLESTON said that the disease often underwent some degree of cure, so that areas affected at one time were subsequently found to have become normal. The explanation of the disease as the result of a progressive fibrosis of the subcutaneous tissues did not appear to be compatible with spontaneous cure.

Dr. SAVILL, in reply, said his own view was that the condition must be a toxo-angioneurosis. An essential clinical feature in those cases was their localization, in the first place, to the ends of the extremities; this brought

them into the category of Raynaud's disease, which was known to be related to vascular spasm or dilatation. Its occurrence at the ends of the extremities also seemed to bring it into the class of toxæmias, because in these parts toxins or blood alterations had more effect on the fibroblasts and plasma-cells than in any other place, since the circulation was delayed in that position. It was difficult to know what was the character of the toxæmia, but if more was known about the organo-therapy of the various internal secretions, it might be possible to find the source of toxæmia in some derangement of internal secretory organs.

Case of Thoracic Aneurysm in a Boy.

By A. Salusbury MacNalty, M.B.

The patient, J. T., aged 14, was admitted to Brompton Hospital on May 5, 1909, complaining of breathlessness and palpitation. He had scarlet fever at 4 and measles at 5 years of age. When aged 8 he had a sore throat and cough, and was then said to have valvular disease of the heart. Since this time the patient has been subject to periodic attacks of breathlessness and palpitation and on the whole has been getting worse. His mother and three sisters are healthy; his father suffers from heart disease.

The patient is a pale, undersized boy; there is a scar of an old hare-lip and he has a cleft palate. Slight cyanosis and clubbing of the fingers are present. There is a marked bulging of the precordium mainly to the left of the sternum. The whole precordium pulsates. Pulsation is visible in the fourth, fifth, sixth, and seventh left intercostal spaces, in the first, second, and third right spaces, and in the abnormal tumour in the neck.

The apex-beat is in the sixth left space 5 in. from the mid-sternal line, and is heaving, rapid, and localized. In the neck, in front of and to the right of the trachea, is an expansile tumour, extending for $1\frac{1}{4}$ in. above the mid-sternal notch, and limited to the left by the sterno-mastoid, while, on the right, part of the tumour appears to disappear under the clavicle and part sweeps up under the right sterno-mastoid in the position of the right carotid. A marked systolic thrill is felt over the tumour and at the base of the heart. The area of cardiac dullness is increased; there is dullness over the manubrium and $1\frac{1}{2}$ in. to the right of the sternum in the first space. At the apex is a blowing systolic murmur well conducted to the angle of the left scapula;

over the sternum and over the pulmonary base is a typical to-and-fro aortic murmur. At the aortic base and in the first space is a harsh, grating, to-and-fro murmur. Two harsh systolic sounds are heard over the pulsating tumour.

The pulse is rapid; sphygmographic tracings show a typical aortic regurgitant pulse. At the base of the left lung the breath-sounds are weak. The right pupil does not react so briskly as the left. There is not any tracheal tug. The blood-pressure is 170 mm. Hg. The blood-count is normal. On May 20 the patient developed a pleural effusion on the right side, and 20 oz. of simple serous fluid were aspirated.

A skiagram was taken by Dr. Greg, who stated that the tumour was an aneurysm.

The patient was discharged on July 9 and has since attended as an out-patient. The tumour now appears to have considerably increased in size, and this opinion has been confirmed by a second skiagram taken on October 25.

DISCUSSION.

Dr. F. PARKES WEBER said that instead of tracheal tugging there was with each expansion of the aneurysmal tumour a slight upward movement of the trachea and larynx. He had only seen this "inverse trachea sign" described in one case of aneurysm. The trachea, instead of being pulled down, was pushed up (though only very slightly) by the expansile movement of the aneurysm.

Dr. HERRINGHAM asked whether it was really an aneurysm. The continuous thrill in the present case was what he had only experienced in cases where there was a communication between arteries and veins, such as a communication between the aorta and the superior vena cava. There were many points against it being aneurysm, and he did not think the swelling in the neck was proved to be aneurysm at all. Professor Osler described a case in which a similar swelling was operated upon and found to be merely a venous enlargement; this was a possible view in this case, the pulsation being conveyed into the vein. It was true there were no dilated veins, unless that was one.

Dr. ROLLESTON suggested that possibly there was an aneurysm of the ductus arteriosus, such as Dr. E. W. Willett had reported[1] in a child aged 4.

Dr. MORLEY FLETCHER suggested that probably the patient had some congenital morbus cordis. The relatively high erythrocyte count supported that view, especially as there was free aortic regurgitation.

[1] *Trans. Path. Soc.*, Lond., 1892, xliii, p. 38.

Dr. HALE WHITE asked whether the patient had had congenital syphilis, for if he had not it was an argument against the swelling being an aneurysm.

Dr. NORMAN DALTON thought there must be some arterial degeneration present, because in the carotids there was the remarkable bend forwards which was found in arteriosclerosis in older people.

Dr. MACNALTY, in reply, said he had not observed the upward movement of the trachea. If the case were one of arterio-venous aneurysm, as suggested by Dr. Herringham, was it not likely that there would be a continuous murmur? He believed the murmur in such cases had been compared to the sound caused by a fly captive in a paper bag. He had carefully auscultated the boy on many occasions, and had never found the murmur continuous; moreover, as the President had remarked, there was no dilatation of the veins. It might be a case of congenital heart disease. He had not had a Wassermann reaction done. The mother gave no history of miscarriages, and the boy had no signs of congenital syphilis. He did not note the fact mentioned by Dr. Dalton as a sign of arteriosclerosis. Le Boutellier collected the records of sixty cases in the *American Journal of Medical Sciences* of aneurysm under the age of 20, and eighteen of them were thoracic aneurysms. In the case which he himself described the patient was all right up to the age of 4, and then had rheumatic fever and joint pains. The present boy was well until 8 years of age, when he had sore throat—possibly rheumatic.

Case of Rheumatic Spondylitis with Torticollis and Subluxation.

By T. H. OPENSHAW, C.M.G., M.S.

F. W., AGED 11. The history is as follows: The patient was always delicate. He has a mitral systolic murmur at the apex. He has had scarlet fever, whooping-cough, and measles. In 1907 he was laid up for three months with rheumatic fever, and in January, 1909, he had a second attack. During the last attack, while in bed, his head gradually became twisted to the right, as is now seen.

He first attended at the Royal National Orthopædic Hospital on August 3, 1909. The face was rotated to the right to an angle of 30° and dropped to the left, so that the tip of the right mastoid was 1½ in. higher than the tip of the left. The cervical spine was rotated to the right, and the transverse processes of (probably) the second and third were very prominent. The transverse process of the axis was also distinctly prominent. The left shoulder was elevated. No movement of the cervical spine was possible. The head was rigidly fixed in

the present position, and any attempts to rectify the deformity were attended with much pain and spasm of the posterior cervical muscles. Since then he has had the head kept at rest by means of a plaster case, and the acute pain has disappeared and the rigidity is passing off. There are now two degrees of rotation, and some very slight flexion movement is possible.

Case of Rheumatic Spondylitis with Torticollis and Alto-axoid Subluxation.

By T. H. Openshaw, C.M.G., M.S.

H. W., aged 12½. Patient has had chorea three times, and during the whole of 1908 was ill with chorea on and off. He has a mitral systolic bruit. The head was first noticed to be twisted to the left side in February last. The position of the head has varied slightly from time to time, but since the onset has never gone back to the middle line.

The patient was first seen in August last with the face rotated to the right and the head bent over to the left. The pain on movement was so acute that he could not bear the slightest touch. During the last two months he has worn a felt collar, and the pain has disappeared. The mobility of the neck also has increased. A skiagram shows that the anterior surface of the odontoid is displaced ¼ in. from the posterior surface of the anterior arch of the atlas. It would appear that the attachment of the transverse ligament had been softened so as to allow a subluxation and a rotation of the atlas upon the axis.

DISCUSSION.

The President said he had seen, this year and last year, two brothers who manifested much the same changes as were seen in these two cases. In the more advanced case several other joints were also affected.

Mr. Charters Symonds said the cases carried him back to 1882 and the next five years, when at the Evelina Hospital he had several children brought to him with acute pain in the neck—with, in fact, all the symptoms of acute spinal caries. In a routine examination he found that several had a mitral bruit. After a short rest in the hospital the symptoms disappeared.

Probably the present cases represented a later stage of the condition with which he was familiar. He mentioned the matter because it showed the danger of the later developments and the importance of treating these cases in early life, and he made it a practice in his teaching, when spinal cases in young children came up, to see that the heart was carefully examined.

Case of Myopathy—Infantile type (Batten's Frog-child).

By T. H. OPENSHAW, C.M.G., M.S.

W. G., AGED 7½. Patient first came under my care in April last. He has never been able to walk. He could move his arms and legs till onset of measles at age of 18 months. After that he could not move his legs. No history of fits. He began to talk at age of 2½. His mental condition is excellent. He can write very legibly. Speech, nothing abnormal. He cannot stand, even with aid. He makes no effort to hold himself up. He seems very heavy for a boy aged 7½. Face movements good, no wasting noticeable. Eyelids can be raised. Fundi normal. Pupils react equally. Arms: There is power in every group of muscles, with marked wasting. The deltoids and spinati are in a similar condition. Both elbows can be hyper-extended. He can place his hands behind his head quite readily, and the deltoids are felt to contract. Spine: No deformity. Sits as a rule with lordosis. The spinal muscles are not specially wasted, but the neck muscles are not strong enough to hold up head. They all contract, but are readily overcome by pressure. Legs: No sign of power in quadriceps or glutei. The calf muscles are overstretched, and the extensors of the ankle and peronei are shortened. The foot is unusually broad, and the toes tend to double up under the sole. The knees are flexed to an angle of 120° by contraction of the hamstrings. Knee-jerks are not obtainable. There is no spasm in any group of muscles. The hips are flexed to an angle of 55° by contracture. There is no spasm. Abdomen: Abdominal walls harden by contraction when he attempts to sit up. Abdominal reflexes diminished. Testicles both descended.

In August, 1909, the sartorius muscle was transplanted into the quadriceps. A portion of the hamstring muscles was removed for examination. The muscle was normal in colour, but appeared unusually tough and fibrous. The vastus internus muscle was very anæmic and

yellowish in colour, and the muscular bundles were separated. The muscles of the legs reacted to faradaic, and gave a brisk contraction with the constant current.

Microscopic examination shows fatty infiltration, absence of cross-striation, and irregular clefts with granular debris with signs of regeneration in various parts.

Since his admission to the hospital his intellect, which was quick, has become very acute. His general muscular condition has improved. He can sit up unaided by pressure of hands on the floor. Writing and control over fingers and hands are much steadier and stronger. He cannot stand on his feet, but the movements of the legs are stronger than on admission.

Primary Hyperplastic Tuberculosis of the Stomach and Duodenum.

By W. Gifford Nash, F.R.C.S.

Tuberculous ulceration of the stomach is rare and appears only to have been recognized in recent years, as the older books dealing with diseases of the stomach and tuberculous disease do not refer to it. Tuberculosis of the duodenum is almost unknown. Hyperplastic tuberculosis of the pylorus can scarcely be said to have been described. Fenwick, in his exhaustive work on "Ulcer of the Stomach and Duodenum," does not refer to tuberculous ulceration. C. F. Martin[1] does not mention it. Maylard[2] says: "For what knowledge we do possess regarding tuberculosis of the stomach we are indebted rather to the teaching of morbid anatomy than to any specific manifestations which are presented during life." Robson and Moynihan[3] say that primary tuberculous ulceration of the stomach is exceedingly rare.

Mayo Robson, in 1895, operated on a tuberculous girl who had a dilated stomach and a thickened pylorus.[4] There were ascites, tuberculous nodules on the stomach and omentum, and enlarged mesenteric

[1] C. F. Martin (in Osler and McCrae's "System of Medicine," 1908), article on "Diseases of the Stomach," v, p. 175.

[2] "Abdominal Tuberculosis," Lond., 1908, p. 10.

[3] "Surgery of the Stomach," 2nd ed., Lond., 1904, p. 455.

[4] *Ibid.*, p. 243.

glands. Pyloroplasty was performed, but the patient died during the second week after the operation. Mr. Mayo Robson has informed me that this is the only case he has seen of hyperplastic tuberculosis of the stomach, and Mr. Bland-Sutton and Mr. Moynihan, in response to inquiries, say that they have never met with a case. The fact that these three surgeons, performing numerous abdominal operations, have only seen one such case shows the extreme rarity of the condition. Maylard, in his recent work on "Abdominal Tuberculosis," discusses tuberculosis of the stomach and refers to various papers on the subject. In every instance the affection was secondary to disease elsewhere, and in only one case (Mayo Robson's) was the disease of a hyperplastic nature, if we except three doubtful cases reported by Patella, in which the author thought pyloric stenosis was due to the slow sclerosis dependent on tuberculous intoxication. In addition to the cases recorded by Maylard, there is one reported by Beadles.[1] It was the case of a woman, aged 58, who had suffered from gastric pain and vomiting for six months before death. She was the subject of pulmonary tuberculosis. Necropsy revealed considerable dilatation of the stomach, with extensive ulceration and thickening of the pylorus. It is probable that primary hyperplastic tuberculosis of the stomach and duodenum, involving chiefly the pylorus, has in the past been diagnosed as malignant disease, and in the absence of an operation or necropsy it is difficult to see how a correct conclusion can be arrived at. Hyperplastic tuberculosis of the intestines for a long while was mistaken for a malignant process, and many cases of excision of tumours of the cæcum, which recovered and remained well, were no doubt examples of this disease. In my first case[2] more than one pathologist gave his opinion that the tumour was malignant. Now that this condition affecting the cæcum and sigmoid has been so well described and has been so frequently recognized, it is probable that more examples will occur of the same process affecting other parts of the alimentary canal.

Notes of Case.

A married woman, aged 33, was admitted to the Bedford County Hospital on October 12, 1908, complaining of loss of flesh, frequent vomiting, and a lump in the abdomen.

[1] *Brit. Med. Journ.*, 1892, ii, p. 735.

[2] *Lancet*, 1907, ii, p. 957.

History: She was the mother of four children, the youngest of whom was two years old. During her last pregnancy she was frequently sick and after the birth of the child suffered much from indigestion. During the six months previous to admission she got much worse and vomited after meals, especially in the evening. Pain came on a quarter of an hour after taking food and was relieved by vomiting. She never vomited blood. The catamenia did not reappear after the birth of the last child.

On examination, the patient was a very emaciated woman. Inspection of the abdomen showed a very dilated stomach extending downwards to about 3 in. below the umbilicus. Peristalsis was very well marked. On palpation a hard, movable, elongated, sausage-shaped tumour could be felt below the right costal margin in the position of the pylorus. The diagnosis was dilated stomach due to a mass in the pylorus of a doubtful nature. The long duration of the symptoms suggested that it was not malignant. There were no signs of disease in the thorax. The patient was too weak at that time to undergo any operation, so she was fed on an easily-digested and nutritious diet, and saline rectal injections were given to supply her with fluid.

Operation: On November 9, 1908, the abdomen was opened above the umbilicus, just to the right of the middle line. There was a very little fluid in the peritoneal cavity. The stomach-wall and omentum were covered with miliary tubercles and the glands in the gastro-colic omentum were enlarged. The tumour was smooth on the surface, very hard in consistence, and occupied the whole circumference of the pyloric end of the stomach. It extended along the posterior wall of the duodenum for about 3 in. It was thought to be unsafe to attempt the removal of the growth owing to the large amount of duodenum involved. A posterior gastro-jejunostomy was performed and the abdomen closed. There was no vomiting after the operation. Water was given by the mouth on the first day, and next day milk was added. Nutrient enemas were given for a week. At the end of a month her fare consisted of three pints of milk and two eggs daily, with such extras as Benger's food, Bovril, bread and butter, fish, and minced meat. On November 26 she weighed 66½ lb., and on January 21, 1909, 81 lb.

On January 28, owing to increased ascites, the abdomen was opened below the umbilicus and seven pints of clear, straw-coloured fluid removed. The intestines and peritoneum were covered with miliary tubercles and the mass in the stomach was larger than at the first operation. The fluid again collected, and on February 22 the abdomen

was opened for the third time and eight pints of fluid removed. On March 23 the abdomen had again filled up and the weight had gone down to 63 lb. The patient became very depressed, and on March 25 her friends insisted on taking her home to die, which she did on April 5, 1909, five months after the operation of gastro-jejunostomy. There was no necropsy.

DISCUSSION.

Dr. F. PARKES WEBER said that with reference to the diagnosis between abdominal malignant tumours and abdominal tuberculous tumours, he would like to mention a curious case—namely, that of a man who had malignant disease of the extra-hepatic bile-ducts, commencing at the junction of the hepatic ducts. Owing to the proximity of the biliary obstruction to the liver cholecystostomy would have been useless, and Dr. Michels, his surgical colleague, made an opening into the liver itself ("hepatostomy"), so that the bile was drained directly from the intrahepatic dilated ducts outwards. But the man did not do well, although his jaundice disappeared. Gradually he lost flesh, and finally he died. At the post-mortem examination a number of "tumours" were found in the abdomen, which at first sight seemed to be metastatic cancers, but were found by microscopic examination to be tuberculous "tumours," except the growth of the bile-ducts, which proved to be a primary carcinoma without any metastases. He described the case with Dr. Michels in one of the volumes of *Transactions of the Royal Medical and Chirurgical Society*.[1]

Dr. ROLLESTON said that probably every one had seen cases of multiple malignant nodules of the peritoneum which to the naked eye were indistinguishable from peritoneal tuberculosis. Mr. Nash had described a most remarkable lesion of the stomach, and it was therefore important to hear if the naked-eye diagnosis of tuberculosis had been established by microscopical examination of tissue removed at the operation.

Dr. W. CAMAC WILKINSON said he also wished to express doubt as to its tuberculous nature. It reminded him of other instances, of which he had seen several, causing obvious gastric symptoms, in which a suspicion of cancer had arisen, and yet not only had he caused a definite reaction with tuberculin, but by continuing tuberculin as a remedial agent he had produced astonishing improvement. Such cases demonstrated the great value of tuberculin. On the other hand he had had one case in a young person in which so-called miliary tubercles were not only present in the abdominal cavity, but also in thousands throughout the lungs, and they proved to be cancerous nodules.

[1] *Transactions*, 1905, lxxxviii, p. 247.

Mr. NASH, in reply, said he could not give any definite microscopical proof that the condition was tuberculous. At the time he had no doubt that the miliary tubercles were tuberculous; they did not look like secondary malignant growths; and the nature of the straw-coloured fluid at the three operations remained the same. There was no tendency to bleed, as would be expected if they were secondary malignant growths. The reason he did not make more careful examination of its nature was that he expected the patient would remain in hospital until the end. But from what she heard of another case, she took fright, and insisted on going home. A similar case was reported by Mr. Mayo Robson, and his verdict seemed to have been accepted without question, whereas his was queried. As far as one could tell from the naked-eye appearances, his own case was one of tuberculous peritonitis, due secondarily to the growth which originated in the pylorus.

Excision of the Larynx, lower part of the Pharynx, and Upper End of the Œsophagus for Malignant Disease (Squamous-celled Carcinoma) of those Structures.

By ARTHUR EVANS, M.S.

THE patient, a female, aged 40, was first seen on September 19, 1909. She complained of hoarseness and increasing difficulty in swallowing solids; this had been noticed for about five months. Laryngoscopic examination revealed a large malignant mass filling up the lower part of the pharynx and extending so far forwards that it covered over a large portion of the vocal cords and almost completely hid the left—one could get a glimpse of the left, and it was "fixed"; the right vocal cord moved freely. By laryngoscopic examination it was not possible to state whether the mass of growth was only post-cricoid or whether it extended round the whole circumference of the upper part of the œsophagus. There was no obvious glandular enlargement in the neck. The patient was markedly emaciated.

The malignant nature of the condition was explained to the friends, and the almost impossibility of its removal; it was suggested that she should be allowed to remain unoperated upon, and that when the disease had so far advanced that any trouble was experienced in swallowing liquids gastrostomy should be performed. The risks of an attempt at extirpation were pointed out, and the unenviable condition of the

patient, after even a successful operation, was described. The patient returned on September 25, having decided on extirpation.

September 28: Gastrostomy was performed, Abbe's modification of Kader's method being adopted.

October 2: An incision was made in the middle line of the neck extending from the hyoid bone to within 1 in. of the suprasternal notch, down on to the thyroid cartilage, the isthmus of thyroid gland, and the trachea. The isthmus of the thyroid was tied in two places and divided between. The structures were separated off the sides of the larynx and trachea.

I was anxious to make a close examination of the extent of the growth, and hoped to find that I could save some part of the larynx and, maybe, a strip of œsophagus; so I incised the second and third rings of the trachea and inserted a tracheotomy tube, through which the anæsthetic was now given. The thyroid cartilage was slit up, and then it was found that the malignant growth had invaded practically the whole of the upper orifice of the œsophagus, and to remove the disease it was necessary to remove the larynx and the upper part of the œsophagus. The trachea was cut across obliquely—backwards and upwards—so as to save as much as possible of the anterior wall. Great care had to be exercised in cutting through the posterior wall and in separating it from the œsophagus. A transverse skin incision was now made at the suprasternal notch, and the portion of skin between this and the longitudinal incision was undermined. Through this the trachea was brought and stitched to the skin with six catgut sutures. To expose freely the upper end of the growth I extended the incision upwards and cut across the hyoid bone, then made a transverse incision on each side immediately below the hyoid bone. With the parts well retracted up, it was now quite easy to incise the posterior pharyngeal wall well above the growth, and with very little difficulty the lateral walls of the pharynx were freed, and the pharynx with the larynx were separated from above downwards; at the level of the cut trachea it was obvious to the fingers manipulating the œsophagus that the malignant disease extended lower down, so the œsophagus was isolated still lower and a strong silk ligature passed round it at a level which felt well below the disease; the œsophagus was cut through and the growth removed. On slitting up the œsophagus it was clear that the incision had been made too near the growth, so the stump was drawn upon by the encircling ligature, the ends of which had been purposely left long and the œsophagus separated for another inch, which was as far as the fingers could reach; here it was ligatured and

cut across. The transverse incision and most of the median one were now closed with one layer of sutures and the interior packed lightly with cyanide gauze. There was no marked shock during or after the operation. The same evening there was a good deal of bleeding from the wound, which was checked by introducing additional gauze packing; a hypodermic injection of 3 m of strychnine was given every four hours for the next forty-eight hours. The convalescence was quite uneventful.



FIG. 1.

Larynx and pharynx viewed from above.

October 14: The secretions from the mouth, which trickled out of the unclosed part of the wound, came on to the surface immediately above the tracheal opening and had constantly to be prevented from being sucked into the trachea; I therefore sewed up the lower inch of the opening and extended it upwards for a corresponding distance.

November 12: The openings are still much too near, and I intend making a fistula immediately below the hyoid bone and closing the present one. In addition to the reason mentioned above, there is

another, in that I have had a small celluloid cup made which is held in position over the fistula by an elastic band which passes round the neck; this cup is connected with the gastrostomy tube, and by means of this the patient is able to take fluids by the mouth, which are then led via the neck fistula and the gastrostomy tube into the stomach. At present the proximity of the fistula to the tracheal opening prevents this working to its best advantage. I feel confident

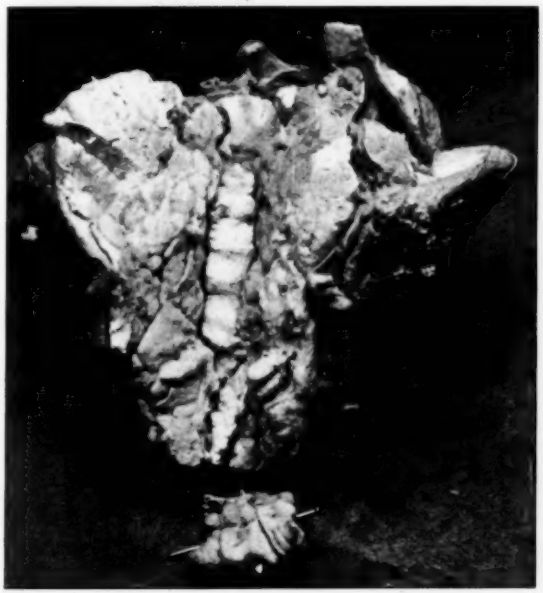


FIG. 2.

Pharynx and œsophagus slit up and viewed from behind.

that when I have placed the fistula in its new position the patient will be able to drink with ease.

Mr. CHARTERS SYMONDS said he did not think many cases of the kind had been operated upon in this country. Although cancer in this particular situation was not uncommon in women between the ages of 30 and 40, it was very rarely seen in men. Professor Gluck, of Berlin, had done most of the work on

this subject, and he had avoided some of the difficulties which had arisen in the present case, especially a fistulous opening in the neck. It seemed better to leave the œsophagus open in the neck, and subsequently pass a rubber tube of funnel-shape through the mouth and down the pharynx and into the œsophagus below. Gluck's patients had swallowed well, with very little saliva escaping from the neck, and that suggestion might be useful to Mr. Evans in a further case. The great objection which English surgeons had hitherto had in attacking such cases was, that when they were suitable for operation the larynx was found to be unaffected. It seemed so great a sacrifice to remove an apparently normal larynx that he had himself hesitated when recognizing the condition early—occupying no more than an inch of the laryngo-œsophageal junction—to advise operation. The most brilliant success which he had seen in the way of operation in such a case occurred in the practice of a colleague. The growth was excised by an incision through the side of the neck, leaving the normal larynx and making a new œsophagus by a flap of skin, which was sewn to the pharynx above and the œsophagus below. He suggested that it would add to the patient's happiness if Mr. Evans would adopt Gluck's method of assisting the patient to speak. The apparatus consisted of a metal attachment to the cannula, from which a rubber tube conducted the air to the lips. Speaking into the stream of air the sound could be heard in any part of this room. Such an apparatus could be seen at Messrs. Mayer and Meltzer's.

Case of Pneumococcic Colitis with Hyperpyrexia; Severe Hæmorrhage treated by Appendicostomy.

By H. E. Bruce-Porter, J. P. Lockhart Mummery, F.R.C.S., and W. Hale White, M.D.

An unmarried lady, aged 31, came to London this year to consult one of us. Her family and personal history were unimportant. She said that for three weeks she had had severe diarrhœa, the bowels being often open ten or twelve times a day, and the motions nearly always contained a considerable quantity of blood. There was much pain in the abdomen, and considerable sickness. The illness began acutely, and could not be attributed to any errors of diet.

She was very weak, and extremely pale from loss of blood. It was deemed advisable to send her to a nursing home, on account of her great weakness and general illness. Her temperature was then found to be about 101° F., and her pulse about 130. She was put to bed and kept

upon small feeds of milky foods. Under this treatment the sickness stopped, the frequency of the diarrhœa diminished somewhat, but the motions still contained a very large quantity of bright blood, and the temperature ranged between 101° F. and 102° F., and the pulse remained about 130. She was examined with a sigmoidoscope five days after admission. The mucous membrane as high up as could be seen was very red and spongy, and there was obvious oozing of blood from it. After this examination we all concurred in the diagnosis that the patient was suffering from acute hæmorrhagic colitis, probably due to pneumococcic invasion. The reasons for this view were the sudden onset, the high temperature, the extreme loss of blood, and the appearance of the mucous membrane.

As the bleeding had not diminished by any medical treatment that had been applied, and as the motions consisted almost of pure blood and the patient was obviously slowly sinking, it was decided, after consultation between all three of us, to try to stop the hæmorrhage by opening the appendix and irrigating the bowel from the opening to the anus; appendicostomy was therefore performed and the colon irrigated with warm water and hazeline at once. The irrigation was ordered to be repeated every six hours. This had the effect of stopping the hæmorrhage, for after this was done no more recent blood was seen, although large quantities of dark altered blood that had evidently laid some time in the colon and smelt most foully were washed out on several occasions. Twelve hours after the operation the patient appeared to be doing well, but to the surprise of the nurses the temperature taken at 8 a.m. was 105·6° F. We were all three immediately summoned by telephone, and by the time we got to the patient her temperature was 107° F. in the vagina. We could not give her a cold bath because of the wound of the appendicostomy; accordingly mackintoshes were put underneath her, and we three and two nurses kept water, in which pieces of ice were floating, constantly running over her by means of big sponges. After we had done this for thirty minutes, the temperature in the vagina was 109° F., and the patient was pulseless; indeed, if it had not been for slight respiratory movements, we should hardly have known she was alive. The sponging was continued, and at the end of forty-five minutes the vaginal temperature was 108° F. We still continued for another quarter of an hour, and then put the patient back to bed, the vaginal temperature then being 100° F. During the last quarter of an hour of sponging we had accelerated the fall of temperature by running cold water through the open appendix and letting it flow out of the anus.

When the patient was put into a fresh bed she seemed very collapsed, and the temperature fell to 95° F., but by means of hot-water bottles she soon rallied. On the evening of the same day the vaginal temperature shot up to 106° F. It was again brought down by the same treatment. In the early morning of the next day it went up to 107° F., and again was brought down by the same method of sponging. As on these three occasions the temperature had risen extremely rapidly, often a degree in ten minutes, after it had reached 101° F., the next time it reached 101° F. she had a short sponging, and after that we always sponged her with iced water whenever the temperature went over 103° F. in the vagina. Altogether eight spongings with iced water were necessary during four days, but by sponging at the lower point the temperature was kept fairly under control, although it usually mounted somewhat during the first quarter of an hour of the sponging and then fell. The patient at the end of these four days was obviously much better, and we gave up taking the temperature in the vagina and took it regularly in the axilla. It remained between 100° F. and 102° F., the pulse fell to 120; the results of washing through the colon were healthy fæces, and everything appeared to be going well until twelve days after the first hyperpyrexia, when the patient did not seem quite so well, and fluid injected into the appendix wound flowed out with difficulty from the anus, her temperature mounted to 102·8° F., the pulse became extremely feeble and rapid, and she died apparently from sheer cardiac failure. The wound at one time looked very unhealthy, a greenish membrane forming over the surface. The stitches were removed; the state of the wound appeared to us to be due to the pneumococcic infection more than to any accidental contamination during the sponging, every attempt having been made to avoid this. As the result of careful dressing, the wound became more healthy, and was granulating satisfactorily when the patient died. Some of the motion washed through the bowel with plain water was referred to Dr. Eyre. He reported that it contained pneumococci, *Bacillus coli communis*, and other coliform organisms. He was fortunately able to isolate the pneumococcus and grow it in pure culture. He himself had not the least doubt that it was the cause of the extensive colitis. Throughout the whole of the illness the lungs remained healthy; there were no cardiac murmurs nor any evidence that the pneumococcus had locally affected any parts of the body other than the colon and the wound.

We have recorded this case because, in the first place, it is, we believe, unusual for a diagnosis of colitis due to pneumococcus to be

made during life and confirmed by bacteriological examination. Secondly, because it illustrates the difficulties of such a case and the successful treatment of the hyperpyrexia by cold sponging. We would especially draw attention to the beneficial effect of running iced water through the colon. Thirdly, the case illustrates the value of appendicostomy as a means of stopping severe bleeding from the colon. Before she came under our notice the bowel had been washed out from the anus by intestinal douches without in any way controlling the hæmorrhage, nor did the same method of applying hazeline and water have any effect during the four days we tried it before doing the appendicostomy. While the patient was in the nursing home she took her food very well, and on several occasions we injected ½ oz. of sterilized olive oil under the skin. It was always completely absorbed, and thus the case shows the occasional advantages of this little-used method of feeding. The milk she took had dextrose added to it.

DISCUSSION.

Mr. LOCKHART MUMMERY said that when he opened the abdomen to perform the appendicostomy the cæcum and the whole of the colon that could be seen or felt were very much thickened and covered with recent lymph, whereas the small intestine and parietal peritoneum were apparently healthy. There was a little fluid in the abdominal cavity.

Dr. DALTON asked if at any time the agglutination reaction with Shiga's bacillus was tried.

Mr. CHARTERS SYMONDS said the subject of the paper was one in which he was specially interested, and for which he had operated several times. He noticed that the present patient died twelve days after the operation. He had had a recent case which led him to think that the appendicostomy did harm, and he mentioned the fact because he thought every aspect of the cases should be presented in discussion. The lady he was speaking of was riding a horse not long before he saw her. She was passing considerable quantities of blood, much mucus, and had considerable abdominal pain. He performed appendicostomy, and from that time he thought she steadily got worse: she was irrigated repeatedly, sometimes for an hour at a time. It might have been that the operation was performed just at the time she was getting worse; that raised the question as to how soon one should operate upon such cases. He performed the operation some time ago on another patient, in whom there was not very much blood; the cæcum was very thick, red and swollen, and covered with white patches, like old keratosis of the tongue. The transverse colon and sigmoid

were both thick in the same way. The temperature, which was not high before the operation, rose steadily afterwards, and the patient died in a little more than a fortnight.

Dr. HALE WHITE, in reply, said an attempt was made to get some blood for an agglutination reaction, but the patient was so bloodless that the small amount obtained was useless for bacteriological purposes. He felt no doubt that in that case the appendicostomy contributed towards the saving of the patient's life. She lived for twelve days afterwards, though at the time of operation she was dying of hæmorrhage as surely as anyone could; she was doing so well that a vaccine was not used, though a pure culture of the pneumococcus was ready in the laboratory.

Clinical Section.

December 10, 1909.

Mr. A. Pearce Gould, President of the Section, in the Chair.

A Case of Acromegaly.

By John R. Lunn.

L. B., female, aged 52, came under observation for piles in August, 1909, for which she was operated on successfully. She stated that she had suffered nearly all her life from headaches, accompanied by giddiness but not by vomiting, the attacks lasting for ten days and then passing. No diplopia. No change in hands, feet, or face noticed, but for two years her voice had altered, and she felt as if she always had a cold, and that the tongue was too large for the mouth. Menopause six years ago. She is an intelligent woman; height, 4 ft. 9½ in.; weight, 10 st. 9½ lb.; voice, rather thick and blurred; tongue enlarged; there is a copious secretion of saliva. The face appears rather long, and the lower jaw protrudes unduly; the lower lip is enlarged. The teeth are separated by intervals; the cartilages of the nose are thickened, but not those of the ears. The skin is coarse; sebaceous glands very obvious; there is a plentiful crop of small warts on the forehead and cheeks. Hair, thick and coarse; cranial bones not enlarged. The hands are short, broad, and square-looking, but without puffiness or skin changes; nails very short. The feet are short and broad, but with no striking changes except that the great toes are enlarged. No abnormal curves of long bones or spine. Circulation feeble, and patient took chloroform badly (? from size of tongue). Hearing and taste normal; no hemianopia or nystagmus; the pupils react to light and accommodation; urine acid and normal; no tenderness of the skull on palpation.

Case of Acromegalic Gigantism.

By Robert Milne, M.D.

Male, aged 19, admitted to Dr. Barnardo's Homes ten years ago. His father was 6 ft. 2 in. in height, his mother 5 ft. 8 in. He has one brother 6 ft. 3 in. high, another 6 ft. 11 in., a third 5 ft. 8 in., whilst one sister is 6 ft. 3 in., and a second, a girl aged 13, is 5 ft. 5 in. high and weighs 140 lb. When aged 9 patient was a little under the average height and weight, but otherwise was quite normal. In March, 1899, he was emigrated to Canada, and did well at first, but grew thin. In June, 1902 (aged 13), he began to grow very rapidly, and suffered from pains in the legs and back, with a feeling of general weakness and difficulty in breathing on exertion. In 1904 he was deported to this country, and has since been under observation in the Homes. In March, 1905, he was 6 ft. high; in July, 1906, 6 ft. 4 in.; in May, 1908, 6 ft 8 in.; in November, 1909, 6 ft. 11 in. His hands have increased out of proportion to the rest of the trunk, measuring now $9\frac{3}{4}$ in. in length; his foot is now $12\frac{1}{2}$ in. long. His mental powers are above the average, and his character is good. He is sociable, full of fun, and a good draught-player. The internal organs are normal, except that the heart is not very strong. In June, 1908, he underwent an operation for genu valgum of the left leg, which was abducted from the right to an angle of 30°. Spinal anæsthesia was employed on account of the cardiac weakness. The ligaments of the knee have become so relaxed that a leather splint with steel support is employed, and in this he can walk well. His field of vision is normal, and he has no headache; the thyroid body is not enlarged. He has been treated with thyroid and pituitary extracts. A skiagram of the head shows evidence of enlargement of the pituitary body.

Two Cases of Achondroplasia.

By Robert Milne, M.D.

A woman, aged 22, and a boy, aged 15, not related, are both typical cases of achondroplasia. In both the vault of the cranium is abnormally large, and there is the typical depression at the root

of the nose. Prognathism is characteristic in each. The hands do not reach to the trochanters. The trunk is normally developed, but with beaded ribs, while the arrested development of the long bones (especially the humeri and femora) is very well marked, and the epiphyses are enlarged. The hands are trident-like, with thickening of the proximal phalanges. The adipose tissue is abundant, with smooth plastic skin and abundance of hair. The mental condition of both is good, that of the boy being much above the average. The condition depends on the arrested development of the long bones from the third to sixth month of fœtal life; centres appearing after the sixth month, and the membranous bones escape. The proximal segments are relatively more affected than the distal. The middle of the body is $2\frac{1}{2}$ in. and 3 in. above the umbilicus respectively. The following are the measurements :—

	Woman	Boy
Height	45 inches ...	45 inches
Length of body ...	24½ ,, ...	26 ,,
Humerus	5 ,, ...	5 ,,
Radius	5 ,, ...	5¼ ,,
Femur	8 ,, ...	9 ,,
Tibia	7 ,, ...	6½ ,,

Case of Adiposis Dolorosa (Dercum's Disease).

By G. McMullan, M.B.

Female, aged 62; married, ten children.

History: Five years ago she first noticed numbness of the hands and arms, followed later by pain, and subsequently by symmetrical fatty masses in both upper arms, that in the left arm appearing some twelve months before that in the right. The fatty masses were at first limited in extent, and confined to the upper and inner aspects of the arms, and I have been informed by a medical man, whom she consulted at that time about her left arm, that the fatty tissue resembled in every respect a circumscribed lipoma. The condition continued to progress steadily until May, 1909, when the severity of the pain and numbness in the arms attained such a degree that she could only with difficulty perform the lightest household duties, and was quite unable to execute the finer co-ordinated movements required in sewing and writing. Pain and numbness were also severe over the scapular regions and the sternum, and to a lesser extent on the inner sides of the thighs just

above the knees. Added to this, muscular weakness and great lassitude were present. Two and a half years ago patient had repeated and alarming hæmorrhages from the stomach; and on another occasion, following upon the extraction of a tooth, a very serious hæmorrhage occurred. Slight injuries always cause marked subcutaneous extravasation of blood. There is a very definite history of alcoholism, but her mode of life and social conditions are now quite satisfactory. No other member of her family has to her knowledge suffered from a similar disorder, and she has several living children who are normal in every respect.

The patient is pale and flabby, and evidently suffers from debility and a mild degree of anæmia. The skin is of a yellow tint and almost



Adiposis dolorosa.

Showing enlargement of arms. View of back.

parchment-like in its dryness, perspiration being very deficient. There are large pendulous deposits of fat upon both arms and back. The hands and feet are normal, and the face is not affected in any way. The fatty masses are present in the following positions: on both upper arms, mainly upon their external and postero-internal aspects, and also upon the upper and inner aspects of the forearms just below the elbows. The back shows symmetrical fatty deposits over the scapular regions, extending as low as the tenth rib. There is also a pad of fat over the sternum, and extending for some 3 in. laterally upon either side over

the second and third costal cartilages. The body below the waist, though covered with a thick layer of adipose tissue, does not show the fatty masses characteristic of the disease, save upon the outer aspects of the gluteal regions, especially the left, where a well-marked pendulous fatty mass has lately developed.

The patient is nervous and sleeps badly at night; she, however, feels heavy and drowsy during the day, especially after meals. Mentally she is bright and intelligent; she complains of loss of memory, and also of numbness and tingling sensations in the limbs, and of aching pains located principally in the upper arms, and to a lesser degree across the back and chest, and upon the inner aspects of the thighs just above the knees. Wherever the fatty tissue is present, even slight pressure elicits pain. The pain is also severe when pressure is made over the nerve trunks. There are areas of marked hyperæsthesia over both outer and inner aspects of the upper arms. No anæsthetic areas can be demonstrated. The reflexes are normal, but co-ordination is not good and muscular sensibility is impaired. The teeth are bad, and there is a mild degree of pyorrhœa alveolaris, and consequently dyspepsia and constipation. The liver is probably cirrhotic. The respiratory and circulatory systems show no noteworthy departures from the normal; urine is straw-coloured, acid, specific gravity 1020, and contains a trace of albumin; the pulse is usually about 80 per minute.

Treatment and Course of the Disorder.—Thyroid extract, 5 gr. night and morning, was commenced in July, 1909. In a few weeks the pain and numbness had almost entirely vanished, and have not subsequently returned. The reduction in girth of the arms was at first remarkable. Later on the patient did not fully realize the necessity of continuing treatment, and consequently no further improvement occurred. Some weeks later she again took the extract, with further improvement. Within the last month, however, in spite of regular treatment, the condition has progressed, especially about the upper part of the forearms, and the fatty mass upon the left buttock, previously mentioned, has made its appearance.

The following measurements show the course of the disease when under treatment:—

	May, 1909 Inches	Aug., 1909 Inches	Nov., 1909 Inches	Dec., 1909 Inches
Upper right arm	23½	21	19½	22
Upper left arm	24	22	20¾	23
Right forearm	12½	12	11½	13½
Left forearm	12	12	11½	12½

DISCUSSION.

The PRESIDENT (Mr. Pearce Gould) asked if the nerve symptoms preceded the abnormal appearance of fat; also if the lipomata were truly circumscribed and encapsuled so that they could be moved about in the subcutaneous tissue?

Dr. GOSSAGE exhibited some photographs of a patient under his care with adiposis dolorosa. Her age was 47, and her symptoms dated back two and a



Dr. Gossage's case of adiposis dolorosa.

half years, to the time of the menopause. Since then she had suffered from pains in the head and limbs, and had gradually increased in weight. No exact history was obtainable as to the place where the pain was first noticed. The photographs showed, as in Dr. McMullan's case, a number of fatty tumours rather than a general increase of adipose tissue. These tumours were largest in the arms; the breasts were enormous, and there were also lipomata in the buttocks, and to a less extent in the thighs. The tumours were painful, and were tender when handled. The increase of weight

was very great. Her height was only 4 ft. 11 in., and yet she weighed 207 lb., which is about 70 per cent. above the normal weight for her height and age. It had been stated that the subjects of adiposis dolorosa were usually mentally deficient, and this woman was not particularly intelligent, but it was impossible to say that she was less intelligent now than she had been when young. She was fairly active, and answered questions rapidly and without hesitation. The treatment had consisted in restricting the intake of carbohydrates and in the administration of thyroid extract, of which 2½ gr. had at first been given two or three times a day, the dose being subsequently increased to 5 gr. The weight had diminished at first, but subsequently increased, and now, eight months after the commencement of treatment, she was 2 lb. heavier than when first seen; moreover, the pain and tenderness had remained quite unaffected by the treatment. There had been no discomfort from taking the thyroid extract.

Mr. WARREN LOW dwelt on the fact that in both the patients the pain preceded the swelling; in cases of symmetrical lipomata he had often noticed that when a lipoma appeared on one side of the body there was, on the opposite side, a painful area in which a similar tumour subsequently appeared. This was brought to his notice some years ago, when seeing a woman with a lipoma of the right buttock who complained of pain in the left buttock; some months afterwards a lipoma appeared on the left side. The history showed that the first lipoma had also been preceded by similar pain in that region.

Dr. LEONARD WILLIAMS said that cases of adiposis dolorosa, or Dercum's disease, were, in his opinion, due to some defect in more than one of the internal secretions. That the thyroid was involved was quite obvious; these cases were characterized by three points, which were also found in myxœdema —i.e., a slow pulse, a subnormal temperature, and a dry skin. The deposit which took place was characteristically capricious. In all cases of well-marked myxœdema, when the patient held out her arms, swellings appeared in the distribution of the triceps muscles. Similar swellings resembling, though they were not, lipomata were present above the clavicles and beneath the breasts. The particular type of myxœdema called Dercum's disease occurred, as far as he knew, only in women after the menopause, in some after a considerable interval, in others immediately succeeding it. The menopause was due primarily to a decline in the ovarian activity, which should be accompanied by a synchronous decline in the thyroid and other secretory activities. But when these activities did not decline together, as they should do, it appeared that some of the defects under discussion were apt to arise. So far as the element of pain was concerned, it was well known that a large number of cases of neuralgia were curable by the administration of thyroid extract in small doses. It had been stated by some of the speakers that they gave 5-gr. doses to begin with; in his opinion an initial dose of that amount was ten times too large. It was a curious paradox that thyroid extract was particularly

badly borne in the first instance by the very patients who were most in need of it. He thought the absence of thyroid was a large factor in the production of Dercum's disease, but that the absence or perversion of some other secretion—it might be ovarian, or pituitary, or suprarenal, or even pancreatic secretion—was responsible for the fact that Dercum's disease differed in many respects from myxœdema. He suggested that these cases should be treated in future by a combination of thyroid extract with ovarian extract or pituitary extract; and he thought it probable that such a combination would bring about great improvement. It was, at any rate, worthy of trial.

Dr. POYNTON said that in his experience patients who required thyroid took it readily, whilst those in whom it was not beneficial were easily overdosed.

Sir DYCE DUCKWORTH said that under the name Dercum's disease several pathological conditions were described, in all of which fat was deposited in greater or less quantity in various places. He could not say he had met with instances of fatty tumours which had been preceded by pain locally, though no doubt they occurred, but he knew that fatty tumours, once they were formed, even if they did not change or enlarge progressively, were sometimes very sensitive to pain and bore pressure badly. At times they caused neuralgic pain, dependent on depressed health or even on changes in the weather, in people who had had these masses for a long time. There was no question about the power of thyroid extract over cases of the kind. The patient's weight could be materially reduced, and the size of the tumours also, by the extract in 2 to 3 or 5 gr. doses of thyroid, best given at night-time.

Dr. GALLOWAY said that since Dercum's first paper a good many cases had been described and shown as cases of adiposis dolorosa, which had but slight resemblance to the original description. Recently he had had the opportunity of seeing some of the cases shown during one of the higher surgical examinations. In one case it was evidently expected that the candidate should make the diagnosis of adiposis dolorosa, but this patient had but the slightest resemblance to the one shown this evening. Dr. George E. Price,[1] working in association with Dr. Dercum, had collated a good many of the cases described as adiposis dolorosa, and it could be seen how different many of them were from the original type. It was probable, however, that the errors of metabolism responsible for Dercum's disease might produce other severe nutritional disorders. Amongst these was the type in which a diffuse adiposis was associated with serious disturbance of the peripheral circulation, constant congestion of the skin, and the tendency to the easy development of gangrenous ulceration. In one such case Dr. Galloway had had the opportunity of making a necropsy in difficult circumstances. No naked-eye lesions of the thyroid or other internal organs referred to by Dr. L. Williams were noted, but, in addition to the great increase of the fat in the body, the muscles showed unusual changes. They were not only infiltrated with fat, but the remaining

[1] *Amer. Journ. Med. Sc.*, Philad., 1909, N. S., cxxxvii, p. 705.

muscular fibres were pale and seemed degenerated, somewhat resembling the changes seen in muscular dystrophy. Not only the feebleness, but the muscular pain suffered by such patients during life is an important feature. Treatment by means of thyroid gland did not appear to have given satisfactory results in most cases of Dercum's disease.

Dr. F. PARKES WEBER said that a genuine feature of the present case was that there were none of the diffuse lipomata (namely, the symmetrical lipomata at the back of the neck in the axillary and inguinal regions, &c.) which were present in so many of the cases published (probably incorrectly) as examples of "Dercum's disease." Many such cases had really been examples of diffuse symmetrical lipomata, which some French authors (on false grounds, Dr. Weber believed) had termed "symmetrical adeno-lipomatosis." As typical cases of such multiple symmetrical diffuse lipomata nearly always occurred in men, and as cases like that now shown by Dr. McMullan occurred almost only in women, were not these two groups of cases perhaps only different manifestations of the same disorder in the two sexes? Yet he had shown at the Clinical Society one genuine case of diffuse lipomata in a woman.[1]

Dr. G. MCMULLAN, in reply, said the nervous symptoms, pain, and numbness preceded the appearance of the fat. The patient was above the average mentally. Her temperature had not been subnormal, nor her pulse below 80 nor over 90. The dryness of her skin had improved. As to the suggested connexion of the condition with the menopause, the menopause in this case occurred very suddenly eighteen years ago, but no unusual symptoms were associated with it. The patient was certain that her arms had increased in size during the last five years. He thought there was a relation between the pancreas and the thyroid, and it would be interesting to try combinations of those extracts, as the pancreas played an important part in the metabolism of fats.

A Case of Rheumatoid Arthritis with Albuminuria, and with Great Enlargement of the Liver.

By W. P. HERRINGHAM, M.D.

MALE, aged 15, has been an in-patient several times since November, 1905, and is a typical and severe case of the disease. The elbows and knees are greatly deformed and the movements much limited; the skiagram shows the bones to be natural. The shoulder movements are limited, the ankles rather swollen, and the fingers are

[1] *Trans. Clin. Soc. Lond.*, 1904, xxxvii, p. 220.

bulbous. The right hip has become affected lately, and it was chiefly for this new symptom that he was admitted in June, 1909.

In 1907 there was enlargement of the axillary glands, and also to a slight extent of the spleen; there was then no albuminuria, and the liver was not enlarged. When admitted in June, 1909, he was found to have two new symptoms: the liver had become greatly enlarged, reaching as low as the right iliac crest; it was smooth, hard, and painless. The urine has become albuminous: it is of low density, 1012—1007, and contains a large quantity of albumin. Under the microscope hyaline, granular, and epithelial casts have been seen. No amyloid reaction with iodine has been seen in the casts.

He is anæmic. Red cells, 4·5 millions; hæmoglobin, 55 per cent.; white cells, 8·8 thousand. The blood has been tested for Wassermann's reaction with a negative result. He has no sign or history of syphilis.

DISCUSSION.

Dr. W. P. HERRINGHAM said the point upon which he specially invited discussion was the nature of the enlargement of the liver. He had not seen it except in the present case, and he was not aware that it had been recorded. It was thought it might be amyloid, but there were no casts in the urine, and no ordinary cause of amyloid disease could be found. The patient had no other disease, and the question was if rheumatoid arthritis was in itself capable of producing amyloid disease.

Dr. POYNTON said that Dr. Beattie had found definite lardaceous degeneration of the liver in protracted cases of rheumatism and anæmia. In Dr. Still's cases the liver sometimes showed slight fatty enlargement.

Dr. PARKES WEBER thought that when the liver was enlarged in children, and when there was no obvious cause for the enlargement, it was probably true in nine out of ten cases that the enlargement was due to fatty infiltration. In rickety children with very large livers, the hepatic enlargement was nearly always due to fatty infiltration. The liver in the present case, however, felt unusually large for a fatty liver, and harder than he would expect.

Sir DYCE DUCKWORTH said the feeling and character and quality of the enlarged liver were certainly more like that of lardaceous or waxy liver than any other kind with which he was acquainted. If the present patient were under his care he would try the effect of full doses of iodide of potassium.

Dr. ROLLESTON thought that the enlargement of the liver was probably lardaceous. These cases of rheumatoid arthritis in children not uncommonly showed nephritis secondary to toxins absorbed from the joints, and it was therefore not unreasonable to suppose that this toxæmia might in exceptional

instances produce lardaceous change in the kidneys (instead of nephritis) and other organs. It was quite possible that the albuminuria in this case was due to lardaceous disease, for the absence of a lardaceous reaction in the casts did not exclude lardaceous disease of the kidneys.

Dr. HERRINGHAM replied that he did not think that the kidneys were generally lardaceous in these cases of rheumatoid arthritis, and that those cases which ended with nephritis generally showed ordinary nephritis and not lardaceous disease. He proposed to follow Sir Dyce Duckworth's suggestion and try large doses of iodide of potassium. That condition of liver had entirely developed in the last two years.

Impaction of Large Calculus in the Lower End of the Right Ureter.

By ALBERT CARLESS, M.S.

P. H. A., MALE, aged 29, admitted to hospital for supposed stone in the kidney. The history dated back one month only, but the patient admitted having lost weight during the preceding twelve months. One month before admission, after a long cycle ride, patient was seized with severe pain in the right loin and during micturition; no hæmaturia: pain lasted for one hour. A second attack occurred one week later, and was much more severe, lasting for some hours; it began in the right loin, but was referred down the inner side of the right leg and settled deeply in the right iliac fossa. Since then the patient complained of a sharp cutting pain in the hypogastrium, even when lying in bed, and some discomfort on micturition. The urine was passed at normal intervals; it was cloudy, but contained no blood. The pathologist reported that there were no tubercle bacilli, but a little pus and large numbers of the *Bacillus coli*. The patient looked ill and anæmic. The abdomen moved freely and was not rigid, but there was slight tenderness and resistance on deep palpation over the right iliac fossa. The right kidney was not palpable, but was slightly tender. On rectal bimanual examination a swelling could be detected high up on the right side, and this could be shown with a sound in the bladder to be outside that viscus. Radiographic examination showed a large shadow, dense and ovoid, on the right side of the pelvis, and just below the brim.

July 9, 1909, Operation: Median laparotomy for exploratory purposes. The stone was located behind the bladder just below the pelvic

brim, and extending upwards therefrom was the ureter, as large as a dilated coil of small intestine, filled with pus. The stone was movable laterally with the ureter, and could be displaced towards the right. An assistant keeping his hand in the pelvis and pushing it over, a lateral incision was made in the iliac region; the peritoneum was displaced forwards, and the distended ureter easily reached. A small incision was made over the stone, and the pus let out little by little, being swabbed away as it escaped. Finally, the incision was enlarged sufficiently to deliver the stone together with a gush of pus. The wound in the ureter was partially stitched up, but a large drainage-tube was introduced into and up the ureter so as to drain the kidney. A counter-opening for drainage purposes was made in the loin, and the wound in the iliac fossa partly sutured after packing. The wound did not heal very quickly, but finally the patient did well. The right kidney is evidently not entirely destroyed, as urine escaped from the wound for a time, as indicated by the methylene-blue test.

The patient is now quite well and the wounds have healed. There is a ventral hernia in the groin, but this can be dealt with later on. The stone weighed when fresh 803 gr., and is evidently composed of uric acid.

DISCUSSION.

Mr. THOMSON WALKER said he had looked up the literature concerning large ureteric stones, and the largest he had found was one recorded by Bloch, which weighed 816 gr., very little heavier than that shown by Mr. Carless. Another described by Fedoroff weighed 780 gr. Mr. Carless's specimen was a small stone for its weight. A stone which Mr. Walker had removed from the lower ureter was not very much smaller, but it only weighed 224 gr. It would be agreed that such a large mass could not pass down the ureter without causing more symptoms than were present in this case; so it must have come down as a small stone and grown in the ureter. This patient had a stone in his ureter with a sac of pus behind it, yet he had very little pus in his urine. This pointed to the presence of a stricture below the stone, a not infrequent complication of such large fixed calculi. But the fact that all his wounds had completely healed was against the idea of stricture; because if there were a stricture of the ureter a fistula would remain. It might be taken for granted that in such cases the kidney of the same side was destroyed. The fact that a kidney was discharging fluid did not necessarily mean that the organ was functionally active. The fluid might be merely water tinged with pus. He thought it might be concluded that in this case the kidney was very seriously damaged, if not destroyed. With regard to the exploratory laparotomy, there

was enough evidence of stone in the ureter to obviate a laparotomy. He would have gone straight on to the extraperitoneal operation on the ureter. Most surgeons had given up removing these stones through the peritoneum, as a good exposure of the ureter could be obtained without opening the peritoneum. There were two dangers in the transperitoneal method: first, that most of these cases were septic and the peritoneum would be infected; and, secondly, that the urine might leak afterwards into the peritoneum. Though the peritoneal route had often been used, the mortality of uretero-lithotomy was very small—namely, 1·6 per cent.

The PRESIDENT said that two years ago he removed a large stone from the right ureter of a woman in hospital. It was about the size of the thumb, cigar-shaped. He saw the patient more than a year afterwards, and there was then no evidence of stenosis of the ureter. But it was not a jagged stone, though slightly nodular and irregular.

Mr. CARLESS, in reply, said that the intraperitoneal exploration was done merely to assist him in the subsequent removal, and in this it proved of the greatest service. There was no idea of any attempt to remove the stone across the peritoneal cavity. The exploration enabled him to see the condition of affairs and how the stone could best be reached, and it did not add to the risk of the operation.

Case of Trephining for General Epilepsy after an Interval of Three Years.

By W. G. SPENCER, M.S.

THE patient was first seen in consultation with Dr. F. Norman in 1899, and was then aged 13. He was born after a premature breech presentation, and was a weakly child. The depressions on the skull were first noticed at the age of 1½. The first fit occurred when the child was aged 8, without any assignable cause; he was found lying on his face, and afterwards was semi-comatose. The fits recurred, sometimes with an aura in the shape of pain in the depressions of the skull, but more often with merely a giddy sensation just before the fit. The fits were general, and there was no evidence of sequence of spasms. A shallow depression existed on each parietal bone midway between the bregma and the parietal eminence; it was 2 in. in diameter, less than ½ in. deep, and the inner margin was about ½ in. distant from the sagittal suture. Trephining was then advised. In 1904 the fits

persisted with no localizing signs, and trephining was again recommended.

In 1906 (aged 20) the fits still recurred at irregular intervals, generally without an aura, but exceptionally with some irritability. The fits were general without any localization, and were followed by a semi-comatose and drowsy state for some hours and by malaise for a day or two. He had not attended school and his education was backward, but otherwise there was no mental defect. He could not join in games, and had not been prepared for any occupation, but was well grown. The patient was now trephined, and the area of depressed bone on each side removed; the inner table was quite smooth, only a little convex inwards. The dura mater corresponding to the trephine hole was excised; quite normal and not adherent to bone or pia mater. The exposed brain looked normal. The operation caused no disturbance, the wound healing by first intention. There has been no fit since the operation except a slight one in April, 1909, after smelling varnish. He is sometimes deaf, or does not understand what is said. He has made progress with a tutor, but is still behindhand in education. He has attained considerable proficiency in photography. He has kept to a vegetarian diet.

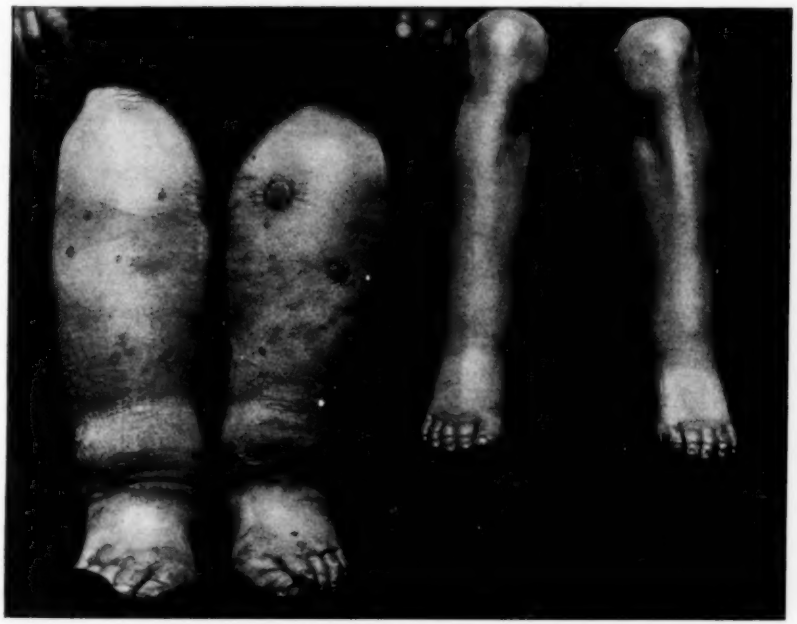
Two Cases of Congenital Œdema with Cardiac Defect in Mother and Daughter.

By F. J. Poynton, M.D.

Case I.—The mother, aged 48. In 1863 Dr. Duffin saw her at the age of 1½, and in her out-patient letter of that date noted congenital œdema and a cardiac bruit. Eleven years later he again noted the œdema and a systolic murmur. The œdema has slowly increased to the proportions seen at the present date. It disappears in great degree on rest in the horizontal posture. The heart shows an enlarged area with forcible impulse; a mitral systolic murmur and an accentuated pulmonary second sound. The right kidney feels movable. The urine is apparently normal. The particular interest of this and of the other case lies in the occurrence in each case of a cardiac lesion which is presumably congenital in origin.

Case II.—The youngest daughter, aged 7. She is the only one of seven children with this condition. Only three children are alive now.

Out of these, the youngest son, has also been attending hospital under Dr. Poynton for a coli infection of the genito-urinary tract. He shows no œdema and the heart is apparently normal. This girl appears well in all respects except for œdema of the feet and legs, which was noted at birth, and which has slowly increased to the present degree. It is greatly diminished by rest. The area of heart is slightly increased, the impulse is forcible, and both at the apex and base there is an obvious systolic murmur, somewhat musical in character, just internal to the vertical nipple line.



Congenital œdema in mother and daughter.

Dr. PARKES WEBER said he supposed that these cases would be regarded as examples of so-called trophœdema of the lower extremities, a name which he used simply because most cases had been described under that heading. Chronic œdema of the same kind, in many of the published cases, had been limited to one lower extremity, sometimes the whole limb being affected from the groin downwards, sometimes only the portion of the limb below the knee.

Case of Resection of the Bladder for Malignant Disease.

By J. W. Thomson Walker, F.R.C.S.

S. C., aged 55, warehouseman, came under my care at St. Peter's Hospital in August, 1909. Nine months previously he began to suffer from pain across the back, and three months later blood appeared once in his urine. After an interval of two months the hæmaturia recurred, and continued until I saw him. The quantity of blood in the urine varied. At times the whole of the urine was a dark coffee colour, and irregular clots were passed; at other times the urine was clear at the commencement of the act and finished with a few drops of pure blood. Since the hæmaturia first appeared there had been increased frequency of micturition to two hours during the day and twice at night. There had been difficulty in micturition on one or two occasions from the impaction of a clot in the urethra. He had not lost weight. On examination he was a fairly developed and well-preserved man. The abdominal wall was thick; the kidneys were not palpable or tender; there was no enlargement of the groin glands. From the rectum the prostate was normal in size, consistency, and mobility; no enlarged glands could be felt in the pelvis.

On cystoscopy a large growth occupied the left lateral wall of the bladder and projected into the cavity (fig. 1). The growth extended from the outer margin of the left ureteric orifice almost to the apex of the bladder. The lowest part of the tumour was close to the outer margin of the left ureteric orifice, and the mucous membrane on each side of the orifice was folded and dragged upon by the growth. The growth at this part was covered with innumerable, small, round bosses of yellow-pink colour, with smooth, light-reflecting surface and semi-transparent appearance, between which fine vessels could be distinguished. The appearance of this part of the growth was not unlike a "white" raspberry. The more distant part of the growth towards the upper part of the bladder was nodular and opaque, with a smooth, tough-looking, dull-red appearance. The growth overlapped its base of attachment, and the mucous membrane of the bladder, right up to the line at which it became hidden under the rounded mass, was healthy and showed no sign of infiltration. The base of attachment seemed to be a broad track leading from the left ureteric orifice

outwards, upwards and forwards, reaching almost to the middle line about midway between the internal meatus and the apex of the bladder. The remaining area of the vesical mucous membrane was healthy. The efflux from both ureters was full, regular, and clear.

I performed the following operation on September 8, 1909: The bladder was distended with fluid, and exposed by a vertical suprapubic incision 3 in. in length. The left rectus was cut across for about half its breadth 2 in. above the pubes, and the peritoneum was stripped from the left side of the bladder. The bladder was now opened by a vertical incision in the middle line and a suture passed through each lip of the wound. The catheter was retained in the urethra, and acted during the operation as a guide, and at the end of the

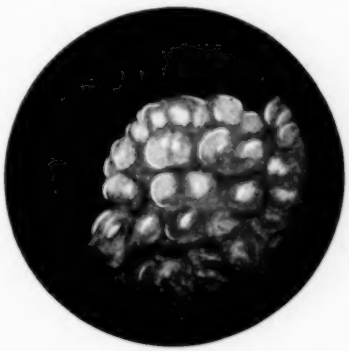


FIG. 1.

Cystoscopic view of growth.

operation as a means of flushing the bladder with hot lotion. The patient was now placed in the Trendelenburg position. With the help of my bladder-retractors a good view of the interior of the bladder was obtained. One broad retractor was placed on the side opposite to the growth, and a narrow one on the same side as and in front of the growth. A roll of gauze was tucked into the post-trigonal space. The upper end of the growth extended to within an inch of the cystotomy wound. A catgut suture was introduced through the thickness of the bladder wall above the growth, and served to steady it during the earlier part of the incision. With blunt-pointed curved scissors the bladder wall was cut through transversely above the growth,

and from this the incision was continued downwards, skirting the posterior aspect of the growth and leaving a margin of about ¼ in. of mucous membrane and muscle attached to the growth. Oozing from the mucous membrane was controlled by pressure, and an occasional spouting vessel was picked up with forceps. As the dissection proceeded, traction sutures of catgut were introduced through the thickness of the bladder wall at its cut edge, at intervals of about ½ in. These traction sutures served to steady the bladder wall, and bring it into view for further dissection. When the dissection had reached the base of the bladder the growth was pulled backwards, and the incision carried down the anterior aspect in the same manner, and traction sutures were placed in the bladder wall at similar intervals. The growth could now be raised, and a nodule the size of a marble was felt attached to the under surface of the mass outside the bladder wall and surrounded by perivesical fat. This was raised, with the rest of the mass, by blunt dissection. Thick strands of tissue passing into the mass were isolated with the finger and cut across with scissors. At this period there was profuse venous hæmorrhage from the veins of the left branch of the prostatic plexus. This was controlled by packing a roll of gauze down behind the mass outside the bladder. The mass was now raised, and the incision carried round its lower end, passing on to the trigone, and including the ureteric orifice. The whole mass was then removed and the gauze packing behind it taken out. The bridge of bladder wall, between the upper end of the gap left by the removal of the growth and the vertical cystotomy wound, had been much narrowed during the manipulation, and was now cut across. The bladder was thus split from the trigone to the cystotomy wound in the median line. The muco-muscular wall shrank away from the gap so that an area some inches in breadth was opened up, and into this the peritoneum bulged. The roll of gauze in the post-trigonal area was removed, and the edges of the retracted bladder wall were raised by means of the traction sutures. The upper traction-suture of one side was now threaded in a fully-curved needle and passed through the whole thickness of the bladder wall at the opposite edge of the wound and tied. This was drawn upon, and the next traction suture treated in the same manner, so that the gap was closed from above downwards, and the deeper part of the wound gradually raised so that the stitches could be more easily inserted. At the basal end of the gap additional stitches were required and were introduced by means of a fully-curved needle, transversely set on a metal handle,

which could be bent to any angle. At this time a search was made for the ureter, which had been cut across. The cut end could not be found, and the lower inch of the bladder wound was left open so that the urine from the ureter might escape into the bladder. The catgut sutures were now cut short and the retractors removed. The cystotomy wound was closed by a row of catgut sutures, passing through the whole thickness of the bladder wall. A large rubber drainage-tube, 1 in. in diameter, was placed in the lower end of the bladder wound. The left rectus was repaired by a number of thick catgut sutures, and the two recti then brought together above the tube by means of two catgut sutures. A small calibre drainage-tube was placed between the large bladder-tube and the pubes to drain the prevesical space; the skin was closed with silkworm-gut sutures. The patient was now lowered into the horizontal position and a pint of hot boracic solution passed through the catheter. This flowed unstained through the bladder drainage-tube; the catheter was then removed. The patient was placed on his right side and kept in this position for several days. The tubes were removed on the fourth day and recovery was uninterrupted.

In this case I did not use any form of apparatus for removing the urine from the bladder by suction, or syphonage, or draining the urine away by overflow.

The specimen shows a large mass of growth, the lower end of which is covered with small round bosses and the upper and larger part nodular and smooth. This is set in a plaque of mucous membrane and bladder muscle, leaving a clear margin of about $\frac{1}{2}$ in. round the tumour. The rounded mass of growth is rolled over this surrounding mucous membrane. Beneath the plaque of bladder muscle is a large nodule surrounded by fat.

Microscopically, the growth was a carcinoma. The bladder was examined with the cystoscope eight weeks after the operation (November, 1909). On the left side of the bladder, a little above and behind the position usually occupied by the left ureteric orifice, there was a large oval opening which would admit the point of the forefinger. The long axis was vertical, and the opening led into a gradually narrowing funnel. From this urine was discharged. This was the new opening of the left ureter. From the upper part of this a slight depression passed upwards and to the left, and indicated the position of the bladder wound (fig. 2).

I shall only refer to three points in the technique of this operation, and say a word in regard to the choice of operation. One of the most

important points in carrying out such an operation is the proper exposure of the growth. The bladder is a deeply placed, collapsible organ, and the growths have a free blood-supply. Imperfect exposure under these conditions means a confused and incomplete operation. The usual vertical median suprapubic incision suffices for most operations on the bladder. I have only used a transverse incision in cases in which a previous operation had left much scar-tissue, or when the patient was unusually stout; 3 in. is a sufficient length in all but stout patients. It may be said that the degree of exposure does not increase with the length of the incision. There is no harm in a longer incision if it be properly closed afterwards, but it is unnecessary, and only exposes the peritoneum-covered bowel, not the bladder. Much room may be gained by cutting the rectus muscle transversely for half its breadth about 2 in. above the pubes. I usually do this on the side on which the bladder growth is set.

The Trendelenburg position is an indispensable factor in the exposure of bladder growths; without it no resection of the lateral or posterior walls need be attempted. Even with the patient in the inverted position, the bowel may give trouble by pushing up the posterior wall. This may be diminished by careful preparation of the bowel before the operation, and by avoiding ether as an anæsthetic. I find it of much assistance to tuck a roll of gauze into the post-trigonal area, and this serves also to soak up blood. It may be renewed several times during the operation. Good retractors are necessary; those which I designed for bladder retraction push the lateral wall of the bladder outwards, and at the same time pull upon the abdominal wound. This is attained by a combination of traction and tilting, and while the retracting blade is specially shaped to fit the abdominal wall and bladder, the special action is obtained by an almost vertical handle with a recurved end which is grooved on the under surface. The handle is grasped and held vertically, and the thumb is pushed up into the recurved portion and provides the tilting action. A broad retractor is placed on the side opposite to the growth and a narrow one on the same side. A good head light is necessary.

The second point is that the ureter was cut across at the operation, and the lower part of the bladder wound left open to allow the urine to drain into the bladder. This is not, of course, the ideal method, but here it has given a good result. In such cases it is better either to perform a preliminary operation for transplantation of the ureter to another part of the bladder, or to place a catheter in the ureter at the

time of the operation and pass a silk thread through its wall when it is cut across and suture it in the bladder wound.

The third point to which I wish to draw attention is the method of suturing the bladder. I place traction sutures in the cut edge of the bladder while the resection is in progress. These form an invaluable guide to the further section of the wall, and raise the bladder wall considerably. They are especially useful, after the growth has been removed, as a means of identifying the cut edge of the bladder, a less easy matter than might be imagined. I use them as sutures by threading them on a curved needle, and much time is thus saved. I make no attempt to avoid the mucous membrane in suturing, but

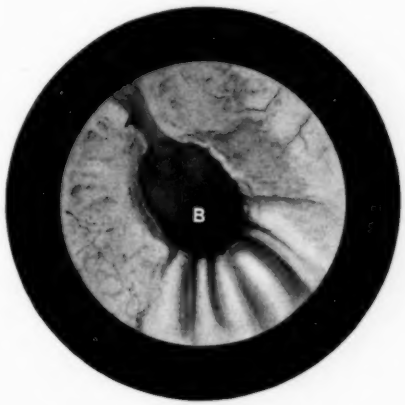


FIG. 2.

Cystoscopic view of bladder after operation. **A**, Scar of resection wound; **B**, Opening of left ureter.

deliberately include it in each stitch and knot the stitch inside the bladder. Experience has taught me that catgut sutures will be absorbed or cut out by the end of a week or ten days, and as I drain all my cases of resection the stitches have all disappeared before the bladder has closed. Concretions are quickly formed on silk and I never use it in the bladder. I close the resection wound from above downwards, bringing the next suture into view by dragging gently on that first tied.

Lastly, a word may be added as to the choice of operation in cancer of the bladder. The choice lies between resection of the bladder wall and cystectomy. I shall state my position in the matter by saying

that resection of the bladder should be performed wherever possible, and cystectomy reserved for cases in which resection is impossible from the position and extent of the growths. From this statement are of course excluded all cases in which there is infection of the lymphatic glands of the pelvis or abdomen, or metastatic deposit elsewhere, or in which the patient is unfit for operation from feebleness or some other reason. The question then resolves itself into what cases are unsuitable for resection. These are cases in which the growth affects the base of the bladder, so as to involve both ureters or the urethra, or has spread to the prostate, and also cases in which the area of bladder affected is so extensive that no hope can be entertained of constructing a bladder out of the remnant. I believe that the number of cases of resection of the bladder wall will increase, and that cystectomy will become an even rarer operation than it now is. The necessity for early and accurate diagnosis of diseases of the bladder by means of cystoscopy must become more and more fully recognized by the profession. When this has come to pass, resection of the bladder wall will suffice to remove the whole growth without the danger of recurrence.

Excavating Rodent Ulcer of the Face under Treatment by Radium.

By Charters J. Symonds, M.S.

Male, aged 65, was admitted into Guy's Hospital on October 4, 1909. The right cheek was destroyed by a deep ulcer, the floor of which was formed by the posterior wall of the antrum. The skin around the ulcer was raised for some distance, as in rodent ulcer. The infiltration was extrinsic, somewhat elastic, and reached in places nearly 1 in. in breadth. The lower eyelid was swollen, but not markedly infiltrated. The bone was covered by granulations and bled easily. The nasal cavity was fully exposed, and, either by operation or disease, the turbinal bodies on the right side had been removed. Looking into the mouth, there was a gap between the cheek and the palate, communicating with the cavity above. The mucous membrane of the hard palate was clean-cut, and not infiltrated. There were no enlarged glands. A piece of the margin was removed for microscopical examination, and showed the typical characters of rodent ulcer.

The man states that twenty-seven years ago a small, dry scab appeared on the face to the right of the ala nasi. This came off from time to time, and did not cause any inconvenience. Sixteen years ago he came to the out-patient department with a small ulcer, thought then to be typical of rodent ulcer. This was removed under local anæsthesia. On three or four other occasions a small, dry spot, from which epithelium scaled off, was removed during the next two or three years. On June 8, 1898, eleven years ago, he was admitted, under the late Mr. Davies-Colley, with an ulcer $\frac{1}{2}$ in. by $\frac{1}{4}$ in. in the old scar, corresponding with the naso-labial fold. The ulcer was widely removed, part



Excavating rodent ulcer.

of the ala being taken away and the gap closed by a flap. The microscopical section showed the characters of rodent ulcer. In July, 1904, he presented himself with a deep ulcer in the old situation. He covered this with a piece of plaster, and so was enabled to keep at his work. The case being thought suitable for the Röntgen rays, fourteen applications were made, as a result of which the excoriation increased in depth to such an extent that the method was abandoned. In September, 1904, a somewhat extensive operation was performed. The incision extended from the inner canthus down the side of the nose into the lip, and over the malar bone. The anterior antral wall was removed and the nasal cavity fully opened. The microscopical characters again showed the

structure of rodent ulcer. In November, 1904, the growth appearing to have been completely removed, the large aperture was closed by a plastic operation. For some time there was no appearance of growth in the skin, but when seen from time to time it was noticed that the skin was becoming thinner, and later broke into apertures, though free from infiltration. Somewhere in 1906 the skin again showed infiltration, and a further operation was carried out. He did not long remain free, and was seen from time to time and was finally admitted with a view of being treated by radium, no surgical operation holding out any prospect of success.

The decrease of the marginal growth is very well marked, and this is especially so where the gap remained after the removal of a piece for examination. The bone is cleaner and bleeds less easily. There has been some smart reaction in the surrounding skin, with œdema of the eyelids and conjunctivitis.

There can be no doubt of the steady improvement in this extensive case, and it is clear that radium has proved more successful than the X-rays. The treatment is carried out by my colleague, Mr. Iredell, in the actino-therapeutic department at Guy's Hospital.

The man will be shown at a later meeting.

A Case of Pachydermatocele.

By Archibald Smith, F.R.C.S., and James McDonagh, F.R.C.S.

The patient first came under observation at the West London Hospital on November 19, complaining of hernia, for which he is shortly to be admitted. It was noticed that he had a swelling on the left side of the scalp, extending from behind, midway between the ear and the occipital protuberance, upwards and forwards. The swelling is divided into three parts, which are soft and semi-fluctuating, and tend to fall towards the ear in the form of folds. Between the middle and posterior parts of the swelling on deep palpation is to be felt a vertical cord about the size of a crow-quill—evidently a portion of the trunk of the occipital nerve—considerably enlarged and tender on pressure. Sections, prepared from a fragment of the growth removed under local anæsthesia, were exhibited.

Arterio-venous Aneurysm with Exophthalmos following an Injury to the Head.

By Frederic Eve, F.R.C.S.

Male, aged 39, was admitted to the London Hospital on September 27, 1909. Three weeks previously, while getting off a fast-travelling motor-'bus, he lost his foothold and fell backwards on the back of his head; he was unconscious for about a minute, and vomited. Next day he suffered with increasing headache, which finally compelled him to take to his bed. The day after the accident he noticed double vision and, later, the left eyeball began to swell and cause pain. Condition on admission: Left eye, proptosis about 8 mm.; ophthalmoplegia externa; ptosis; only very slight movement of levator palpebræ superioris; no lateral movements of the eye, and slight upward and downward movements. Left pupil reacts to light and accommodation; vision good. Pulsation is felt on placing a finger upon the eyeball, and especially near the inner canthus (angular vein), where a thrill is also felt. A loud systolic bruit is heard on placing a stethoscope over the globe, left supra-orbital region, and left temple. It is audible to the patient. The bruit and pulsation disappear on compressing the left carotid artery. Fundus: pulsation of veins, no swelling of disk. T: + 1. Arteries normal. A radiograph shows a fissure in the left parietal region, which may be a fracture.

His condition at present date (December 6) is as follows: The proptosis is the same; the pulsation has much diminished; adduction of left eye is fair; abduction is lost; the left pupil is smaller than the right, but reacts to light; the edge of the disk is well defined; the physiological cup is filled up by a non-inflammatory exudate; + 4 is required to focus centre of disk.

Treatment advised: Rest in bed; potassium iodide and Tufnell's diet. The patient takes meat once daily, and only one pint of fluid.

Partial Luxation of the Odontoid Process.

By Frederic Eve, F.R.C.S.

Female, aged 17. In March, 1908, she was standing on a chair, reaching for an article upon a shelf, when she overbalanced, but saved herself by clinging to the top of a door which stood near by. Her head fell suddenly back and she heard a "crack" in her neck. She had a good deal of pain in the neck, and went to bed for a fortnight. From that time her neck became stiff. She was admitted to the London Hospital in October, 1908. The head was bent down towards the left shoulder, and she suffered much pain. Extension was applied for a month, with improvement, and she was discharged with a jacket and head support.

Present condition: Flexion of the neck is good, extension fair, rotation much limited. A radiograph shows considerable displacement of the odontoid process backwards. No signs of nerve injury, and no thickening suggestive of tuberculosis. The movements of the neck have become freer.

Transverse Fracture of both Patellæ, with much Separation of Fragments, treated by Operation.

By Frederic Eve, F.R.C.S.

Lighterman, aged 41, admitted to the London Hospital four years ago with transverse fracture of both patellæ. Accident occurred nine months previously, and he had been treated elsewhere by strapping for six weeks. A subsequent fall determined complete inability to walk. On admission the patellar fragments were separated on each side by about 3 in., but were freely movable. Operation on January 13, 1906, consisted in freely dividing the insertion of the quadriceps into each upper fragment; the fibrous bond of union was dissected away and the bony surfaces freshened; the fragments were then wired. Healing by first intention. The patient was able to walk out of hospital, and now—four years later—he has regained good power of extension and can do his work as a lighterman. The gaps in the quadriceps tendons have partially filled up.

A Case of Recklinghausen's Disease.

By F. Parkes Weber, M.D.

The patient, a married woman, now aged 45, had enteric fever at the age of 18. After that little lumps began to develop in her skin, and finally became almost too numerous to count. As a girl she suffered at times from abdominal pains and vomiting. When first seen, about six



Figure showing the skin of the back in January, 1904. One of the few patches of cutaneous pigmentation is seen at **A**.

years ago, she was subject to abdominal pains, which she said often preceded the menstrual periods. There were innumerable molluscous tumours, of various sizes, on the trunk (*see* figure) and limbs, and there were some pigmented patches on the skin of the back of the thorax. In July, 1909, at the patient's request, one of the tumours at the back of the left leg, just above the tendo Achillis, was excised, because it was sometimes tender or painful. Microscopically it showed no nerve fibres, but it was probably connected with a small sensory nerve, as its excision was followed by temporary impairment of sensation over a limited area. According to Mr. J. E. R. McDonagh, the microscopical section showed

the typical structure of a neurofibroma—namely: (1) loose connective tissue rich in cells with very small nuclei; (2) dense fibrous strands in the above-mentioned loose connective tissue; (3) great excess of fibrous tissue in the walls of the minute blood-vessels. No subcutaneous tumours of nerve trunks were felt. Hypodermic injections of fibrolysin were tried for a short period, but without any decided result.[1]

A Patient a few months after Omentopexy and Peritoneal Drainage for Chronic Ascites connected with Hepatic Cirrhosis.

By F. Parkes Weber, M.D.

A woman, aged 49, formerly enjoyed good health. She was married at the age of 28, and had eleven children, nine of whom were still living and healthy; the other two died early. Swelling of the abdomen was first noticed in December 1908, and she then became short of breath and confined to bed. Before she was admitted to the German Hospital (July 14, 1909) paracentesis abdominis had already been performed six times. She had been accustomed to drink beer, but apparently not in excess.

On admission the abdomen was distended with fluid, the lower extremities were œdematous, and there were bronchitic signs and some dyspnœa and cyanosis. The heart showed nothing abnormal. The urine, of rather high colour, was free from albumin and sugar. There was no jaundice. During the first five weeks in the hospital paracentesis abdominis had to be repeated four times; the quantity of the ascitic fluid withdrawn on these occasions varied from 6,000 to 9,500 c.c., and its specific gravity (twice estimated) was found to be 1·010. After tapping the belly the liver could be felt enlarged and hard, reaching four finger-breadths below the costal margin. The patient's temperature, though occasionally slightly raised in the evenings, never exceeded 100° F. The pulse varied between 64 and 88. The quantity of urine was decidedly below the normal.

Omentopexy was performed by Dr. E. Michels on August 20, and the free bleeding from the abdominal walls at the operation seemed to show that considerable collateral venous circulation was already established. After the operation the ascitic fluid soon reaccumulated

[1] Cf., however, F. E. Fenton, *Proc. Roy. Soc. Med.*, 1909, ii (Clin. Sect.), p. 33.

and was removed by paracentesis or by a small incision on September 4, September 14, and September 22, 6,200 to 8,000 c.cm. being withdrawn on each occasion. On October 1 Dr. Michels, after letting out the ascitic fluid by a small incision near the umbilicus, inserted a tube (a small catheter) for "permanent drainage." This tube was removed after a few days when the fluid had ceased to flow. On October 12, 6,000 c.c. were again let out by incision and a catheter was again inserted for peritoneal drainage, but it had soon to be removed, as fluid did not come away. About this time (after October 12) there was moderate pyrexia (up to 100° to 101·5° F. in the evenings), but after October 21 the temperature did not reach 100° F. No more tapping was needed after October 12, though the fluid at first reaccumulated. By November 8 the ascites was obviously diminishing, and by November 15 the signs of ascites had for a time completely vanished. The patient's general condition was satisfactory. The daily quantity of urine was about normal. The temperature was generally about 98° F. in the morning and 99° F. in the evening. The liver (November 25) felt hard and was greatly enlarged, the lower edge reaching down to the anterior superior iliac spine. The splenic dullness commenced above at the eighth rib in the mid-axillary line, and the lower edge of the organ could be felt about 2 in. below the costal margin. A little ascites was again noted in the early part of December.

It should be observed that in this case, as in the case shown by Dr. Weber on May 14, 1909, the decided improvement followed Dr. Michel's peritoneal drainage rather than the omentopexy operation itself. The moderate fever immediately preceding the favourable result was likewise a point to be remembered. In regard to chronic ascites and the question of operative treatment beyond simple tapping, cases of hepatic cirrhosis might perhaps, as stated on a previous occasion, be roughly divided into the two following groups: (*a*) Patients who for some reason (for instance, the presence of old perihepatitis and perisplenitis and extensive spontaneous omental adhesions) have the collateral venous circulation well established, and do not readily develop ascites, but are especially liable to hæmatemesis from dilated œsophageal or gastric veins. The liver is generally decidedly enlarged in this group of cases. (*b*) Patients with a poor collateral venous circulation, who develop ascites early. The main object of omentopexy and peritoneal drainage should be to convert patients of class *b* into patients of class *a*. In the present case no contra-indication existed, but, on the contrary, the patient's general condition appeared particularly favourable for the operative interference.

Gumma of the Breast Simulating Malignant Disease.

By H. J. Paterson, F.R.C.S.

Female, aged 26, admitted to hospital for a swelling of the right breast in November, 1901. It had been noticed for three months and had given little or no pain. Patient was thin and very anæmic, looked very ill, and had lost much weight. The upper and outer quadrant of the gland was hard and irregular, not adherent to the skin or deep fascia; no enlarged glands in axilla. Explored under chloroform and found to be densely hard; histological examination at the time suggested a sarcoma. A complete Halsted operation was therefore at once undertaken. Two weeks later the suture-holes became ulcerated and small red nodules appeared in the surrounding skin; gradually the skin bordering on the line of incision became brawny and infiltrated, and the scar itself became ulcerated. The ulcerated area slowly spread, and the axillary and supra-clavicular glands became enlarged. Fourteen weeks after operation a smooth, hard swelling developed on the sixth rib in the axillary line. Mercury and iodide of potassium were administered and the mischief quickly cleared up. Three years after operation she was in excellent health. Further investigation of the breast demonstrated the inflammatory nature of the swelling and corroborated the opinion as to the gummatous nature of the tumour.

Mr. H. J. Paterson said he would be glad if any member could tell him how a similar mistake could be avoided in future. He could feel nothing in the axilla either before or at the time. He removed the axillary glands, but they were not enlarged. He thought it was sarcoma. He had a section cut, and the pathologist reported that his diagnosis was correct. The Wassermann reaction was not tried because it was not then known.

Case of Fracture of the Base of the Skull. Operation: Recovery.

By C. A. Ballance, M.V.O., M.S.

W. S., male, aged 29, was admitted to St. Thomas's Hospital on October 14, 1908. Patient was knocked down by a motor-car and brought at once to the hospital. He was in a semi-conscious state and

bleeding freely from both ears. When seen by me three hours later he was completely comatose, the respiration was Cheyne-Stokes in type and the pulse was slow and very irregular; the bleeding from both ears was very free. He was taken at once to the theatre.

Operation: The temporal muscle on the left side was split vertically and a circle of bone 1 in. in diameter was removed by the trephine, exposing the dura at the lowest part of the middle fossa. There was a little blood effused between the dura and bone. The dura appeared dark and bulging; it did not pulsate. On incision of the dura about one teaspoonful of blood-clot was forced out by the intradural pressure. The temporal muscle was then allowed to resume its position so that, as the dura was not sutured, drainage of cerebrospinal fluid and blood could continue to take place under it. An exactly similar operation was performed on the right side and with the same result; about the same amount of clot oozing through the incised dura.

Patient was conscious next day and made a good convalescence. For three weeks he had incontinence of urine and fæces. During convalescence it was noted that he talked incessantly. Patient resumed work on the railway in April, 1909.

December, 1909: Patient is perfectly well. He never has a headache, but says he sometimes has difficulty in smelling things.

Cholecyst-duodenostomy for Acute Emaciation following the Formation of a Biliary Fistula.

By John D. Malcolm, F.R.C.S.Edin.

The patient, a female, aged 69, had an attack of jaundice, of three weeks' duration, at the age of 19, from which she recovered completely. After forty-four years of good health she suffered for six years from pain, nausea, and vomiting after food. A severe pain, felt in the gall-bladder region and radiating to the left shoulder, began about a month before the patient's admission to hospital, and jaundice developed a fortnight after this severe pain began.

A diagnosis of gall-stones was made, and on June 4, 1909, the gall-bladder was opened and drained. No stone was found nor any enlargement of the ducts. An attempt to pass a probe into the duodenum from the gall-bladder was unsuccessful. The highest temperature after

the operation was 99·2° F.; the highest pulse-rate was 100 to the minute. The wound healed perfectly, except where it was kept open by the drainage-tube. Bile escaped in large quantity, and there was no evidence that any passed into the intestine, the stools being quite colourless. Digestion and defecation gave much trouble and the patient rapidly became emaciated. In the third week after the operation the pulse, which had fallen to a normal rate, again quickened and became very feeble. It seemed certain that the patient would die unless some change was brought about, and the cause of mischief seemed to be the absence of bile from the intestine.

On June 25, just three weeks after the first operation, the abdomen was again opened and the scar-tissue was cut away. The fundus of the gall-bladder was separated from the liver to the extent of about ¾ in. to facilitate its displacement, and its open end was attached to the edge of an incision in the duodenum. The abdomen was closed without drainage and the wound healed without fistula formation, although there was for a few days a little suppuration close to the track of the temporary fistula which had not been sufficiently widely excised. The tone of the pulse was obviously better on the day following this operation, and the patient's nutrition steadily and rapidly improved from that time.

The case is brought forward to elicit, if possible, information concerning the cause of the extreme and rapid emaciation which took place. It is known that bile assists in the digestion of fat, but that, in animals, nutrition can be maintained as a rule, even although all the bile is diverted from the intestine, by placing a ligature upon the common bile-duct and by a formation of a biliary fistula between the skin and the gall-bladder. It has been shown[1] that a patient can remain well nourished in every way for more than a year although all the bile escapes from the body and none passes into the intestine, no special precautions being taken except that the amount of fatty food is limited. In the patient exhibited, it was believed that the common bile-duct was occluded and that no bile passed into the intestine after the fistula was made at the first operation. It seemed to be proved that in this case the absence of bile from the intestine was the cause of an emaciation that threatened to prove rapidly fatal, and the second operation at once arrested the loss of flesh and quickly led to a fresh formation of fat. The only beneficial change made by that operation was the reintroduction of the bile into the intestine. It therefore

[1] Mayo Robson, *Med.-Chir. Trans.*, Lond., 1890, lxxiii, p. 61.

appeared that some patients depended much more than others upon the presence of bile in the alimentary canal for the maintenance of their nutrition, but if this distinction existed the explanation of it was not clear.

DISCUSSION.

The PRESIDENT pointed out that the notes said "healing perfect," and asked whether the meaning of that was that the biliary fistula was closed.

Mr. CARLESS said he could give a very similar history in the case of a woman of middle life on whom he operated for gall-stones. A biliary fistula was established, which he expected to heal in the ordinary way, but it persisted, and the patient passed into a condition of extreme emaciation and malnutrition, her temperature for some time not rising above 96° F. Finally, she became so ill that it was felt the fistula must be closed at all costs. By using saline infusion before and during the operation, it was carried through successfully. At the end of the operation the temperature rose to 98° F., and there it remained whilst she made an uninterrupted recovery and quickly regained her former healthy state. He could, however, mention an exactly opposite case, that of a man aged 70, on whose bile-duct he had cut down three times and cleared it of stones and biliary mud. It seemed impossible to keep the bile liquid; the duct became blocked as soon as it was left to close. For this patient he had established a biliary fistula and he was now well, but passing all his bile out of the fistula in the side. The loss of bile did not seem to interfere with his nutrition in the least, and indeed the latest report was to the effect that he had put on a stone in weight recently. Those two cases seemed to suggest that there was some unknown factor connected with the loss of bile, and perhaps the narration would suggest to somebody an inquiry as to what was the meaning of such contradictory experiences.

Mr. MALCOLM replied that everything but the fistula was closed. The rest of the wound healed perfectly between the operations, and there was no evidence of bile in the intestines during that time.

A Case of Disappearing Abdominal Tumour.

By PERCY SARGENT, F.R.C.S.

(Exhibited by BERNARD A. CHEADLE.)

THE patient was a sweet-maker, aged 43, with a forty-eight-hour history of sharp attacks of pain in the epigastrium; no vomiting. A mass was felt in the right iliac fossa, very hard in consistency and of

irregular shape, filling up a greater part of the fossa. The patient was operated upon and a hard, very irregular growth, in every way simulating a carcinoma, was found in a coil of *small* intestine in the right iliac fossa. No glands were palpable, but there was also a small growth discovered in the omentum of like nature. A lateral anastomosis was performed, but the growth had to be left as the condition of the patient was too bad to allow of its removal. The wound healed by first intention, and a month later the patient was discharged from hospital. The patient came up again three weeks later, when I found that the tumour had increased to double its original size and the patient was rapidly becoming emaciated. There were, however, no bladder symptoms, nor any of obstruction. I then first tried potassium iodide and mercury, though not expecting any improvement. The patient came up again three weeks later, the tumour having decreased in size considerably. The dose was increased, and at the end of ten weeks from the start of this treatment no tumour could be felt, and the patient's general health had considerably improved; so that I am bound to consider that the growth must have been a gumma of the small intestine, although the naked-eye appearance and its consistency were those of a typical carcinoma.

Clinical Section.

January 14, 1910.

Mr. A. Pearce Gould, President of the Section, in the Chair.

Stricture of the Rectum treated by Fibrolysin.

By Heaton C. Howard.

Miss N. P., aged 40, lavatory attendant. When first seen in July, 1908, there was a hard fibrous stricture of the rectum and a large recto-vaginal fistula, the result of an operation for piles at a hospital.

She was sent to St. George's Hospital, and the following is an extract from the notes:—"July 27: More complete examination under anæsthesia reveals that there is a dense fibrous stricture surrounding the rectum, about 2 in. from the anus. There is an oval perforation of the recto-vaginal septum about 1½ in. by 1 in.; its lower limit comes within 1 in. of the perineum. The cervix uteri points backwards through this so that the external os lies in the rectum. Posteriorly there is a diverticulum, which admits the finger passing upwards behind the rectal stricture, between it and the sacrum." It was considered that nothing could be done for her except a colotomy, which she declined.

She came under my care again, and at intervals of two or three weeks I kept the stricture open by passing my finger into it, sometimes under an anæsthetic, but it was exceedingly painful to her. It was with great difficulty that she was able to continue her occupation, and there was no doubt the stricture was slowly getting worse.

June 12, 1909: Injections of fibrolysin were commenced, and continued about three times a week. Her condition at this date: A dense fibrous annular stricture about 1 in. from the anus, only admitting my finger under an anæsthetic; above this the recto-vaginal fistula about

¾ in. long and ½ in. wide; above this again were two tight bands stretching horizontally across the anterior wall of the rectum, with a large space between them, and another space beyond, so that I could not tell which was the bowel. There was a discharge of blood occasionally, and of purulent matter. There was great trouble in getting motions through the anus; in fact they came just as much through the vagina.

January 7, 1910: Fifty-fifth injection. Complained for a week or two of burning and smarting in the rectum on standing, but not on walking. Bowels acted yesterday without medicine; motion loose, a little came through the vagina; generally obliged to take aperients; discharge a little less.

Present condition of the parts: Annular stricture thinner than formerly and admits the finger easily; recto-vaginal fistula merely a small slit; first band just above this much smaller than before, and seems to be the anterior boundary of a cul-de-sac, of which the second band is the farther boundary—this band has lost the sharp edge and is smaller; beyond this band the finger enters the bowel easily. In the region of the second band on the anterior wall of the rectum is felt a dense hard mass of fibrous tissue.

Although this seems to be a severe case of pathological fibrosis, the following improvements are noted:—

(1) An arrest of the growth of the fibrous tissue.

(2) A certain amount of absorption has taken place in the annular stricture and the fibrous bands.

(3) The pain has been much less; in fact she could not have gone on much longer in June last without a colotomy.

(4) Motions are passed through the anus with little or no pain.

On account of these improvements she has been able to earn her own living. The fistula is now merely a small slit, but it is doubtful if this condition is due to the fibrolysin. Twelve injections of iodolysin have also been given, but without any benefit.

DISCUSSION.

The PRESIDENT (Mr. Pearce Gould) said he could confirm what Mr. Howard said in the latter part of his description. There was a very good sphincter ani, which contracted well on to the finger, and immediately above this there was a tight fibrous stricture, through which his little finger passed easily. Above that there was a dilated portion with very thick fibrous walls, and an aperture at the posterior part which he believed was into the bowel. There was a bulging of

the bowel towards the vagina anteriorly. The fistula was now a narrow slit. From the description in the notes at St. George's Hospital, it was evident that the patient had undergone considerable improvement. Perhaps, when she got better still, Mr. Howard would show her again.

Dr. LEONARD WILLIAMS had had experience with fibrolysin in another condition which might be of interest. A postman with apparently idiopathic sciatica had had a large number of remedies without any benefit, until injections of fibrolysin over the affected nerve were employed. Three weeks after commencing this treatment the patient, who previously could not put his foot to the ground, could easily walk across the ward. Probably the injections removed the contractions around the nerve sheath.

Professor WILLIAM OSLER, F.R.S., pointed out that, in order to form a satisfactory estimate of the therapeutic effect of fibrolysin, it was desirable to collect experiences as to the results following its use in an obvious lesion, such as cases of Dupuytren's contraction.

Mr. CARLESS said he had seen a clergyman a short time ago with well-marked Dupuytren's contraction of three fingers in both hands and a gouty history. He ordered iodolysin by the mouth, 20 drops three times a day and plenty of hot water. When seen three months later, the result was most satisfactory, as, although the condition was not cured, the fingers were very much more movable than formerly.

Mr. HERBERT TILLEY asked if Mr. Carless had seen his patient recently, to ascertain if the condition had recurred. It had been suggested that fibrolysin might be used for so-called chronic dry catarrh of the middle ear, in which the essential condition was the fixation of the stapes in the oval window, with possibly hyperplastic changes in the labyrinth. The literature seemed to show some extraordinary results in that very intractable condition. But such results generally occurred in connexion with dark, inacessible regions of the body. If he could see the scar of a large burn melt away under the influence of fibrolysin, he would be more tempted to make use of the preparation.

Mr. CARLESS, in reply to Mr. Tilley, said he had not seen the patient recently; but, as he was a very nervous man, he thought he would have seen him if there had been any increase in the trouble. The drug could be taken diluted with water and was usually well borne.

Mr. ARTHUR BARKER said he had experience of fibrolysin in only a few cases, but he had not seen any advantage from it at all. In a case of Volkmann's ischæmic paralysis in which it was injected, small sloughs formed opposite some of the points of injection; no good results ensued. In Dr. Leonard Williams' case the improvement might have been due to simple puncture of the part, for acupuncture was a recognized form of treatment of sciatica. He could not explain on any pathological grounds the improvement which Mr. Carless described in his case. Dupuytren's contraction was supposed to be a development of fibrous tissue at the expense of the fat, which was so abundant

in the palm of the hand, and which gave it its wonderful resiliency. It seemed to him incomprehensible that the administration of any drug by the mouth could restore that fat which had been lost by inflammation and replaced by fibrous tissue.

The PRESIDENT said that one of the most striking cases he had seen of the satisfactory use of fibrolysin was in a patient shown at the Fellowship examination of the College of Surgeons, when he was on the Court of Examiners. The case came from Guy's Hospital, the patient having a considerable contraction at the back of the thigh, so that the joint was bent up to about a right angle, and when he was brought down at a subsequent examination to show the result there had been astonishing improvement.

Dr. ESSEX WYNTER said he had only a limited experience of the use of fibrolysin. He knew a gentleman who many years ago was shot in one eye, and within three days developed sympathetic ophthalmia in the other. He had been blind for the greater part of his life. Recently, under the influence of fibrolysin, the patient was sure he could see much better than formerly. He stated this with some reserve, as ophthalmic surgeons assured him that under any new or special treatment a nearly blind man readily fancied he could see better. No doubt expectant attention was a potent factor.

A Case of Thoracoplasty for very large Empyema.

By J. E. ADAMS, F.R.C.S.

G. E. T., MALE, aged 23. Admitted to St. Thomas's Hospital under the care of Dr. T. D. Acland, on May 29, 1908, with three weeks' history of illness.

Examination: Signs of pleural effusion occupying the whole of left side of chest and displacing the heart to the right, so that its pulsation extended from the right margin of the sternum to the mid-axillary line.

First operation, May 30: Aspiration gave exit to seven pints of greenish-yellow, sweet-smelling pus. A few hours later 2 in. of the seventh rib were resected, and three pints more of pus escaped, and a quantity of thick fibrin was removed. A rubber drainage-tube was left in the wound. A pure culture of the pneumococcus was obtained from the pus, and the patient was treated with a stock pneumococcic vaccine. Three weeks later the discharge was very slight, but there was very little evidence of expansion of the lung. The heart was still displaced far beyond the right border of the sternum.

Second operation, June 26: Chloroform anæsthesia. Patient on right side with left arm drawn across chest. Long incision parallel to vertebral border of scapula and 1 in. behind it, terminating in original wound. Large portions of sixth to ninth ribs, together with intercostal muscles and parietal pleura, removed. Superficial muscles divided by primary incision freed from overlying skin. Lower muscle-flap pulled over tenth rib and sutured with catgut to diaphragm and base of shrunken lung. Scapula pushed into pleural cavity, and muscles attached to its vertebral border secured to lower part of lung with catgut stitches. Skin closed except for drainage-tube at lowest point of incision.

Result: Completely healed in ten weeks.

The present condition, eighteen months since the operation, is as follows: The left arm can be freely used; the spine exhibits slight lateral deviation, with convexity of the lower dorsal spine to the left. Skiagraphy shows that the base of the left lung is on a level with the upper border of the ninth rib; above this the lung is well expanded, and breath sounds are normal from the apex of the lung to the level of the scar, though they can also be heard for a short distance below it. The patient has been following his occupation for about a year, and enjoys excellent health.

DISCUSSION.

The PRESIDENT said that the interval between the first and the second operations in this case was much shorter than was usual between an ordinary drainage operation for empyema, and the extensive operation of thoracoplasty. The skiagram showed that portions of two ribs had been removed, and a little piece of a third. At the operation the scapula was placed inside the ribs and stitched down to the lung; but now it was outside the ribs. One could hardly attribute the expansion of the lung to the direct pull of the scapula. He had usually regarded thoracoplasty as an operation which consisted in removing the bony and rigid outer wall of the empyema cavity, so that the supple parts of the thoracic parietes might fall in and be in contact with the compressed lung. The present operation differed in an important way from the usual operation of thoracoplasty, though the successful result was undoubted. The deformity which resulted from those operations varied very much in different cases. In some cases, after extensive operation, the patient might be quite upright, with no more curvature of the spine than the present patient presented. In another case, with a not more severe operation, there might ensue a very distressing deformity, with very considerable curvature of the spine. In the present case it was striking that the opening and drainage of that very large empyema did not seem to have had any effect on the displacement of the

heart. The heart was said not to have moved over to the left at all before thoracoplasty was performed. He would be glad to hear why it was that the heart was so sluggish in moving over.

Mr. C. H. FAGGE congratulated Mr. Adams on the result of his operation, because he believed that in chronic cases requiring extensive operations the results from the surgical treatment of empyemata were little better now than they were twenty-five years ago. It was not uncommon to find, in large hospitals, several cases with discharging sinuses in which, if any extensive operation were undertaken, it was done some months after the original one, when a very dense thickening of parietal and visceral pleura had taken place, and this was a most important factor in determining the unsatisfactory result. It was therefore particularly fortunate that such an early radical operation had been employed in this case. He inquired whether the cavity which was found at the second operation was only the size of the area over which the ribs (6-9) were excised. If it were not so, he did not see why the cavity, which presumably was above, had closed without drainage and remained healed; for in his experience in a few such cases the difficulty had been that there was a track extending upwards behind towards the apex, often under that portion of the ribs which was covered by the upward extension of the erector spinæ, and in which, in order freely to excise the parietal pleura and ribs in that region, a very bloody operation was needed. Did Mr. Adams do anything to the visceral pleura? His own experience was that the best results had always followed free removal of the visceral pleura until what looked like lung was exposed—i.e., great slices of the visceral pleura should be removed with a knife over the whole exposed area. He would have liked to hear the experience of the senior surgical Fellows in the Section in the treatment of empyema by thoracoplasty, because he thought that if any statistics of a large hospital were taken they would be found to be much less satisfactory than one would suppose on casual consideration of individual experiences.

Professor W. OSLER said he could not agree with Mr. Fagge. He was brought up in the days when many of the empyema cases did not go to the surgeon, but to the post-mortem room. The latter was the place to inquire concerning the results of empyema. The cases were now found not in the post-mortem room but in the surgeon's clinic, because physicians diagnosed them early and sent them on to the surgeon. Cases would do better were it not the practice of the surgeon to perform the operation and then leave the case to be looked after by those who often did not properly understand the after-care of the condition.

Mr. ARTHUR BARKER said the case was a very admirable one, and the result depended a good deal on the early interference. Those who operated much in such conditions knew what a difference there was if operation was undertaken promptly. In the early stages of empyema the lung was not covered over with the enormous masses which were present in the old empyemata, from which the lung could not release itself. Only one difficulty was suggested in the case—

namely, that the heart was pushed over so much at the end of three weeks. Why did it not return more or less after the evacuation of the fluid? But he thought the more ready expansion in the present case was due rather to the early operation than to the fact that operation was done in the manner described. In the lungs of children affected with empyema, expansion was very common; but the story was different with the adult. No doubt the resiliency of the ribs and lungs was different. And children with the condition were more likely to be placed early in the surgeon's hands than adults, who naturally wished to postpone operation as long as possible.

Mr. ADAMS, in reply, said that a piece of the latissimus dorsi and some of the rhomboideus major was attached to the scapula. The muscles which passed from the scapula to the lung favoured expansion through the movements of the arm; the muscle which was pulled over the tenth rib and sutured to the diaphragm lessened the size of the pleural cavity. By these means the lung was encouraged to fill a pleural cavity of less than the original size. The interval between the first operation and the thoracoplasty had been only four weeks, therefore the lung was not covered by thickened pleura. It was a total empyema, and at the second operation the lung lay shrunken on the left side of the spine. But he could see that it was expanding slightly, and the visceral pleura was not thickened so as to prevent its movement; therefore there was no necessity to remove it. The heart showed no great tendency to return to its normal position until two or three weeks after the thoracoplasty had been performed. He agreed with Mr. Barker that the success of the operation was largely due to the fact that the second operation was undertaken early, and that the lung was in a condition to expand. No pulmonary gymnastics were carried out.

Objective and Rhythmical "Clicking" Noise in the Left Ear, associated with synchronous Muscular Contraction of certain Muscles forming the Floor of the Mouth.

By HERBERT TILLEY, F.R.C.S.

S. G., FEMALE, married, complains of a "beating noise in her left ear," of twenty months' duration. The cause of this symptom is unknown, and in all other respects the patient is healthy. The noise complained of has a "clicking" character, and is audible if the examiner's ear is placed close to the patient's left ear; it is still more noticeable when an auscultation-tube is used. It can also be heard if the ear be placed close to the patient's open mouth. Between the chin and larynx what

appears to be a regular, rhythmic pulsation is noticed, and this is synchronous with the "clicking" noise above described. Both the noise and the contractions cease if the patient holds her breath. The rate of the "clicking" and contraction is 120 per minute (when examined December 9); pulse-rate 72 per minute.

It was noticed that the submental contractions did not cease during quiet respiration, but that the "clicking" noise in the ear occasionally ceased for longer or shorter intervals. The patient says that the "beating under the chin goes on during sleep."

DISCUSSION.

Mr. TILLEY added that the patient was aged 30 and had had three children. She thought the condition followed influenza. The clicking was audible to others a foot away, but was more noticeable when the auscultation-tube was placed in her left ear. The palate also exhibited rhythmic contraction synchronous with the "clicking" and the submental movement. He had never seen or heard of a similar condition of things.

Dr. MORLEY FLETCHER asked if it had been ascertained whether the contraction continued while the patient was under anæsthesia.

Dr. HENRY HEAD said that when the mouth was half open she could be seen to make the clicking partly with the tongue and partly with the palate. With the mouth wide open the movement could be seen at its best. So long as the mouth was closed nothing very definite was heard by an observer, but she complained that she could hear the click in her ear. He considered that this condition belonged to the group of "clicking tic," analogous to the "blinking tic." No treatment seemed to have any beneficial effect, and the prognosis of the condition was excessively bad. She said she had never been ill before in any way. Such patients tended to become insane, at any rate for a time. Such a fact was very startling. Tics of this character seemed to be associated with a tendency to violence of act or language. A remarkable proportion of tics of this type seemed to end badly.

Mr. HERBERT TILLEY, in reply, said the patient had not been placed under an anæsthetic, and he did not know how far such an experiment would be justifiable. When the patient first attended the noise was in the ear, but that was absent that evening. He thought it was caused by the contraction of the salpingo-pharyngeus muscle, which took origin from the Eustachian tube. That evening the noise was chiefly caused by the clicking of the tongue, which moved backwards and forwards from the posterior wall of the pharynx, and caused, as it were, an unsticking of the mucous surfaces. He would add one of his own experiences to that of Dr. Head bearing upon prognosis. Four years

ago, at the Laryngological Society, he showed a male patient in whom there was a very noticeable rhythmical contraction of the right side of the pharynx. Nothing else could be found in the patient indicating serious organic disease, but twelve months later the patient developed symptoms of general paralysis of the insane, from which he eventually died.

A Case of Bending of the Bones of both Legs and Partial Ankylosis of both Hips in a Young Man.

By SOMERVILLE HASTINGS, M.S.

THE patient, aged 22, has been employed as a porter in a restaurant. Seven years ago he began to suffer from genu valgum, for which he was treated by external splints for two years. Three years ago pain and stiffness of both hips came on, and has since increased, so that at the present time he is unable to sit down. The feet are everted, and both great trochanters are prominent. Skiagrams show bending of both tibiæ near their upper extremities, enlargement of the acetabula, flattening of the heads of both femora, and some prominence of the great trochanters. The genu valgum, which was most marked on the right, was much improved by an osteotomy of the lower end of the right femur in June, 1909.

DISCUSSION.

Mr. HASTINGS added that it had been suggested that the condition of the hips might be due to rheumatoid arthritis, but he had not found definite evidence of septic poisoning. The patient had slight pyorrhœa, but this was not marked. The skiagrams did not show any rarefaction of bone.

The PRESIDENT said the change in the shape of the femur seemed to be in the nature of a rotation of the upper end of the femur.

Dr. F. PARKES WEBER asked whether the case might not be an early one of osteitis deformans, or, more probably, an imitation of osteitis deformans produced by inherited syphilis. The patient was very young for genuine osteitis deformans. Later on the formation of gummata might prove the syphilitic nature of the condition. He (Dr. Weber) knew of a boy who suffered from a chronic painless, or relatively painless, symmetrical overgrowth (with bending) of both tibiæ, somewhat resembling osteitis deformans. Later on the change was proved to be due to inherited syphilis by the appearance of gummata.

Intermittent Claudication, due to Chronic Arteritis Obliterans, in a Tobacconist.

By F. Parkes Weber, M.D.

The patient, B. R., is a Russian Jew, aged 41, who has been fourteen years in England, and used to work in a cigarette factory. Now he is a tobacconist. He has enjoyed good health on the whole, and denies intemperance in alcohol or having had venereal disease of any kind. During the last twenty months or so he has been troubled with typical "intermittent claudication" in the left lower extremity. After walking for about five minutes he has to rest on account of pain or a cramp-like feeling, chiefly in the calf muscles. Then, after resting for a short time, he can walk on again for another five minutes, and so on. Some paræsthesia (formication, &c.) in the left foot is likewise complained of. The objective symptoms can be altogether overlooked. The left foot is not ordinarily red or cyanosed like the affected foot is in most cases, but when he moves both ankle-joints the skin over the back of the left foot becomes markedly paler, and the toes feel colder to the touch than the corresponding parts of the right foot. Pulsation cannot be made out in the dorsalis pedis artery of either foot, but is present in the femoral artery at both groins, as it also is in the posterior tibial artery at the right ankle. No pulsation can be detected in any of the arteries of the left foot. No disease in the thoracic and abdominal viscera has been discovered. The symptoms complained of commenced quite suddenly about twenty months ago. Prior to this there was probably already a certain amount of arteritis obliterans present in both legs, and the sudden onset of the symptoms was doubtless due to some acute complication, possibly thrombosis in the left popliteal artery. Subjective symptoms, similar to those mentioned above, have likewise been noted in the right lower extremity, but to a lesser degree than in the left. It is on account of the possibility that the objective signs may be overlooked in similar cases that the patient is now brought forward.

Chronic Arteritis Obliterans (lower extremity) associated with Phlebitis.

By F. Parkes Weber, M.D.

The patient, M. M., is a Russian Jew, aged 44, who presents the characteristic symptoms of chronic arteritis obliterans in the left lower extremity—namely: (1) redness or cyanosis of the foot when it is allowed to rest in a dependent position; (2) pallor of the foot on movement of the ankle-joint; (3) intermittent claudication or "angina cruris," with feeling of cramp or pain in the muscles of the calf or instep on walking for a few minutes; (4) absence of pulsation in the arteries of the foot, notably in the dorsalis pedis artery. The symptoms are of six years' duration, and the patient was shown before the Clinical Section on December 13, 1907.[1]

He is now shown again to illustrate the following points: (*a*) The arterial disease seems not to have progressed, and there is at present no "ischæmic ulceration" of the toes; (*b*) phlebitis, especially of the internal saphenous vein, has recently associated itself with the arteritis in the affected extremity, as in several other reported cases, especially those described by Leo Buerger under the title "thrombo-angeitis obliterans." In the amputated leg from a similar case (1904) Dr. Weber found in one of the sections of the affected vessels that both the artery and one of the veins accompanying it were occluded by organizing thrombi. This showed that the veins accompanying the arteries might sometimes be thrombosed when the superficial veins were apparently not affected. In the present case the blanching of the dorsum of the foot, on movement of the ankle-joint, seems to be mainly produced in the following way: The skin over the dorsum of the foot is made tense by extension of the ankle-joint, and the cutaneous capillaries are thus emptied into the veins. Owing to the arterial obstruction the refilling of the capillaries from the arteries proceeds very slowly. On the other hand, the "intra-capillary" circulation is extremely free, so that the white mark left by pressing with the tip of a finger on the back of the patient's left foot almost instantaneously gives place to redness when the pressure is withdrawn (probably a compensatory phenomenon). There is no evidence of syphilis in the case.

[1] *Proc. Roy. Soc. Med.*, 1908, i (Clin. Sect.), p. 44.

Symmetrical Trophic Lesions of the Extremities in a Child. Syringomyelia (Morvan's Type).

By H. Morley Fletcher, M.D.

The patient, a female, aged 6, was admitted to St. Bartholomew's Hospital on July 27, 1909, with the following history: In infancy she was backward and began to talk late. Up to the age of 4 years she did not walk, but dragged herself in a sitting posture about the room; at 4 years she began to walk properly. When she was 2 years old the tip of one forefinger became swollen and began to discharge; this ultimately healed with the loss of the tip of the finger. A similar process took place subsequently in the fingers and thumbs of both hands, and was entirely painless. When 3 years old the feet first became affected, and the left great toe was lost by a process similar to the above. On one occasion she put her feet in the fire, and, although severely burnt, she felt no pain. In the summer of 1908 she fractured the right tibia and fibula, the result of a fall whilst playing; she did not complain of any pain, and the injury was not recognized till two days later, when the mother discovered the fracture by the "looseness and grating" of the parts. The fracture was treated at the London Hospital, where she came under the observation of Dr. Head. The patient has had measles, but no other illnesses. She is the ninth of a family of twelve, of which five are now alive and well. The others died in early life from acute illnesses. Both parents English, and alive and healthy. There was no history of any other member of the family having any similar disease. There was no evidence of syphilis, acquired or congenital. The child has never been abroad.

When admitted to the hospital she was well grown, but dull and lethargic. Eyes natural, except for left internal strabismus; no pain felt on touching the cornea or sclerotic, but corneal reflex was present. Mental development retarded, but this improved greatly during her stay in the hospital. Thorax and abdomen natural. There was no muscular wasting or paralysis. Habits clean. Limbs: Both arms were natural. Right hand: The terminal phalanges of all the fingers and the thumb were lost, and when a phalanx had fallen off the separation had occurred either at the interphalangeal joint or a small piece of bone had remained,

representing the terminal phalanx and movable in any direction. Left hand: The terminal phalanges of the fingers and thumb were similarly affected (fig. 2). Legs: Left lower limb was externally rotated and could not be fully rotated inwards; slight flattening of prominence of left



FIG. 1.

Syringomyelia (Morvan's type).

trochanter. Knees: Left, natural; right, 5° of genu valgum present. Legs: Left, natural; right, bent with convexity inwards, the apex of which is the site of the old fracture, slight shortening of right leg;



FIG. 2.



FIG. 3.

some creaking in right ankle-joint. Feet: Left, great toe and first metatarsal absent; right, terminal phalanx of great toe missing (fig. 3).

On admission, fetid pus was oozing from the finger-stumps and from a sinus in the left foot. There were several superficial ulcers on the legs and buttocks. The discharge and the ulcers soon cleared up



FIG. 4.
Skiagram of left hand.

under local treatment. Gait: She was just able to stand, but could not walk; this appeared to be due in part to muscular weakness due to confinement, and in part to the deformity resulting from the fracture. Sensation: Over the whole of the cutaneous surface and the mucous membrane of the mouth and tongue the sensation of pain was absent.



FIG. 5.
Skiagram of right foot.

Corneæ insensitive to pain. Tactile sensation present over the whole body. Thermal sensation certainly impaired, but it was impossible to decide whether it was absent or not. Electrical reactions: Dr. Lewis Jones reported that these were normal, and further that "it is very doubtful if she feels the current in her extremities." Reflexes: Knee-jerks and plantar responses not obtained. Abdominal reflexes absent.

Skiagrams: These show no evidence of rarefaction of bone such as is described in adult cases of syringomyelia (figs. 4, 5). In the right leg there is an old fracture with callus at the junction of middle and lower thirds of tibia and fibula (fig. 5).

During the child's residence in hospital the tips of the fingers have on several occasions become inflamed, but this has quickly subsided under treatment.

There can be no doubt but that this is an interesting example of syringomyelia of the variety known as Morvan's type, occurring at an unusually early age. Dr. Head has informed me that he described some years ago, in the *London Hospital Gazette*,[1] a somewhat similar case in a child, but with this exception I have not met with any account of this type of the disease in an infant in English publications. All forms of syringomyelia are very rare in children, judging by the figures given by Schlesinger. In a series of 260 cases collected by him, nine occurred in children under 10, and the type of disease is not specified. It is interesting to note that syringomyelia is twice as common in males as in females, taking all ages into account, but that, in his nine cases in children, five females were affected to four males. The striking features of the case are the universal distribution of the analgesia and the complete absence of muscular atrophy.

I must express my thanks to Dr. Yates for the notes on the case, and to Dr. Walsham for the skiagrams.

DISCUSSION.

Dr. HENRY HEAD, F.R.S., said he was glad to see the case again. The patient had been under Dr. Percy Kidd or himself for a considerable time. When he saw her first, at between four and five years of age, she was very neglected and backward. When last she was in the ward, in 1907, she was totally insensitive to pin-prick, and they made a note that the observations on temperature were not satisfactory. That seemed to be the condition now. When she left the London Hospital she fell back into a filthy condition, with sores all over the body, the hands and feet smelt, and the thickened skin

[1] 1903, x, July (Clinical Section).

underwent putrefactive changes. She had been in the London Hospital on the surgical side from time to time since 1907 to be cleaned up. The sores would heal if proper means were taken. He reported a similar case in the *London Hospital Gazette* of 1903. It was the third case of the kind he had seen. In this country Morvan's disease could have nothing to do with leprosy. During the twenty years that the nature of this disease had been discussed, the evidence was overwhelming that the condition was associated with central gliosis in the spinal cord. The advocates of the lepra view said that was not against their theory; that the lepra bacillus might invade the cord. He thought it was nothing more than a form of syringomyelia, coming on excessively early. Whether there were changes in the peripheral nerves in addition could only be settled by more autopsies.

Dr. LAKIN asked how long such children were likely to live.

Dr. FLETCHER said he could not answer Dr. Lakin's question, as the present case was the only example in a child of which he had any knowledge. With the exception of the somewhat similar case described by Dr. Head, he had not come across any account in this country of this type of the disease occurring in early childhood. Cases of syringomyelia of Morvan's type, or in fact of any variety, were extremely rare in children. Schlesinger, in his series of 260 collected cases, found nine only in children under 10. Dr. Fletcher further pointed out that the mental condition of his patient had immensely improved during her stay in the hospital; also that the tongue was quite insensitive to pain.

Clinical Section.

February 11, 1910.

Mr. A. Pearce Gould, President of the Section, in the Chair.

A Case of Angioneurosis.

By W. Essex Wynter, M.D.

A. W., aged 29, female, married, suffered at the age of 15 from spinal abscess. Father and two brothers died of tuberculosis. Three years ago, when weak and out of health, ulcers broke out on the legs and spread to trunk and arms, following a scratch on one foot. These ulcers still recur, mostly on the legs, being preceded by bruise-like, hæmorrhagic patches, are very painful, and are frequently associated with the menstrual period. The patient feels weak and tired, is short of breath, and very susceptible to cold. The hands, feet, and ears readily become blue and numb, and are extremely tender. The appetite is poor, and she suffers from indigestion and constipation; all functions appear to be sluggish. The pulse is small and soft, and blood stagnates in the skin. A purpuric eruption occurs from the knees downward on standing, and is associated with pigmented patches (erythema figuratum) over the shins. The knee-jerks are present, and there are no definite abnormal physical signs in connexion with the heart or lungs. Blood normal.

Urine acid, specific gravity 1012—1032; a trace of albumin (twice) and indican. The condition of the legs improves when in bed, but the purpuric eruption recurs on getting up, the feet and ankles becoming œdematous.

With an infected throat during the last week of December there was some fever, and the circulatory condition improved. Afterwards patient relapsed into the original state.

DISCUSSION.

Dr. W. ESSEX WYNTER added that, thinking that thyroid extract should do good, he had kept the patient on it for several weeks, but without any definite effect on the lesions. Nitroglycerine produced warmth in the hands, but no decided improvement in the condition. Strychnine and strophanthus were then tried with the view of improving the heart's action; and he also used the constant current, but without permanent benefit, for as soon as the patient was exposed to cold again, the condition returned. In the summer she was fairly well.

Dr. GALLOWAY said that the form of vascular lesion of the extremities shown in the case of Dr. Wynter's patient was very unusual. The two most striking features were, in the first place, the pain in the extremity, which was frequently very severe; secondly, the change in the superficial blood-vessels of the affected region, which had resulted in what appeared to be permanent dilatation of the superficial vascular plexus of the skin. The individual telangiectases were small, difficult to distinguish, and tended to produce a uniform purple lesion. This area of damaged skin usually commences on the inner and outer surfaces of the feet, passes in a band-like form behind or over the malleoli, and upwards on the leg, the internal aspects of the lower extremity being usually most severely affected. Two women, both aged about 30, with what appeared to be almost exactly the same condition, were now under the speaker's observation, and had suffered from superficial inflammatory attacks of an eczematous character. This secondary condition, however, soon improved under rest and appropriate treatment, leaving the painful vascular lesion as the real disease. This telangiectatic state, which seemed to be the result of chronic inflammatory changes of the blood-vessels, tended to spread. Dr. Wynter's case appeared to show discrete lesions on the leg as well as the main areas involving the ankles.

Dr. F. PARKES WEBER suggested that some of the cases in middle-aged people, in whom occasionally there were raised blue swellings on the legs, coming on without any apparent reason or following very slight trauma, were identical with Dr. Wynter's case. Recently he saw at the hospital a woman, aged 61, who said that "blue lumps" sometimes appeared on her legs. None could at first be seen, but, apparently during the examination, a swelling of the kind developed where an assistant had pressed in lifting the leg up. Probably the condition was not of such bad prognostic significance as one would be inclined to suppose. In some cases it might be quite a local condition affecting the lower extremities, and did not necessarily signify any very peculiar liability to cerebral hæmorrhage or cerebral thrombosis. In the woman to whom he specially referred (who was of medium general nutrition) the "lumps" never occurred anywhere except on the legs. The coagulation-time of her blood, as estimated by Sir A. E. Wright's coagulometer, was six and two-third minutes. She frequently suffered from painful muscular cramps of the lower extremities at night-time.

Dr. ROLLESTON expressed some doubt as to the suitability of the name angioneurosis for a condition in which hæmorrhage formed a prominent feature. Although hæmorrhages might be neurotic in origin, the question arose whether a focus of cryptic infection in some part of the body was not responsible for the damage to the vessels which allowed the hæmorrhages to occur.

Dr. WYNTER replied that he regarded the condition as a neurosis partly on account of the similarity to Raynaud's disease, which he considered was a neurosis, and partly because of the variability of the condition in regard to posture, temperature, &c. This seemed to point to functional disturbance in the walls rather than to organic change. In Dr. Galloway's and his own cases the patients were women, and he had not seen the condition in a male. The trophic changes, also, were consistent with neurosis: they varied in position, they healed, and there was no evidence of thrombosis, embolus, or any organic vascular lesion; it seemed to be a very variable change in the vessels.

"Rheumatic Nodules" in a Case of (probably) Gonococcal Arthritis.

By H. MORLEY FLETCHER, M.D.

THE patient, a stoker, aged 24, has the following history: November, 1907, contracted gonorrhœa, the discharge lasted three months and ceased under treatment; no joints were affected. April, 1908, had "rheumatic" fever; kept in bed ten weeks with painful joints (knees, hands, and jaw); no nodules observed. July, 1908, spent two weeks in a convalescent home and afterwards returned to work, but had to give this up as the pains returned in his joints. The urethral discharge recommenced (thin and watery in character), and has continued ever since. From January to February, 1909, was again laid up with pain and swelling in many joints, and he has not been free from joint trouble since this time. In April, 1909, he was under treatment as an out-patient at St. Bartholomew's Hospital. Both wrists and ankle-joints were swollen and tender, the first interphalangeal joints of both hands showed fusiform enlargement. The heart was natural. Urethral discharge present. The case was regarded as one of gonococcal arthritis. No nodules were observed till May 12. Numerous nodules were found on the flexor tendons of both wrists, extensor tendons of backs of both hands and knuckles, on both elbows, on the tendons of the ankles, on the

dorsum of both feet, and on both tibiæ. These nodules exactly resembled those associated with rheumatic fever and morbus cordis in children. No improvement occurred under treatment, and he was admitted on June 9, 1909. He remained an in-patient under my care till July 23, and was discharged in much the same condition that he now presents. The temperature was normal throughout. The joint pains improved, but never entirely left him. Repeated bacteriological examinations were made, but gonococci were never found. The nodules diminished in size, but had not disappeared when he left the hospital in July; they can be still felt on the hands and wrists, the elbows, the shins, and on the tendons of the ankle.

DISCUSSION.

Dr. POYNTON raised the question of the relation of some of these cases of gonorrhœa to true rheumatism. It was common knowledge that rheumatic patients, or those who had previously had rheumatic fever, were more liable to have gonococcal arthritis. No doubt the nodule was a local focus containing the organisms, just as a nodule was a local condition of rheumatic fever. In three cases Dr. Paine and he had been able to demonstrate the organisms in the dilated vessels, which were on the periphery of the ordinary rheumatic nodule. But it was possible that in cases such as the present there was a mixed infection, just as was possibly the case in rheumatism. Possibly the present patient had had gonorrhœa on a basis of rheumatism. He thought the nodules of gonorrhœal infections had not been so marked on the tendons, and that they were larger; and the present lesions resembled gonococcal nodes rather than rheumatic.

Dr. GARROD, F.R.S., did not consider that the evidence forthcoming in this case would justify the conclusion that the nodules present were manifestations of gonorrhœal rheumatism rather than rheumatic nodules in the ordinary sense. There could be no doubt that true rheumatic nodules were met with occasionally in young adults, and in such patients they were more persistent than in children and were less closely associated with cardiac lesions. On the other hand, the nodules might be regarded as kindred lesions to those of joints, and it cannot be assumed that the rheumatic infection is alone able to produce them. It seems more probable that any infection which tends to implicate fibrous structures may lead to the production of such nodules as one of its manifestations.

Dr. MORLEY FLETCHER, in reply, said there was little doubt that the case was one of gonococcal infection, although the organism had not been demonstrated. It was often very difficult to find the gonococcus in cases of chronic urethritis. Multiple nodules associated with gonococcal arthritis had been described in France, and he believed the gonococcus had been actually

demonstrated in the nodules. He was interested to hear that Dr. Poynton had met with nodules in gonococcal arthritis, and he asked for further details about them. The nodules in his (Dr. Fletcher's) case were in the tendon sheaths, and appeared to have a distribution exactly similar to that met with in children suffering from rheumatic fever. The nodules occasionally found in cases of rheumatoid arthritis in adults were usually larger and of almost cartilaginous hardness as compared with the nodules in this case.

A Case of Hereditary Multiple Telangiectases.

By Frederick Langmead, M.D.

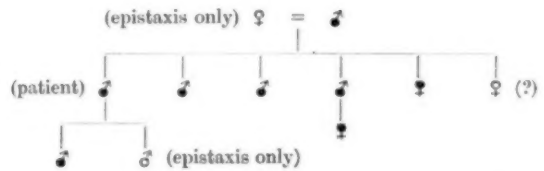
The patient, formerly a brewer's drayman, is a fairly well nourished man, aged 68. He is somewhat pale, the complexion having a slightly yellow tint. On the face are about thirty small vascular swellings, of a red or purplish colour and nævoid in appearance. Some have the "spider" form, others are more rounded and circumscribed. All are small, varying from mere points to the size of a large pin's head. The lower lip is swollen from a similar purplish nævoid condition, the mucous membrane being mottled and irregular from the presence of several vascular elevations. The under-surface of the tip of the tongue shows a crowded row of similar swellings. Elsewhere they are but few in number. One is seen on the buccal surface of the right cheek, one on the palate, a few on the neck, chest, and back. None are visible under the finger-nails. Mr. Bickerton has kindly examined the eyes and reports that there are no star-shaped pigment spots or hæmorrhages as described by Dr. Parkes Weber, but some tortuosity of the veins below the right disk and opacities in the left vitreous humor and lens. A slight spur is visible on the right side of the septum nasi, but no telangiectasis has been seen in the nose. For about twenty years the patient has been the subject of frequent epistaxis, whilst occasionally the face or tongue has "burst out bleeding."

Patient first came under observation in June, 1907, for a fractured elbow, which was followed by nose-bleedings almost daily. His anæmia was at that time very severe, and repeated examinations of the blood were made to exclude pernicious anæmia. A blood-count made on August 31, 1907, was as follows: Red blood-corpuscles, 2,200,000; hæmoglobin, 30 per cent.; white blood-corpuscles, 9,000. Differential

count: Polymorphonuclears, 52 per cent.; lymphocytes, 25 per cent.; hyalines, 4 per cent.; transitionals, 7 per cent.; coarsely granular eosinophils, 2 per cent.; myelocytes, 10 per cent.; basophils, ½ per cent. No nucleated red corpuscles were seen, but there was a good deal of poikilocytosis. No changes other than those of secondary anæmia were ever found. Carcinoma of the stomach was also suspected, but no physical signs of its presence could be found, and two estimations of the gastric contents did not support this view. The stools were also examined for the eggs of the ankylostoma and the sputum for tubercle bacilli, both with negative results.

Patient came first under my care in March, 1909, when he still had very much the appearance of pernicious anæmia, but the condition of his blood had greatly improved, as shown by the following report: Hæmoglobin, 65 per cent.; red blood-corpuscles, 5,984,000; white blood-corpuscles, 14,440; no abnormal red cells seen. The heart was considerably dilated and acting quickly and feebly, with occasional extrasystoles. Since then, under treatment with iron, he has considerably improved in general condition. Oozing from the nose still occurs from time to time, but there has not been any severe hæmorrhage.

Family history: The strong family history of this condition is well shown in the following pedigree:—



♂ = Affected male; ♂ = unaffected male; ♀ = affected female; ♀ = unaffected female. The table is constructed from the patient's account.

It will be seen that the patient is one of a family of six—four brothers and two sisters. All four brothers and one sister are the subjects of epistaxis and telangiectases. There are no data relating to the other sister. The patient's father was similarly affected, and the mother suffered from severe epistaxis only. The patient has two sons, one of whom, aged 35, has nævoid patches and epistaxis; the other, younger, has epistaxis only. A daughter of one of his brothers has both similar patches and recurring epistaxis. No account of the other families is forthcoming.

Lymphangioma of the Tongue.

By E. Rock Carling, M.B., B.S., F.R.C.S.

Edith F., aged 13, a patient at the Seamen's Hospital, Greenwich. First child of the marriage; face presentation; scars of forceps-marks symmetrically placed on temples; nævus of lip was noticed at birth, but condition of tongue not till a "bad attack of thrush" seven weeks later; the child did not walk till after second birthday; no illnesses of moment. Mother had two other pregnancies; no miscarriages. No ascertained evidence of specific disease in the parents; nothing in either family known to the mother at all comparable to the condition in the child; neither father nor mother has any nævi, but the father has several "moles." Three-fourths of the dorsal surface of the tongue is the seat of a lymphatic nævus or lymphangiectasis. The tongue is beset with clear vesicles, between which are scarlet capillary loops and blood-filled vesicles; a few of the vesicles are filled with opaque, white material. Associated with the tongue condition is a capillary nævus of the lower lip, which involves rather more than ½ in. of skin surface parallel with the muco-cutaneous margin, and extends, on the inner side, across the alveolar ridge to the under aspect of the tongue, where are some dilated veins. Neither general nor special sensation seems to be impaired. The tongue is generally about normal in size, and, though it is at times swollen enough to be tooth-indented, the teeth are not everted. The movements of the tongue are quite free. The child has no other nævi, but upon the skin of the front of the neck there are some dilated venules and there is a minute mole at the back of the neck. The inner aspect of the lip is at times the seat of shallow ulcers, and the tongue is then sore; occasionally a very little blood is lost from some burst vesicle, but, though the lymph-distended vesicles certainly suppurate and burst from time to time, there has not been at any time any considerable ulceration.

DISCUSSION.

Mr. Rock Carling added he was anxious to know if anything could be done for the condition. Possibly it might be treated with radium.

Dr. Parkes Weber regarded Dr. Langmead's case as of the same character as the cases described by Professor Osler and others and by

himself.[1] The severity of the case was due to the general condition of the patient; his arteries were very hard, the man had been accustomed to drink a good deal of beer—he had to do with brewing—and he (Dr. Weber) thought there was likewise probably some kidney disease present. The chronic vaso-dilator effect of the beer drinking exercised a bad influence when combined with the family tendency to telangiectases. In reference to Mr. Carling's case, in which the association of capillary hæmangioma of the lip with the lymph-angioma of the tongue was a feature, it would be interesting to ascertain in how many cases of capillary hæmangioma there was also a lymphangioma present; in other words, in how many cases of lymphangioma of the tongue, or elsewhere, more or less capillary hæmangioma ("port-wine nævus") of the skin of the face or elsewhere could be detected.

Mr. A. E. BARKER, discussing Mr. Carling's case, said he had seen two or three such cases, one of which had recently come under notice after an interval of fourteen years since the patient was first treated as a little girl, aged 8. Consideration of this case might, perhaps, show what must be done in some instances, whilst in others it was best to wait. That child since birth had had a tongue which was larger than normal, and had the same appearance as the present one. But the tongue began to grow, and with the occurrence of the first dentition it began to chafe on the incisors and became inflamed. Then the tongue began to grow in all dimensions, and when she came to Mr. Barker she could not get it into the mouth without difficulty. He made lateral flaps, starting from the foramen cæcum behind, and removed the front half of the tongue. The wound healed perfectly. She was seen again four years ago, with the tongue still in the mouth, but it was said to be too fat, and pressed against the roof of the mouth. On that occasion he made two flaps in the contrary sense, going from the front downwards and back to the floor of the mouth. He took out a wedge and brought the edges together. She was now quite well and the only defect was a slight lisp. He took it that many of these nævi, whether they were lymphatic or blood nævi, underwent fibrotic changes in time and disappeared. This led him to suggest that the present case should be left alone. In connexion with Dr. Parkes Weber's inquiry, he might say that the first portion, about one-third of the tongue, which was removed was injected at the time, and showed typical lymphangiectasis; the other piece, which was removed many years afterwards, showed a greater increase in the venous radicles than in the lymphatics; in fact, it was more like a venous nævus.

[1] F. P. Weber, "Multiple Hereditary Developmental Angiomata (Telangiectases) of the Skin and Mucous Membranes," *Lancet*, Lond., 1907, ii, p. 160.

Sarcoma of the Ilium treated by Coley's Fluid.

By Albert Carless, M.S.

G. C., aged 47, a builder, was admitted to hospital for a tumour in the right loin. Eighteen months previously he had complained of shooting pains in the buttock and calf, which were supposed to be sciatica, and he was treated for that complaint. At times the pain was acute, but in the intervals it was merely of a dull aching character, and was increased on taking exercise. The swelling in the iliac fossa and loin was only noted one week before admission. It extended upwards as far as the lower border of the ribs, and projected forwards into the abdomen as far as the mammary line. It was elastic to the touch, and almost gave the sensation of fluid under pressure, but on the introduction of an exploring needle nothing but a little blood was obtained. It was quite fixed and immobile. The pain was very severe, even whilst the patient was lying in bed, and he frequently required morphia. Radiographically there is some slight irregularity of the ilium and apparent thinning of the bone, but nothing very definite.

Operation being obviously out of the question, treatment by Coley's fluid (obtained from Parke, Davis and Co.) was relied on. Twenty-two injections were given up to Christmas, and as a rule a well-marked reaction, followed by profuse sweating, was secured. The dose has never exceeded five minims, and a severe reaction always followed that dose. The result has been very striking in the shape of a gradual diminution in the size of the swelling, which, however, still remains very large ; but more especially in the disappearance of the pain, and a general feeling of improvement in his health and strength. He has recently had an interval of treatment and was out of hospital for two or three weeks, and complained but little of pain during that period. Treatment has again been started, the injections being made into or around the growth, and the œdema to be seen in the loin is the result of the local injections. Five minims of the fluid are given two or three times a week, and the improvement appears to be still continuing. His general condition is decidedly good.

Excision of both Superior Maxillæ: Extension to Frontal Sinus.

By Albert Carless, M.S.

This patient was shown at the October meeting of the Clinical Section this session.[1] The maxillæ were removed—the left on June 19, 1908, and the right on November 6, 1908. Some difference of opinion has been expressed as to the nature of the disease, some histologists saying the tumour on both sides was epithelioma, and some stating the growth on the left side to be fibro-cystic disease.

At the end of October, 1909, patient noticed a swelling of the forehead, just to the left of the middle line; this steadily increased in size, but caused no pain or discomfort, except some intermittent headache. He was admitted to hospital on December 1, and the notes state that the swelling was then about 2½ in. in diameter; it was soft, and had fairly regular but not very well defined margins. A radiograph suggested that the frontal sinus was dilated, but Dr. StClair Thomson reported that he could not explore the sinus from the nose, owing to the distortion of the upper part of the nasal cavity, but he thought it was not a mucocele. There was no evidence of recurrence in the nose and no enlargement of glands.

An exploratory operation was undertaken through an irregularly cruciform incision. The swelling was found to be neoplastic; it extended, however, so deeply through the sinus that removal was not attempted, the more so as the patient took the anæsthetic very badly, and artificial respiration had to be undertaken twice during the procedure, owing to cessation of the breathing. The wound healed satisfactorily, and since then X-rays have been applied. The tumour does not seem to have increased in size of late.

At the first operation it was noted that the growth had extended far up on the inner side of the orbit, and in all probability it is along the left infundibulum that the extension has occurred. The absence of enlargement of lymphatic glands, and the absence of recurrence in the nose are interesting points in the case, if the growth be of an epitheliomatous nature.

[1] *Proceedings*, iii (Clin. Sect.), p. 10.

P.S.—It was noted at the meeting that some swelling was present about the right supraorbital border, and that a lymphatic gland was enlarged about the great cornu of the hyoid bone on the right side. These had appeared since the patient had last been seen at the hospital.

Four Cases of Epithelioma of the Larynx after Operation.

By StClair Thomson, M.D.

These four cases are interesting from several points of view. All of them show the safety of the operation of laryngo-fissure. One of them has remained without recurrence for nearly six years. In one case both vocal cords were completely removed, and yet the patient has a loud, strong though harsh voice, and is free from recurrence two years and four months after operation. This case is also noteworthy because the whole operation was done under local anæsthesia, and because part of the thyroid cartilage was excised at the same time. Fortunately, a coloured drawing of this case was made before operation. The third patient has a good voice one year after operation. In the fourth case laryngo-fissure had to be followed by complete excision of the larynx. The result is particularly interesting, as the patient is enabled to speak and to carry on much of his respiration through his nose and mouth by means of a von Bruns tracheotomy cannula.

Case I.

J. K., aged 48, came under observation in October, 1903, with a greyish, sessile, dimpled growth occupying the anterior two-thirds of the left vocal cord. As he had been treated successfully in the Victoria Park Chest Hospital in 1893 by Dr. Heron with Koch's tuberculin, the possibility of the affection being tuberculous had to be considered. He was shown before the Laryngological Society of London,[1] but no definite opinion was given by members, although it was generally thought that the case might prove to be tuberculous. It was not until July of the following year (i.e., 1904) that the growth of the tumour, the infiltration of the cord, and the consequent interference with its movement made

[1] *Proceedings*, ii, December, 1903, p. 68.

me conclude that it was an epithelioma. It is here well worth noticing that the patient had been hoarse, with fatigue in speaking, for thirteen months before he presented himself, and that he was under observation for nine months before the diagnosis was settled. This long period of twenty-two months shows how slow is the progress in some cases of intrinsic cancer of the larynx, and how difficult the diagnosis may be.

Laryngo-fissure was done under chloroform on July 12, 1904. Hahn's tube was used in the operation. The portions removed showed well-marked epithelioma; even in the deeper tissue removed cell-nests were seen quite near to the attached surface. Six weeks later a small thickening was noticed in the anterior commissure. As this developed into a pale, pedunculated growth, the size of a pea, it was removed with intralaryngeal forceps, and, under the microscope, proved to be granulation tissue.

The patient's case is further reported in the *Proceedings of the Laryngological Society of London* for January 13, 1905. Since he was last inspected he has successfully come through attacks of influenza, bronchitis and pneumonia, and a fairly good voice has enabled him to carry on his occupation of a baker in the East End.

Case II.

C. L., aged 54, presented himself on October 4, 1907, with loss of voice for over a year. The left cord was quite immobile and replaced by a white, nodular growth, which extended from the arytænoid right up to the anterior commissure. The reddish character of the growth in this latter region suggested perichondritis and the invasion of the thyroid cartilage.

Laryngo-fissure was performed on October 17, and was entirely carried out under cocaine. Hahn's tube was not employed. All the left vocal cord and the soft tissue in the left of the larynx were removed, and as the disease was found to have extended to the under surface of the anterior part of the right vocal cord, all this was removed up to the processus vocalis. As there was some suspicion as to the condition of the thyroid cartilage, about $\frac{1}{2}$ in. of this was removed on each side of the median wound. The tracheotomy tube was left in and the wound above it plugged with gauze. The patient was fed by the mouth within twenty-four hours, and at the end of seven days he was eating solid food. The tracheotomy tube was abandoned nineteen days afterwards.

A microscopic examination of the growth showed the disease to be a squamous-celled carcinoma.

The patient was lost sight of until December 3, 1909, and was then found to be able to earn his living. He had no stridor even in sleep, in spite of the very free removal of his vocal cords. He reports that he may be a little short-winded on exertion. He speaks easily and loudly, with a strong, harsh voice, and this voice is apparently produced by the vibration of the ary-epiglottic folds.

Case III.

Mr. R. B. E., aged 59. When this gentleman consulted me in January, 1909, he said he had been hoarse continuously for three or four months, but that his voice had not been good for two or three years. The middle third of the left vocal cord was occupied by a papilloma-like, irregular, greyish growth, and the cord moved freely. The growth was removed endolaryngeally, and it was only as a matter of precaution that it was sent for examination to Dr. Eastes' laboratory, where it was reported on as being a squamous-celled carcinoma, with marked keratoid changes and epithelial "pearls." The patient and the sections were seen by Mr. Butlin, who strongly recommended operation, and gave a prognosis that the result would most likely be excellent.

Laryngo-fissure under chloroform was performed on February 17. Hahn's tube was not used. The microscopic examination of the removed portions confirmed the original diagnosis of epithelioma. (Sections were shown under the microscope.)

The patient was fed by the mouth from the first day. Six weeks later an oval, rounded, pale, smooth growth, about the size and shape of a small bean, and evidently a large granulation, was removed from the anterior third of the newly-formed cicatricial left vocal cord. Microscopical examination of this by Dr. Cuthbert Lockyer showed it to consist of granulation tissue, with a tiny scrap of degenerated epithelium which was quite benign.

The patient's voice is so satisfactory that he has been able to resume his profession of a schoolmaster. He smokes, and is in good health.

Case IV.

Mr. J. S. P., aged 42. This patient consulted me in February, 1909, reporting that his voice had not been strong for a year. The cords

moved, but not freely, appearing to be impeded by an infiltration into the right vocal cord, of which the anterior third was ulcerated and the posterior two-thirds white and warty. The patient was seen in consultation with Mr. Butlin, who wrote that "the tumour ought to be much more widely cut out than the growth in your preceding case. It is not a nice-looking growth, so I would go wide of it at each end."

Laryngo-fissure under chloroform was performed on February 25. Hahn's tube was not used. The microscopic report showed a surface epithelioma. By May last the patient had a rough, strong voice and was able to bicycle and play tennis, but by the end of that month thickening appeared over the anterior end of the right ventricular band, and stenosis with stridor slowly set in. He was seen several times in consultation with Mr. Butlin, and by the end of November the glottis was so narrowed that a low tracheotomy had to be carried out.

After consultation with Mr. Butlin, the larynx was completely removed on December 6, 1909. The extremity of the severed trachea was attached to the front of the neck, and the opening into the larynx was closed with catgut and the muscles sewn together over this. It was intended to cut off all communication between the mouth and the lungs. But these stitches broke down, leaving a good-sized opening into the pharynx and necessitating careful plugging and feeding with a nasal stomach tube. The result was much more satisfactory than if the stitches in the pharynx had all held; for as the wound steadily closed by granulation, the opportunity was taken of keeping open a track by means of an inverted rubber tracheotomy tube. Into this track later on the lobster-tail part of a von Bruns' artificial larynx was fitted. Through this, by closing up the orifice in the neck, the patient when at rest, is able to carry on respiration comfortably through his nose. The instrument also permits of his talking freely in a strong whisper. He is not yet able to adapt the reed, for, although it gives him a loud though somewhat "Punch-and-Judy" voice, it does not permit of adequate respiration. The patient's general health and spirits are excellent.

DISCUSSION.

Mr. A. E. BARKER asked if Dr. Thomson did the operation on the fourth patient under local anæsthesia, as he knew some who preferred local anæsthesia rather than general.

Dr. HERON said that one of the cases came under his observation at Victoria Park Hospital in 1893, with tuberculosis of the lungs, and the man told him he

had then received several doses of tuberculin each of 200 mg. at the close of his treatment. Dr. Heron disliked the use of the word "cure" in such cases. The man now had tuberculosis of both lungs. In such a case it was no small matter that the man had the record which this patient had, for he had earned his living for years. Dr. Heron strongly objected in the public interest to a man with tuberculosis being allowed to work as a baker, as for years this man had done. That was a matter to which the medical profession and the public should give attention. He thought the cases treated in the old days without the opsonic index and with large doses of tuberculin were as successful as those treated in the present day with the help of the opsonic index. Dr. Heron expressed doubts of the value of the opsonic index as a help in the treatment of tuberculosis of the lungs in adults.

Mr. BURGHARD desired to congratulate Dr. Thomson, particularly on Case IV, which raised the important question as to the best method of procedure in total excision of the larynx. As Dr. Thomson remarked, it had been the object of everybody of recent years to close off the pharynx and œsophagus from the air-passages in order to avoid post-operative pneumonic troubles. That was a very attractive view, and one which he had adopted recently in all the cases of total excision of the larynx which he had done. But if the alimentary canal was shut off from the air-passages, the patient was left in a condition which he felt acutely afterwards. Some private patients, especially, were often very much disappointed with the after-results; they could not smell or blow their nose, and they acutely felt their inability to speak; and their condition, after the removal of the malignant disease, was very wretched. The case shown by Dr. Thomson almost made it seem that one did wrong in aiming at the immediate and complete closure of the air-passages from the alimentary canal, and in his next case he would try to establish a fistula, by suturing the pharyngeal wall as far as possible around a small drainage-tube. The present patient said his only discomfort was the passage of saliva through the opening. That could not be avoided, but he only had to change the dressing at the most six times a day, and sometimes not more than twice. An inconvenience like that was nothing in comparison with the enormous relief to the man in being able to speak, and it made a vast difference to his mental condition. He asked Dr. Thomson whether he had any difficulty after the operation in connexion with the feeding, as that was a very great difficulty in old cases, in which no attempt was made to suture the pharynx. He was interested to hear from Dr. Heron that one of the cases shown by Dr. Thomson had phthisis; he understood that anybody who had tubercle of the lung and had an entire or a partial laryngectomy was sure to die; this case seemed to negative that view.

Mr. HARMER said the modern treatment had been to shut off the air passages in the first few days to prevent the occurrence of pneumonia from the swallowing of food or infection from the throat wound. He would be chary of leaving the passage from the trachea into the mouth; he feared it would increase the mortality of an operation which was already high.

Dr. StClair Thomson, in reply, said he agreed that the operation under local anæsthesia, *qua* the operation, was infinitely preferable to that under general anæsthesia. It was then easy to get the patient to cough, which was useful at certain times, to prevent the blood and mucus from going the wrong way; and with the use of adrenalin, the operation was remarkably bloodless. But, on the other hand, the mental and moral strain of laryngofissure was much greater under local anæsthesia, both to surgeon and patient. Most private patients refused to have it done under local anæsthesia. In Case I he would try to get the man's tuberculosis again arrested. In Case IV the stitches held for the first three or four days, luckily, for on the second day the man was out of bed and was swallowing comfortably by the mouth in forty-eight hours. When the stitches broke down, the food tended to come out through the wound in the neck. But feeding by the nose caused no distress. The tube was at first left in for twelve hours, and after the first ten days the patient passed it for himself. Dr. Thomson thought that Mr. Burghard and Mr. Harmer were, like himself, agreed that the upper end of the trachea must be stitched to the front of the neck above the sternum. No one desired to give up this (so-called Solis-Cohen) method, or revert to an attempt at keeping direct communication between the lungs and pharynx. But the lesson his fourth case taught was that an indirect communication might be maintained with immense comfort and benefit to the patient.

Pyæmic Knee- and Shoulder-joints treated by Tapping and Incision.

By Edred M. Corner, F.R.C.S., and Martin Huggins, F.R.C.S.

The case is that of a man aged 54, and by occupation a brewer's drayman. He was admitted to St. Thomas's Hospital on October 2, 1908, for a large carbuncle at the back of his neck, due to infection with the *Staphylococcus aureus*. Incisions were made immediately on admission, and repeated three days later (October 5). The right knee swelled on October 7, five days after admission. It was tapped on October 9, and the *Staphylococcus aureus* grown from the fluid removed. The knee-joint never refilled or gave any further trouble. On October 10, eight days after admission, the left shoulder-joint swelled and became painful. It was tapped and the *Staphylococcus aureus* grown from the fluid removed. Instead of subsiding, as the knee had done, the joint had to be incised and drained on October 14. The right shoulder-joint then became painful, but subsided without either tapping

or incision. The case is shown to illustrate the practically perfect recovery of the knee, which was proved to contain the *Staphylococcus aureus,* the imperfect recovery of the left shoulder-joint after arthrotomy, and the practically perfect recovery of the right shoulder-joint.

DISCUSSION.

Mr. A. E. BARKER said that perhaps the explanation in this case might be that the knee-joint was tapped fairly early, and the shoulder-joint was not opened until it was in an extremely bad condition. Everyone had seen cases in which pyæmic joints had recovered with perfect movement, and also cases in which joints had been opened and disastrous destruction had been found which could not be recovered from or prevented by any operation. But in a recent case of his own, in which the knee-joint was full of thick fluid containing *Staphylococcus aureus,* he opened and washed out the joint and closed it without drainage, and now this joint was as well as its fellow. A bad result, therefore, should not always be attributed to incision or other treatment of the joint. It was more a question of the stage at which the operative interference was attempted, and in that case it was fairly early. But he agreed with Mr. Corner that the surgeon should not be in a hurry to open such pyæmic joints, seeing that so many of them recovered without. If they were opened, they should be washed out and closed again carefully.

Dr. PARKES WEBER remarked that what occurred in the knee in Mr. Corner's case was analogous to that which sometimes, though very rarely, took place in empyemata. He referred to those rare cases in which simple tapping sufficed to bring about the cure of an empyema. In such cases probably the microbes, the local presence of which originally gave rise to the empyema, died out, and, the purulent effusion in the pleura becoming sterile, simple tapping was all that was required. In still rarer cases, recovery seemed to have been effected without any operative interference, without even tapping, so that a post-mortem examination long afterwards might show nothing beyond a pleuritic cicatrix surrounding a little inspissated (cheesy) material.

A Case of Traumatic Pancreatic Cyst cured by Operation, and followed twelve years later by Medullary Leukæmia.

By ARTHUR E. BARKER, F.R.C.S.

THE patient is aged 28. Fourteen years ago Mr. Barker operated upon him for a pancreatic cyst of large size, following upon injury. The cyst was opened in the middle line and drained. An analysis of the

contents showed a typical reaction for pancreatic secretion. The result was a complete convalescence. Up to about three months ago the patient was in good health. About that time he began to have discomfort in the abdomen, and three weeks ago noticed a swelling in the left hypogastrium. On admission there was considerable swelling in the left side, and the outline much resembled that of the former tumour in size and shape. An enlarged spleen was made out and a blood examination revealed medullary leukæmia. The patient seems in fair general health. Blood examinations show considerable improvement under arsenic and iron since admission.

BLOOD COUNT.

	February 2	February 11
Total number of red corpuscles	3,600,000	4,840,000
Percentage of hæmoglobin	30 per cent.	45 per cent.
Colour-index	0·4	0·8
Total number of white cells	77,700	42,000

DIFFERENTIAL COUNT.

Polymorphonuclear neutrophiles	50·7	46·0
Large lymphocytes	11·2	12·5
Small lymphocytes	2·3	1·5
Hyaline lymphocytes	0·5	0·5
Transitional	0·7	9·5
Basophiles	5·5	—
Eosinophiles	0·3	4·0
Myelocytes—		
Neutrophile	17·0	—
Basophile	11·0	26 0
Eosinophile	0·6	—
Mixed	0·2	—
	100	100

Many nucleated reds; poikilocytosis; anisocytosis; vacuolation.

Fibrosarcoma of Nose Removed after Temporary Ligature of both External Carotid Arteries and Laryngotomy.

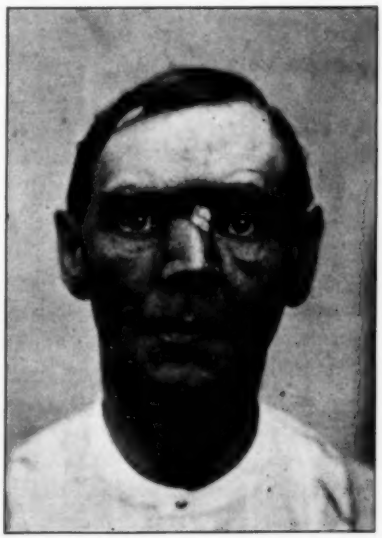
By DOUGLAS HARMER, M.C.

MALE, aged 47, admitted to hospital with large nasopharyngeal tumour, causing epistaxis. Five years ago continuous attacks of nose-bleeding, gradually increasing until three and a half years ago, when part of the middle turbinate was removed in New Zealand; for nine

months no bleeding. Two and a half years ago left nasal cavity curetted and large tumour removed through mouth, said to be fibroid and non-malignant. Two years, left eye displaced outwards; later, swelling noticed externally. One year, complete nasal obstruction; occasional epistaxis, recently very severe. For three weeks, treatment with Coley's fluid without improvement.

Patient was advised to come to England with a view to operation. He consulted a surgeon, who told him that the condition was inoperable. Later he saw Mr. Butlin, who advised him to take the risk of operation





Fibrosarcoma of nose.

FIG. 1.
Before operation.

FIG. 2.
After operation.

and referred him to the hospital. On admission he appeared in good health, but anæmic. All his organs were normal. There was a large vascular growth in the left nostril, projecting through the nasal bones in front (*see* fig. 1) into the left orbit and left antrum (dull to trans-illumination), into the nasopharynx, but not into the temporal fossa. The skin over the tumour was covered with dilated venules and the palpebral arteries on the left side were enlarged to the size of the radials. The left eye was displaced forwards and outwards; movements good;

vision $\frac{6}{12}$; field not diminished; marked signs of venous obstruction in retinal vessels; slight œdema and swelling of disk; almost the whole of the nasal side of the retina crumpled up, this part of the retina being seen with a much higher glass than the rest of the fundus, showing that it had been pushed forward, probably as the result of pressure upon the sclerotic. Right eye normal, no diplopia. Submaxillary glands slightly enlarged. X-ray photos did not assist in defining the extent of the tumour.

Operation: Chloroform anæsthesia. Both external carotids exposed and temporarily compressed with Crile's clamps. Infrathyroid laryngotomy. Pharynx plugged with marine sponges. Transverse incision through line of eyebrows, slightly curved downwards over bridge of nose. A second vertical incision along left side of nose to lower margin of alar cartilage. Skin covering tumour easily dissected free without severe bleeding. Nose turned down with elevator. Nasal and antral portions of tumour easily delivered from their bone cavities. The tumour, when separated from soft tissues of the left orbit by dissection, was found to be attached to the base of the skull, from the frontal sinus in front to the sphenoidal sinus behind. It was forcibly separated by an elevator. Floor and anterior walls of the frontal and sphenoidal sinuses then removed; also part of the base of the skull (2 in.) in the region of the cribriform plate; the dura mater appeared to be involved and was partially dissected away, a small quantity of cerebrospinal fluid escaping. Free scraping of bone cavities with sharp spoon. Hæmorrhage was never very severe. External carotids then carefully released; bleeding points seized and tied with catgut. The whole cavity firmly plugged. Incisions accurately united, excepting a large opening between the eyes for drainage. Carotid wounds drained. Laryngotomy tube removed; wound not sutured. Duration of operation two hours. Recovery uneventful; shock slight; pulse never above 100; temperature of 100° F. for six evenings. Plugging removed in forty-eight hours: a small piece of necrosed bone extracted after ten days. No secondary hæmorrhage.

Tumour exhibited: Fibrosarcoma.

Two months later, deformity much less (*see* fig. 2). Small opening between eyes retained for inspection. No sign of recurrence. Left eye almost in normal position; sight not affected. "Swelling of nasal side of disk less prominent by about 2 D than in the previous note; the folds in the retina are unchanged, and there are many other folds now visible to the temporal side of the disk; the signs of venous obstruction are no longer present" (Holmes Spicer). General health excellent.

The case is interesting as it shows that an extensive and vascular growth may be safely removed from the nose after temporary ligature of both external carotids and laryngotomy. If removal had been attempted without these preliminaries, it is probable that the patient would have bled to death. Ligature of the left external carotid alone would not have been sufficient to control the hæmorrhage.

The Diagnosis of Aneurysm of the Descending Thoracic Aorta.

By W. Essex Wynter, M.D.

The observations I wish to offer on the subject of aneurysm affecting the descending thoracic aorta were made on a series of cases in the Middlesex Hospital before the introduction of Röntgen rays as an aid to diagnosis, and include a recital of such clinical indications, more or less common to them all, which should raise suspicion of such lesion sufficient to call for the employment of this means, either to confirm or exclude its presence. Acting on such indications, I am able to cite two cases in which the diagnosis was made eighteen months or more before the final catastrophe, in which confirmation was afforded by skiagrams.

It may be premised that an uncomplicated local aneurysm projecting forwards in the left pleural cavity will give rise to practically no signs or symptoms, because in this situation it can for a time increase in size without affecting any important organs or structures. Such was the case in a woman, aged 58, dying of advanced pulmonary tuberculosis, in whom was found a sacculated aneurysm of the last 4 in. of the thoracic aorta, eroding the corresponding vertebræ, and almost filled with clot. There had been no complaint which could be referred to this source during life, and the condition was quite unexpected till disclosed at the necropsy. In other cases, however, there were certain indications which, if taken into account, would, with the aid of X-rays, establish the diagnosis. These were:—

Constant pain, with exacerbations, especially at night, referred to the back and left side of the chest. Post-mortem examinations disclosed erosion of vertebræ, exposure of intercostal nerves in the sac, and recurring pleurisy at the edge, extending over a variable portion of the lung surface. One or other of these, in addition to varying tension in

the aneurysm itself, would probably supply an explanation of the varying nature and intensity of such pain.

A brassy cough, with slight expectoration or hæmoptysis, was frequent, and might be attributed to the same causes as the pain, or, in many instances, to a concomitant condition of atheroma and dilatation of the arch. For extensive aortic disease was the rule, and in some instances produced such definite signs as pulsation over the anterior chest wall, a ringing aortic second sound, paralysis of the left recurrent laryngeal nerve, unequal pupils, tracheal tugging, and that lengthening of the arch which is associated with displacement of the heart downwards and outwards. In two instances a discovery of this kind led to diagnosis of thoracic aneurysm and the adoption of a suitable line of treatment, but when they proved fatal the disclosure of the sac below the arch was quite a surprise.

Weakness of the respiratory murmur on the left side of the chest was noted in a number of cases, and complaint was frequently made of dyspnœa and palpitation when lying on that side.

Hæmoptysis in relatively slight degree occurred at intervals in a minority; in some, many months before the fatal rupture. In one instance this appeared to be the only indication besides the patient's own sensations, for on blood-casts of the long lower bronchi being identified in the sputum the patient consulted a leading physician, who failed to discover any indications of disease at all, yet within a fortnight the man died suddenly of a copious hæmorrhage.

Profound weakness and inability for work are the first, and sometimes the only, complaints made by the patient. This might reasonably be attributed to the pretty constant association of low blood-pressure, so far as this could be inferred from a soft pulse.

The two cases which, acting on these indications, I submitted to X-rays, and so definitely recognized this obscure form of aneurysm, were :—

(1) J. W. C., aged 45, admitted March 21 and discharged May 25, 1904. He had had syphilis twenty-three years before, and first complained of pain between the shoulders in 1899. Slight hæmoptysis occurred eight months before admission, and he complained also of great weakness and dyspnœa on exertion, with constant pain in the back and left side of the chest. There was just a suggestion of clubbing of the fingers. The pupils were equal and reacted to light. Pulse 72, regular; tension low, synchronous in the radials, and apparently not delayed in the femorals. There was some thickening of the arterial walls, but no pulsation in the capillaries. The heart's apex was in

the sixth space in the nipple line, and there was some epigastric pulsation. No bulging or pulsation on the chest wall could be found, either back or front, nor any abnormal area of dullness. Slight tracheal tugging could be elicited. There were no cardiac or other bruits. There was tenderness to the left of the spine at the level of the seventh, eighth, and ninth spinous processes, but no pulsation. The second sound was accentuated over the aortic cartilage and at the apex. Heart-sounds were audible down the back. There was no obstruction of the œsophagus. The diagnosis of aneurysm of the descending thoracic aorta with general atheroma was made. The patient improved on full doses of iodide of potassium, and left in May, 1904. Though the diagnosis was made at this time, there were no means of employing X-rays, and it was not till a year later—May, 1905—when, readmitted, that the aneurysm was actually seen and a skiagram taken. He improved again in the hospital, where he remained three months—the limit of time that could be allowed—and then, as he would not go to the infirmary, was taken into one home after another—no easy matter to arrange for a patient who, while having nothing ostensibly the matter with him, was liable to fatal hæmorrhage at any moment. In November, 1905, the patient was again submitted to examination by the screen to determine, if possible, if consolidation had occurred, as the difficulty of finding a further asylum had become insuperable. The appearance suggested that the sac—easily visible above the heart and about equal in size—was rather smaller and less definitely pulsating. On the strength of this the patient was discharged, but within a week returned to the home on account of weakness and hæmoptysis, and two days later had a violent hæmorrhage, and died November 19, 1905.

The second case, which was shown at the meeting on January 14, 1910, was first seen in consultation in the country in February, 1907. G. S. M., aged 58, had then suffered for fifteen months from dull aching in the left side of the chest, back, and axilla. The apex-beat was in the fifth space in the anterior axillary line, dullness extending to the right edge of sternum. The first sound was normal; second (aortic) was accentuated. The sounds heard but indistinctly at the left base posteriorly. There was a tracheal tug. The pain at night was so severe that the patient could not remain in bed. As seen by X-rays, there is a large shadow showing to the right side of the dorsal vertebræ reaching from the fourth rib downwards. The arch of the aorta is much displaced to the left, and the heart is also displaced towards the left side.

Such cases are comparatively rare, these being the only two encountered in the last five years; but the striking absence of physical signs and tragic ending in nearly all by sudden copious hæmorrhage make them conspicuous in the memory, and any means of recognizing them will be valuable in at least preventing mistakes which would never be forgotten.

DISCUSSION.

Dr. ALEXANDER MORISON said the pain in such conditions was sometimes of a very excruciating character and its relief often difficult. Last year he saw such a case in consultation, in which for the relief of pain the patient had to rest on his knees and elbows. He had discussed with Mr. Godlee the question of division of the posterior roots of the spinal nerves concerned. Such an operation would have been critical for many reasons, but if he met with such a distressing case again he would take the risk of disaster by advising surgical isolation of the area of intercostal stimulation, either by careful dissection or by injecting alcohol into the posterior root-ganglia.

Dr. MORISON, answering Dr. Rolleston, said the patient was to a considerable extent under morphine, but yet suffered great pain, and he (Dr. Morison) did not think the operation suggested, if properly carried out, would entail undue risk.

The PRESIDENT (Mr. Pearce Gould) said that two years ago he was asked to see a gentleman who had a very large aortic aneurysm springing from the first part of the arch and bulging a long way into the right pleural cavity. The patient also had gall-stones and had had repeated attacks of very severe biliary colic, which caused him to writhe in bed. Large doses of morphine had been given to secure him some respite from the pain. The physicians felt that the frequent recurrence of the severe biliary colic was so prejudicial to him that he (Mr. Gould) was asked to see him with a view to operation. The patient was submitted to it, he bore it very well, and recovered well from it. Last year the same patient had right-sided empyema. He was again sent for, and first aspirated and then drained the right pleural sac. He stood both operations well, but he subsequently had venous thrombosis first in the jugular and subclavian veins on one side and then on the other, and after a prolonged illness he died. He mentioned the case as an illustration of the possibility of operating upon patients who are the subjects of extensive aortic aneurysm.

Dr. ESSEX WYNTER, in reply, said the point he wished to draw attention to was the occurrence of pain in the back and left side, which was so constant, and the peculiar weakness and absence of physical signs. X-rays should be used in those doubtful cases, because unless there was some associated complication, such as dilatation of the arch, aneurysm of the descending thoracic aorta was out of the reach of diagnosis.

Clinical Section.

March 11, 1910.

Mr. A. Pearce Gould, President of the Section, in the Chair.

Detachment of Entire Scalp.

By A. Pearce Gould, M.S.

M. J., a girl aged 16, was admitted to the Middlesex Hospital in July, 1909. She stated that, when at work in a flannel factory, on March 31, 1908, her hair was caught in a machine and her whole scalp was torn off, from the eyebrows to below the occiput, and on the right side the loss of tissue included the upper half of the pinna. Repeated attempts at skin-grafting had been made, but with little or no success. There was a slight growth of epidermis over the lower part of the forehead and on each temporal region, but for the rest the entire calvarium was covered with irregular, pale, poorly-developed, and freely-suppurating granulation-tissue. The girl was in constant pain, and dressing the part caused her considerable suffering. I tried to prepare the large, raw surface for grafting by antiseptic fomentations containing cyllin, carbolic acid, or boric acid, but it was not until I resorted to the use of boiled water only that any marked improvement occurred. Since then I have, on several occasions, applied Thiersch grafts, and the surface is now almost entirely skinned over; the patient has lost all her pain, and is able to close the eyelids, which she could not do before. Three attempts have been made by my colleague, Mr. Kellock, to hasten the repair of the scalp. Once he implanted the lining of a dermoid cyst; on another occasion he raised a large flap of skin from the back of the neck, leaving its ends attached, and after several days, when the

circulation of the flap seemed to be assured, he detached one end and laid the flap over the skull; on the third occasion he covered a large granulating surface with skin excised from a large ventral hernia. I am sorry to say all these attempts failed.

Attention is called to the intolerance of antiseptics and the good effects of the continuous application of sterilized water, and to the success obtained by Thiersch grafts.

Mr. GOULD added that it was only the second case of the kind he had seen. The first was when he was on the staff of Westminster Hospital thirty years ago; a girl was brought in with her entire scalp removed by having her hair caught in moving machinery. The present patient had been in the country fifteen months before she came under his care, and there was then no new epidermis growing over the scalp. There were even now some small points which had not yet epitheliated over, but they were healing rapidly, and he hoped to be able to send her back before the next meeting of the Section.

Case of Obliterative Endaneurysmorrhaphy (Matas's Operation) for Popliteal Aneurysm.

By MAYNARD SMITH, F.R.C.S.

E. W., FEMALE, aged 34, was admitted to hospital on December 8, 1909, suffering from popliteal aneurysm on the right side. She had first noticed the swelling behind the knee in the previous August; since that time it had been increasing in size and she had noticed the pulsation. There had never been pain, nor had the leg or foot ever been swollen; there was no history of injury or undue strain, and her work was of the ordinary domestic kind. She had been married for eleven years, and had two children, aged 10 and 2 years respectively; there was no history of miscarriages and the Wassermann reaction was negative, in fact, there was nothing in the history of the case to point to the ætiology of the aneurysm. A skiagram was taken, and showed well its outline and scattered calcareous masses in its wall.

On December 16 I operated, and, having placed a rubber tourniquet around the thigh, made an incision down to the sac wall and opened the aneurysm in the middle line posteriorly. The interior of the sac was filled with recent clot, and only on one or two places on its wall was there any trace of old clotting. The lining was smooth and glistening, except

where a rough patch here and there marked the position of calcification. The entrance and exit of the parent trunk were at the upper and lower poles of the sac, the former being rather anterior and the latter posterior to the long axis. The aneurysm was thus of the fusiform type, and no operation with a view to restoring the lumen of the artery was feasible. The sac was therefore obliterated by successive rows of continuous Pagenstecher thread-sutures. These lay inside the sac in such a way as to draw the opposite walls together, and in no case pierced the outer coat. This latter was a most important point to observe, as the popliteal vein and its branches were spread out in all directions over the surface of the aneurysm. The wound was closed without drainage. Recovery was uninterrupted, natural colour and warmth soon reappearing in the foot.

Case of Popliteal Aneurysm in both Legs cured by the Matas Obliterative Operation.

By Hugh M. Rigby, M.S.

W. R., male, aged 32, was admitted into the London Hospital in April, 1909, for popliteal aneurysm of the *right* leg. He had noticed aching and stiffness of the knee for one month. He had worked in a brewery up to one week before admission. He gave a history of syphilis. A large pulsating tumour was present in the right popliteal space; pulsation was present in the arteries of the leg, which was swollen and somewhat œdematous. An obliterative Matas operation was performed a few days after his admission to the hospital. The sac was found to extend beneath the calf muscles; the openings of the artery and the floor of the sac were sutured by two rows of chromic-gut sutures and the rest of the sac wall drawn together by several layers of similar sutures. Recovery was complete; the patient returned to work in August. He was re-admitted into the hospital in December. He was then found to have a large pulsating swelling in the upper part of the *left* popliteal space; this had been noticed for six weeks; the swelling extended up the thigh, was very painful, and evidently increasing rapidly in size. Four days after admission the aneurysm was incised and the arterial openings secured by sutures. The aneurysm arose from the popliteal artery just below the opening in the adductor magnus; its wall was much

thinned out and friable; the sac cavity was obliterated from within by layers of chromic-gut sutures. Recovery was uneventful. Although arterial pulsation cannot be felt in either leg, the present condition is quite satisfactory. The legs are not swollen and the circulation is apparently good.

DISCUSSION.

Mr. RIGBY was anxious to obtain the opinions of members as to the results of treatment of popliteal aneurysm by ligature. He had looked up the different methods of treatment adopted in twenty cases at the London Hospital during the last fifteen years, and the results were, in his opinion, unsatisfactory. He had performed the Matas obliterative operation three times, with a good result in each case. One of the other cases was a man, aged 60, who was now at work as a labourer in the fields. He regretted that the present patient had not been put on anti-syphilitic treatment after the first operation, as there was a history of his having had syphilis when he was in the Army, for which he was treated only three weeks. There seemed to have been no intermediate symptoms. At his second admission to the hospital the Wassermann reaction was positive, and he was now being given anti-syphilitic treatment. For this reason he would not be allowed to return to work for some time. The records of the twenty cases referred to were as follows: Nine cases were treated by ligature of the superficial femoral artery in Hunter's canal. In three of these cases gangrene occurred; in two of these cases amputation of the thigh was required; in the other case the gangrene was limited to the toes, which gradually separated. Three cases were treated by ligature of the artery in Scarpa's triangle, with good results. Three cases were treated by ligature of the first part of the popliteal artery; in one case gangrene of the toes occurred; one case was treated by ligaturing the artery above and below the sac, with opening of the sac and turning out the clot. In this case secondary hæmorrhage took place; at a second operation a vessel opening into the sac was found bleeding and was ligatured; the case recovered. One case was not operated on. In two cases the aneurysm was ruptured: in one the sac was excised with a fatal result; in the other amputation of the leg was performed, but the patient died twenty minutes later. In one case the sac was opened, the vessels were ligatured, and the bleeding points secured; the case recovered.[1]

The PRESIDENT (Mr. Pearce Gould) said the Section was much indebted to Mr. Maynard Smith and Mr. Rigby for bringing the cases forward, and it would be satisfactory if other surgeons who had performed this operation would record their experiences. He had not had a case of popliteal aneurysm to treat since the operation was introduced, and, as far as he was aware, none of his colleagues at the hospital had done the operation there.

[1] *Clin. Journ.*, Lond., 1909, xxxiv, p. 263.

Fibroid Lung and Displacement of Heart.

By Sir JOHN BROADBENT, Bt., M.D.

E. P., FEMALE, aged 10, was admitted to Hampstead Hospital in October, 1909. History: Whooping-cough when aged 4; said never to have been quite well since; cough on and off since; used to sweat at night. No family history of tuberculosis. Nine other children, all healthy. Patient slightly cyanosed; slight clubbing of fingers; well nourished; no myoidema; does not look ill. Heart felt beating to right of sternum, almost in anterior axillary line. Dullness over nearly the whole of the right side of chest, with increased vocal resonance, and in places pectoriloquy and cavernous breathing; no moist sounds; Calmette's reaction negative. No expectoration while in hospital; temperature normal for the six weeks whilst under observation.

Sir JOHN BROADBENT added that he recently saw a case: a man, who was admitted to St. Mary's Hospital with a similar condition—i.e., the heart displaced to the right side. That, however, was a case of bronchiectasis, the patient spitting up quantities of offensive and purulent sputum. The man had been in St. George's Hospital twelve years before, and the registrar kindly gave him notes of the case. Those notes showed that the condition twelve years previously was almost similar to the present—i.e., there was displacement of the heart with tubular breathing over the right side of the chest. He was now suffering also from lardaceous disease, the result of the prolonged toxæmia. In the child now shown there was no expectoration or evidence of bronchiectasis.

A Case of Tibio-calcaneal Resection at the Ankle, with Amputation of the Foot.

By C. GORDON WATSON, F.R.C.S.

THE patient, a female, aged 52, was admitted to the Metropolitan Hospital with a melanotic sarcoma growing from the skin between the fourth and fifth digits of the foot. Amputation was performed three months ago by resecting the astragalus, removing the cartilaginous surfaces of the lower end of the tibia and fibula and the upper surface

of the os calcis, amputating the remainder of the foot, and pegging the os calcis to the tibia. Two similar cases were shown at the last session of the Clinical Society.[1]

DISCUSSION.

Mr. WATSON added that in 1907 he showed before the Clinical Society two cases of amputation of the ankle-joint by a new method. They were both labouring men, one aged 30 and the other aged 40, who had had infantile hemiplegia, and were incapacitated from active work on account of the long-standing equinus. In both of them he amputated by excising the astragalus, removing the cartilaginous surface from the lower end of the tibia and fibula, wedging the os calcis between the malleoli, after removing its upper cartilaginous surface, leaving the heel-pad intact. They both secured excellent stumps, and were able to carry out active labouring work. He had since performed two similar operations—one in a man with ulceration of the toes in a partially paralytic limb, and the other the case now shown. The advantage of the method was that it left the original heel intact for walking on; the shortening need not be more than ½ in. The presence of the malleoli made the stump club-shaped, and the patient could use an ordinary boot with a dummy in the foot of the boot. An important detail in the operation was division of the tendo-Achillis to prevent tilting. He put the case in plaster straight away. The pin could be taken out in a fortnight, and the plaster left on a month or five weeks, if necessary. In the previous cases firm, bony ankylosis had taken place. In the present case it was doubtful whether there was movement antero-posteriorly between the os calcis and the tibia, but firm ankylosis was expected.

Mr. A. E. BARKER asked in what way the operation differed from Le Fort's operation. He did not wish to be hypercritical, but he thought this latter operation had been before the profession for many years—i.e., the removal of the lower end of the tibia and fibula, the upper end of the os calcis, and the planting of the os calcis between the bones. He thought the suggestion that this was essentially the same operation as Le Fort's was made on the occasion when Mr. Gordon Watson read his paper last time. He believed it was suggested by Mr. Burghard, but there might be differences, and he would be glad to hear what they were. Admittedly the operation was good and the result excellent.

Mr. GORDON WATSON, in reply, said that when he showed the two cases referred to, he described them as modified Le Fort's amputations, and the late Mr. Clutton, who was the President of the Clinical Society at the time, said that he thought they were an advance on Le Fort's operation, and should be described under another name. Mr. Barker was present at that meeting and agreed with the President. Le Fort, according to his description of the operation, sawed through the lower end of the tibia, removing both malleoli, and

[1] *Trans. Clin. Soc. Lond.*, 1907, xl, p. 277.

sawed through the upper surface of the os calcis, removing half that bone. The point of the present operation was to wedge the os calcis between the malleoli to get union without removing bone, simply taking away cartilage. He did not claim any particular originality for the operation, but Mr. Burghard, to whom Mr. Barker had referred, described the operation in his "System of Operative Surgery"[1] under his (Mr. Watson's) name, which showed that Mr. Burghard considered that the operation was not the same as Le Fort's.

Spleno-medullary Leukæmia: Intercurrent Erysipelas.

By JAMES GALLOWAY, M.D.

DR. JAMES GALLOWAY said that he brought forward this patient not so much on account of the leukæmia from which he suffered, but to show some of the effects of an intercurrent infection on this disease.

The patient, a male aged 47, was sent into Charing Cross Hospital on October 6, 1909, suffering from spleno-medullary leukæmia. The symptoms were severe on admission; he could move about only with difficulty, partly owing to muscular weakness and partly to easily-induced dyspnœa; there was also a considerable amount of irregular fever. The spleen was much enlarged, extending to a point a little way beyond the umbilicus on the right side, and nearly to the pubes; the lymphatic glands were not appreciably increased in size. On December 9 the condition of the blood was as follows:—

Hæmoglobin	46·0	per cent.
Red cells	2,280,000	per c.mm.
Leucocytes	284,000	,,
Polymorphonuclears	65·0	per cent.
Lymphocytes	4·0	,,
Large mononuclears	3·0	,,
Transitionals	1·4	,,
Eosinophiles	1·6	,,
Basophiles	4·4	,,
Myelocytes	20·6	,,
Normoblasts	6·6	,,

Poikilocytosis well marked.

It was decided to treat the patient by means of arsenic, and accordingly he commenced to take 6 mg. of arsenious acid in twenty-four hours, the dose being gradually increased. A month later his general condition had distinctly improved, the spleen having become smaller and

[1] "System of Operative Surgery," 1909, i, p. 200.

the leucocytes having diminished to 108,000 per c.mm. The anæmia, however, had increased, and his condition was by no means satisfactory. The arsenic was increased in quantity till he was taking 16 mg. daily. On January 20 the patient's condition was slowly improving; at this date he complained of a good deal of discomfort with nasal catarrh, and the amount of arsenic was gradually diminished. On January 24 the blood examination was as follows:—

Hæmoglobin	56·0	per cent.
Red cells	2,280,000	per c.mm.
Leucocytes	36,000	,,
Polymorphonuclears	47·6	per cent.
Lymphocytes	8·4	,,
Large mononuclears	6·4	,,
Transitionals	3·6	,,
Eosinophiles	7·6	,,
Basophiles	9·2	,,
Myelocytes	17·2	,,
Normoblasts	40·4	,,
Megaloblasts	15·6	,,

At this time the patient's general condition was not so satisfactory, and he complained a good deal of feelings of nasal discomfort, as if he had a severe cold. In a day or two an eruption of herpes broke out in the neighbourhood of the right nostril, and this was almost immediately succeeded by a rise of temperature, associated with an outbreak of erysipelas commencing near the right nostril, but extending over the whole of the right side of the face, and then involving the left side of the face and part of the scalp. The temperature, during the attack, rose to 103° F. The attack of erysipelas may be stated as having lasted from January 28 to about February 10. On February 3, during the height of the attack, examination of the blood gave the following results:—

Hæmoglobin	56·0	per cent.
Red cells	2,680,000	per c.mm.
Leucocytes	48,000	,,
Polymorphonuclears	43·2	per cent.
Lymphocytes	7·8	,,
Large mononuclears	5·6	,,
Transitionals	1·6	,,
Eosinophiles	3·6	,,
Basophiles	8·8	,,
Myelocytes	29·4	,,
Normoblasts	28·6	,,
Megaloblasts	4·8	,,

As soon as the local discomfort produced by the erysipelas began to diminish, and the temperature to fall, the patient expressed himself

as being much improved in his health, and this improvement increased till within a few days he stated that he was better than he had been for months, and, so far as could be judged by his appearance and his ability to walk and move about with comfort, the improvement was remarkable. No arsenic had been administered to the patient since February 3.

The improvement in the patient's health still continues; he is able to move about with comfort. The anterior border of the spleen is now 3 in. to the left of the umbilicus; his temperature varies only slightly from the normal, but the number of leucocytes has increased. On February 15 there were:—

Hæmoglobin	...	...	...	...	60·0 per cent.
Red cells ...	...	...	...	...	3,150,000 per c.mm.
Leucocytes	...	...	...	...	148,000 ,,

Within the past week the cervical lymphatic glands became enlarged suddenly, producing a little pain and discomfort. The patient's health, however, was not interfered with, and since this enlargement the leucocytes have again diminished. On March 11 the condition was:—

Hæmoglobin	...	...	...	...	60·0 per cent.
Red cells ...	...	...	...	...	3,440,000 per c.mm.
Leucocytes	...	...	...	...	48,000 ,,

The patient has taken short courses of arsenic in small doses between February 15 and March 6, but the drug has been discontinued since that date.

Dr. Galloway desired to emphasize one or two points in the history of this case. First, the remarkable increase in the number of red blood cells at the time of the greatest diminution in the number of leucocytes on January 24. This date was coincident with the conclusion of the first course of arsenic and with the commencement of the uncomfortable nasal sensations which culminated in the attack of erysipelas. Secondly, he wished to draw attention to the very obvious improvement in the patient's general condition, appreciated by himself and easily noted by others immediately after the attack of erysipelas abated. The influence of accidental infections on the course of leukæmia has not infrequently formed a subject of comment, but they were of sufficiently rare occurrence, and raised so many points of importance as to the progress of the disease, and possibly even as to methods of treatment, that Dr. Galloway hoped that such a case as this would not fail to interest members of the Section.

DISCUSSION.

Dr. H. D. ROLLESTON had seen cases of leukæmia in which the occurrence of an abscess, due in one case at least to the hypodermic injection of arsenic, had been followed by a marked diminution of the number of leucocytes, but unfortunately this did not lead to any real permanent benefit to the patient. He had also tried the injection of antistreptococcic serum, but it had not produced any special change in the blood picture. The striking influence of streptococcic infection on the bone-marrow in leukæmia, as shown by the altered blood-count, suggested that the beneficial effect of arsenic in this disease might be due to an analogous effect on the bone-marrow. Conceivably the good effects of arsenic might be due (*a*) to its action on the bone-marrow, or (*b*) to an antiseptic effect exerted on the alimentary canal. Dr. Drysdale had had a most remarkable result in a case of acute myelogenous leukæmia treated with naphthaline tetrachloride, the patient being apparently cured. This might be regarded as evidence in favour of the view that arsenic did good in virtue of an antiseptic effect exerted in the alimentary canal. The effect of erysipelas and streptococcic infection on the other hand pointed to the probability that arsenic produced its effect on the blood-count in virtue of a direct influence on the bone-marrow.

Dr. F. PARKES WEBER said that if erysipelas really had a favourable effect upon leukæmia, it would perhaps be justifiable to try treatment by Coley's fluid in cases of leukæmia. But in Dr. Galloway's case one could not jump to any conclusion as to the effect of the erysipelas; in fact, it was remarkable that the great improvement in the blood-condition seemed to have taken place before the fever of erysipelas occurred. Other toxins had been used in leukæmia, at all events tuberculin had been, but with undecided results.[1]

Dr. GALLOWAY, in reply, said that he should be very unwilling to propose the employment of the infection of erysipelas as a means of treatment for splenomedullary leukæmia! Cases such as the one shown this evening must, however, give rise to the consideration of the effects of a streptococcic infection on the disease. He would draw attention to a paper by Dr. George Dock on "The influence of complicating diseases upon Leukæmia,"[2] in which a critical résumé of the subject up to that time was given, including references to cases of streptococcic infections. The therapeutic aspect of the question had not been absent from Dr. Dock's mind on writing this paper. Dr. Galloway agreed with Dr. Weber's suggestion, that a preparation possessing the properties of a vaccine prepared from the infecting micro-organisms might possibly have

[1] W. Weitz (*Deutsch. Arch. f. klin. Med.*, Leipzig, 1908, xcii, p. 551) treated leukæmia by tuberculin injections, and found that the action of the tuberculin appeared to be favourable until the patient's organism became accustomed to the toxins contained in the tuberculin.

[2] *Amer. Journ. Med. Sc.*, Philad., 1904, N.S. cxxvii, p. 563.

an effect when a serum would be inefficient. Dr. Galloway regretted that he had not isolated the micro-organisms during the attack mentioned, but there could be no doubt that it was a characteristic attack of acute facial erysipelas.

"Trophic" Dermatitis commencing on the Extremities.

By JAMES GALLOWAY, M.D.

MALE, aged about 65, has suffered from the present condition, in varying degrees of severity, for the past seven years. The disease commenced on the hands and feet, but now involves the greater part of the surface of the legs, and has affected numerous areas on the arms and trunk. It spreads partly by continuity, but also by the development of separate, discrete lesions. The tips of the fingers and toes have been severely affected, many of the nails have been injured and some totally destroyed. The disease commences by the appearance of small "phlyctenular" lesions, which contain a small amount only of a gummy serous exudation, at first apparently free of micro-organisms; these lesions coalesce and cause exfoliation of large patches of epidermis, almost denuding the underlying corium. The reddened areas thus produced have wide circinate outlines, and while the disease is spreading at their edges show an overhanging margin of epithelium becoming gradually detached. A large amount of epithelium is constantly being exfoliated in large flakes.

The patient shows some degree of general arteriosclerosis, but with this exception no definite visceral lesions have as yet been ascertained. The malady appears to belong to the class of disease to which the name of "dermatitis repens" has been given; it is of rare occurrence, and this case is shown as an example of the cutaneous manifestation of some morbid condition producing "trophic" degeneration of the epidermis. The cutaneous lesions occurring in chronic arsenical poisoning and in pellagra suggest themselves as being analogous conditions.

Dr. GALLOWAY invited any member to see the case at the Charing Cross Hospital, as the patient was too ill to attend.

Bluish Pigmentation of the Skin.

By F. Parkes Weber, M.D., and R. H. Norman, M.D.

The patient, an unmarried woman aged 46, is remarkable for a decided, though slight, ashy-bluish tinge of her skin, which is best seen on the forehead, at the sides of the nose, around the mouth, and on the trunk, but is scarcely noticeable on the arms and legs. The mucous membrane of the mouth has a very slight slaty tinge, similar to, but less marked than, that of the face. On the parts flushed with blood, including the lips, tip of the nose, pinnæ of the ears, and the fingers, the peculiar coloration can only be detected when the blood is pressed out of the skin—for instance, by a flat piece of glass or a glass lens. There is no pigmentation of the sclerotics, such as is found in ochronosis. There is no chromidrosis or "seborrhœa (steatorrhœa) nigricans." There is no true cyanosis, except sometimes in cold weather, when the hands become blue. Nothing abnormal can be detected in the thoracic or abdominal viscera at present. Neither spleen nor liver is enlarged. In 1890, however, she was treated in a London hospital for gastric ulcer. Since then she has suffered occasionally from pains in the epigastrium or chest after eating. In May, 1907, she was operated on by Mr. E. C. Stabb, at the Great Northern Hospital, for hernia, a femoral hernia on the right side being found to contain an adherent vermiform appendix, which was excised. The bluish colouration of the skin was present at that time, and was thought to have come on gradually about six years previously (about 1901). Examination of the patient's blood (Dr. G. Dorner) gives the following results: Hæmoglobin (by Sahli's method), 80 per cent. of the normal; red cells, 4,653,330 in the cubic millimetre; white cells, 12,330 in the cubic millimetre. Stained blood-films show nothing abnormal. By spectroscopic examination only the normal absorption bands of oxy-hæmoglobin can be seen; there is certainly no band due to sulph-hæmoglobin or methæmoglobin. The blood-serum is clear. The urine is free from albumin, sugar, and other abnormal constituents, and does not darken on being kept. Systolic brachial blood-pressure, 135 mm. Hg. Menstruation natural. Excepting for chronic constipation and occasional headache, the patient has no complaints. She thinks that the peculiar colouration of her skin is not progressing. She has no idea what could have caused it. There

is no evidence pointing to the possibility of argyria. No lotions (such as might have contained silver nitrate or carbolic acid) have ever been used. A patent medicine and some "liver pills," which she has employed for many years as purgatives, have been chemically examined for a trace of silver, but with completely negative results. By exclusion, therefore, the peculiar colour of the skin seems to be due to a form of hæmochromatosis of obscure origin.[1]

Addendum.—Dr. Weber afterwards discovered that the patient had, after all, been taking nitrate of silver. A pill containing $\frac{1}{4}$ gr. nitrate of silver and $\frac{1}{2}$ gr. opium, to be taken three times daily, was prescribed for her on account of the gastric symptoms in December, 1889, and she continued taking this pill on and off for quite two years after leaving the hospital in 1890, and then continued to take it occasionally for gastric pain until seven years ago.

DISCUSSION.

Dr. C. F. MARTIN (Montreal), at the invitation of the President, said that the Montreal case resembled the present one in a measure, but was much more pronounced. Its interest was pathological, as well as clinical, inasmuch as the case came to necropsy, and the sections of the various organs showed marked pigmentation. The diagnosis originally made was pigmentary cirrhosis of the liver, as that organ was somewhat enlarged, but otherwise there was no evidence of a cirrhotic condition clinically. The necropsy, too, revealed very little cirrhosis, though the enlargement of the organ was present; apart from that, there was no condition to explain the pigmentation, and the examination of the various tissues showed marked hæmochromatosis.

Dr. GALLOWAY said that Dr. Weber's patient in some respects resembled in appearance, cases of hæmochromatosis which had come under his observation. The most remarkable example of this type of pigmentation the speaker had seen was in the case of a young woman admitted seriously ill to Charing Cross Hospital. She was greatly emaciated, but the abdomen was extensively distended with fluid. The patient was of a deep greyish-blue slate colour from head to foot, the tint, however, varying slightly, being more intense in the flexures and similar situations. It was decided to explore the abdomen, and on operation a large quantity of fluid was removed, but the only lesions found were small quantities of adherent lymph, occupying mainly the upper and posterior aspects of the abdomen. Before the operation the patient was so seriously ill that little hope was entertained of her recovery. But she recovered rapidly from the operation; she gained in strength and health

[1] Compare the case of a patient, known as "Blue Mary," described by Dr. Maude Abbott in the *Trans. Path. Soc. Lond.*, 1900, li, p. 66.

daily; there was no re-accumulation of fluid, and slowly the pigmentation, which had previously been so striking, began to diminish. It was now some four years since the operation was performed, and the patient had been under observation from time to time. She had now the aspect of a vigorous healthy woman, aged about 30, was hard at work as a teacher in an elementary County Council school, and was able to do unusually hard work without serious strain. Examination of the abdomen now showed nothing abnormal, and the remarkable pigmentation had almost vanished. She said, however, that she had not recovered her original colour. Faint traces of pigmentation might still be detected. Dr. Galloway had made efforts to bring the patient before the Section, and hoped that he might still be successful in inducing her to come.

Dr. THEODORE THOMPSON said that he considered the case to be one of argyria. He understood that this patient had been under treatment for gastric ulcer in 1892, and that the pigmentation had come on since that date. Was there not the possibility that she might have been treated with silver nitrate for the gastric ulceration? He had recently seen a case of hæmochromatosis, but in this case, as commonly, the pigmentation was a late feature in the disease, and only followed the cirrhotic liver and glycosuria after many months. The pigmentation of the skin was dark brown and patchy, and was little marked on the face, and not at all like the dull leaden hue of this patient.

Dr. H. D. ROLLESTON had seen two cases of hæmochromatosis confirmed by necropsy. A curious feature about the condition was that it was less rare in men than in women; he believed there was no case reported in a woman in which the condition had progressed to its full extent—i.e., to produce so much fibrosis of the pancreas as to pass into bronzed diabetes. This case, if it was one of hæmochromatosis, was in a very early stage, as he understood there was no enlargement of the liver. He suggested that the pigmentation might be due to arsenic, a drug which was commonly given for any obscure condition.

Dr. WEBER, in reply, said he did not think the patient had had arsenic, and one would scarcely give arsenic for gastric ulcer; the purgative pills she had been taking did not contain that drug.

Exophthalmic Goitre in a Man with Symmetrical Telangiectases of the Ocular Conjunctivæ.

By F. PARKES WEBER, M.D.

THE patient is a man, aged 42, who states that, with the exception of scarlet fever during childhood and (apparently) articular rheumatism at the age of 16, he enjoyed good health till March, 1909. He then noticed that he was losing weight, and in April, 1909, a doctor whom

he consulted drew his attention to the prominence of his eyes and the swelling in his neck. During the summer of 1909 he had considerable diarrhœa, and was troubled with profuse night-sweats. Since the commencement of his illness he has felt nervous and "on the twitch," and has been subject to palpitation on any excitement or muscular exertion, but there is no history of any fright or mental shock which can in any way be connected with the onset of his symptoms. There is nothing known of special interest in the family history (no insanity, hysteria, epilepsy, or other nervous diseases).



Exophthalmic goitre with symmetrical conjunctival telangiectases.

He is a thin man, 5 ft. 5¾ in. in height, and only 9 st. 10 lb. in weight, though his former average weight is stated to have been 13 st. 7 lb. There is considerable bilateral enlargement of the thyroid gland, the right lobe being slightly larger than the left. A systolic vascular thrill can be felt over the upper part of both lobes. In the hospital the pulse has varied from 74 to 120 per minute. The highest pulse-rate in the morning has been 88, in the evening 120; during sleep the pulse-rate

has been 85 to 96. No evidence of organic disease in the heart can be detected. There is much exophthalmos (bilateral, symmetrical), and both von Graefe's and Stellwag's signs are well marked. The pupils react normally to light and accommodation. The knee-jerks can be obtained, but are not excessive. There is a variable degree of fine tremor in the hands and fingers. Over a circumscribed area of the sclerotic at the outer angle of each eye the loose conjunctiva is thickened, and contains dilated blood-vessels. This condition is probably of an angiomatous nature, and may be congenital, though the patient apparently was unaware of its existence until his attention was recently directed to it.

Congenital Tumour in the Left Parotid Region (? Cavernous Lymphangioma).

By F. Parkes Weber, M.D.

The tumour is soft, flaccid, and slightly fluctuating, about the size of half a Tangerine orange, and situated subcutaneously between the masseter muscle and the skin. It extends from the condyle almost to the angle of the jaw, and from the tragus of the ear to a point on the cheek corresponding to the first molar teeth. The skin over it is normal and freely movable. By firm pressure the tumour can apparently be made slightly smaller. It is not painful or tender to handling. The patient, a young woman aged 22, who has signs suggesting slight pulmonary tuberculosis, states that the tumour has given her no trouble. According to her father it has been present from birth. It feels something like a lipoma, but when examined recently the most anterior portion of it (in the region of the socia parotidis) was found to be cystic. About 2 c.cm. of a nearly clear, straw-coloured fluid (withdrawn by a small syringe) gave a slight coagulum on standing. The fluid "went almost solid" with albumin on boiling, but no reaction for the presence of mucin was obtained. Rivalta's test gave a completely negative result. The scanty centrifuge sediment consisted of cholesterol crystals, fat globules, and cells stuffed with fatty granules.

The President said that the bimanual method of examination demonstrated that there was a small and very loose cyst in the anterior part of the parotid gland. He agreed with the view expressed by Dr. Parkes Weber that it was a cavernous lymphangioma.

Case of X-ray Carcinoma.

By J. Howell Evans, M.Ch.

This specimen, the hand and lower half of the forearm, was removed from a patient who was one of the earliest experimenters and practitioners of radiotherapy in this country. His work followed directly upon Röntgen's publication, and the injuries which have culminated in the condition presented were set up before the usual protective measures of to-day were known.

Two Cases of Myotonia Atrophica.

By H. G. Turney, M.D.

Case I.

C. S., male, aged 38, the seventh in a family of fifteen, of whom thirteen are living. No history of syphilis or other serious ailment. He dates his present complaint back three and a half years, when he noticed weakness and wasting of the right hand. He has never noticed any difficulty in starting a movement or in relaxing his muscles after one. He has noticed nothing wrong with his lower limbs.

On examination: Patient is found to display extensive muscular atrophy associated with weakness. The face is of the myopathic type with marked weakness of the orbicularis palpebrarum and frontalis. In the neck both sternomastoids are almost completely gone, as is also the lower part of the trapezius. The deltoids are well preserved, but the biceps is much wasted, especially on the right side, and has hardly any power. In forearm and hand there is a general wasting somewhat varying in degree on the two sides. The trunk and buttock muscles are normal, as also the thighs, but in the leg there is very marked wasting of the tibialis anticus and flexor longus digitorum, the peronei being fairly preserved. The calf muscles are small, but would pass muster except that there is marked shortening.

Electrical reactions: There is very marked reduction in irritability to the faradaic current amounting in the biceps and hand muscles

almost to extinction. In the long flexors of the fingers there is an imperfect myotonic reaction shown both on volitional movement and electric stimulation. The patient is able to relax his grasp at once, but only to an extent that leaves the fingers somewhat in the position of tetany; from this they gradually relax. On faradaic stimulation the same occurs. Mechanical irritability shows no change, except perhaps some reduction. Patient's gait is noticeably brisk and of the high-stepping type on account of the shortening of the calf muscles. Reflexes are normal, and there are no signs of nervous disease. Fibrillar tremors are absent.

Patient has a son, aged 10, who from the description is in a similar condition. No other cases are known in the family, with the exception of the sister now to be described.

Case II.

L. S., female, aged 33, sister of the above and the eighth member of the family of fifteen. She noticed the wasting of the muscles of the left upper limb about eight months ago, and regards that as the beginning of her complaint. She was a good walker, and not much more than a year ago was able to do something like fifteen miles in the day.

The general condition resembles that of her brother. There is the same weakness of the orbicularis palpebrarum, but the orbicularis oris is very little affected. The masseters act well, but the left is notably smaller than the right. The tenuity of the facial soft tissues generally is very striking. The voice is somewhat nasal, but there is no paralysis of palate or larynx, and she has not noticed any change in it herself. The sternomastoids are much wasted and very weak, but their condition is not so extreme as in her brother's case. The lower part of the right trapezius is very atrophic, as also the left, but to a slighter degree. In the upper arm the deltoids are so well nourished that they suggest hypertrophy, and their power is good. The biceps and triceps are both weak and wasted, more on the right side than the left. In the forearm and hand all muscles are small and the seat of actual wasting of varying degree, the most advanced being the intrinsic muscles of the right hand. In the thighs the vastus internus on each side, but more particularly the right, is atrophic. In the legs the wasting is general, but the anterior tibial groups have suffered most. On the right side the peronei act fairly well, on the left hardly at all.

Electrical reactions show the same diminished or lost irritability to faradaic stimulation as in the case of the brother, but there is no myotonic reaction either to electrical or mechanical irritation. The patient is able to relax her grasp quite rapidly, but, though the grasp is relaxed, the fingers are left with some degree of spasm for several seconds, with slight flexion at the metacarpo-phalangeal joints and extension at the others. For a long time and at repeated interviews the patient could not be brought to acknowledge that she had experienced any difficulty in relaxing her muscles, but when the condition just described was pointed out to her she owned up to having noticed a feeling of stiffness in the fingers under similar circumstances.

All reflexes are normal and there are no signs of nervous disease.

Dr. F. E. BATTEN said these cases corresponded most closely with those which had been described under the term "myotonia atrophica." The questions which arose were whether they were really cases of myopathy or were they cases of Thomsen's disease, on which myopathy was engrafted, or were they due to some changes in the spinal cord? These questions must await decision until further pathological evidence was forthcoming. Only one case had been examined pathologically, and that was by Steinert, and the result showed changes in the muscles which were similar to those found in myopathy. In that case certain changes were found also in the posterior columns of the cord, but Steinert suggested that these were accidental, and not part of the disease. Hoffmann, who originally described the cases, believed that they were cases of Thomsen's disease, upon which myopathy was engrafted, and in support of this view quoted cases of Thomsen's disease in which the myopathic condition had developed later. In the cases he (the speaker) had seen, however, this was not so; myopathy had been the earlier manifestation.

Case for Diagnosis.

By ARTHUR E. BARKER, F.R.C.S.

PATIENT, a female, aged 34, states that a small nodule was removed from the middle of her palate thirteen years ago. About six years ago a lump appeared on the left upper gums and became nodulated. It has since spread across the hard and soft palate and over the left tonsil. A gland appeared under the left angle of the jaw about the same time, and is still enlarged. In spreading the growth can hardly be said to

have ulcerated, but its inner border is raised and indurated and its middle seems to have lost substance. It looks like an epithelioma, but its long duration and the patient's age seem to negative this view. The case is put forward for diagnosis.

DISCUSSION.

Mr. BARKER said the case presented very great difficulties, inasmuch as the young woman had a growth spreading, during the last six years, from the gums, over the palate, and back into the posterior nares and over the tonsil on the left side. He was inclined to regard it as a tuberculous process, so-called lupus. The President suggested a diagnosis, but he did not agree with it. He would have been glad to have had help from physicians and surgeons as to the nature of the case.

Mr. HOWELL EVANS said that, from the clinical point of view, he regarded the case as "endothelioma." He regarded endotheliomata, which were spoken about as being of rapid growth, as likely to occur particularly in connexion with lymphatic spaces and such situations where slight congenital anomalies occurred—for example, lymphangiomata. During the last four years he had removed four tumours of that nature in connexion with the palate. In the first case he diagnosed the tumour as endothelioma, and the microscope bore this out. Encouraged by such diagnosis, he felt that it was possible to appreciate the distinctive clinical features of endothelioma.

Dr. H. D. ROLLESTON considered that histologically the growth was an endothelioma, and not a squamous-celled carcinoma. There was also some inflammation, and that might depend on the ulceration. He thought the tumour resembled the benign tumours of the palate.

Dr. FINZI said he had seen a case similar to the present one, clinically and in microscopical section. The disease started at the posterior part of the hard palate, and spread forward from there; the total duration before death was six years. After about three-and-a-half years the upper jaw was removed, and the disease was found extending back to the pterygoid process of the same side, and going back behind the ramus of the inferior maxilla. The sections were similar to those in Mr. Barker's case, and there was always a good deal of discussion wherever he showed the sections; some said that it was carcinoma of the mucous glands, others that it was endothelioma.

Case of Myositis Ossificans Traumatica.

By P. MAYNARD HEATH, M.S.

J. S., MALE, aged 42, a horsekeeper, was kicked on the left thigh by a horse three months ago. Very extensive bruising appeared two days after the accident, and a small lump on the front of the thigh was noticed two days later. The lump has gradually increased in size. The thigh was very painful soon after the accident, but the pain has gradually decreased.

The patient is a healthy-looking man. On the front of the left thigh is a discoloured patch of skin, the site of the kick. Under this is a swelling. It is very hard, fixed to the femur, and involves the deeper layers of the muscles. The lower edge of the swelling is sharply defined and overhanging. Above and at the sides the swelling is ill-defined. It extends more to the outer than to the inner side. The knee can be fully extended, but flexion is very limited owing to the involvement of the extensor muscles in the mass. No other tumours can be felt in the body, and there are no enlarged glands. There is no evidence of a previous attack of syphilis.

The skiagram, by Dr. Finzi, shows a bony mass with two attachments to the femur, one at the upper end and the other in the middle of the swelling. The lower end of the mass is free. The bone is deposited in loosely packed laminæ with spaces between, unlike the cancellous tissue usually seen in exostoses. The femur itself is not affected except at the points of attachment of the mass.

A Case of Acromegaly.

By E. I. SPRIGGS, M.D.

A. P., AGED 61. Height, 5 ft. 3 in.; weight, 11 st. 13 lb. No member of her family in this or the last generation has had any similar affection. Enlargement of the hands was first noticed by her when 45 years of age: about that time the catamenia ceased. A little later the feet began to get larger. She has been subject to headaches, but

less so in the last fifteen years. At the present time the hands are broad and the fingers large; the nails are longitudinally ribbed. The feet are $9\frac{3}{4}$ in. long, and have a good arch. The enlargement is chiefly of the bones. There is variable tingling and numbness of the hands. There are no pains in the legs, but they feel weak at times. The hair is coarse and much scantier than formerly. The skin of the face is thick, the lower jaw is much enlarged, as are the lower lips, the tongue and the palate, and the ears and nose. The lower teeth project much in front of the upper when the jaws are closed. The voice is less clear than it used to be. The clavicles feel big. The chest measures 35 in. in expiration and $36\frac{1}{2}$ in. in inspiration. There is no kyphosis. Mr. Grimsdale has kindly examined the eyes and reports that the pupils are equal and react. Vision: Right $-$ 1·0 $= \frac{6}{6}$. Left $-$ 1·0 $= \frac{6}{6}$. Field of vision: to white, normal; to green, a small field, reduced in the outer half on both sides. Wernicke's hemiopic reaction not obtained. No optic atrophy; the retinal veins a little tortuous. No lesion of the third nerve. The patient suffers from a moderate degree of thirst and sweating, and says she is sometimes drowsy and cannot do her work well as a seamstress. Pulse-rate, 68. Urine abundant, specific gravity 1020, acid; a faint trace of albumin; no sugar.

Dr. Allpress Simmons reports that a skiagram of the hands shows an irregular expansion of the distal and proximal ends of all the phalanges, and to a less extent of the metacarpal bones, accompanied by some rarefaction; the soft tissues also are somewhat increased in thickness. A skiagram of the skull shows abnormal thickness of the cranial bones.

The patient sought advice for bronchitis. On November 19, 1909, 2 gr. a day of pituitary tabloids were given. Fourteen days later the patient complained of swelling of the joints of the fingers on the left hand and of the nose; one of the phalangeal joints was visibly enlarged. The dose was reduced to 1 gr. a day. The skin of the hands has become clearer under treatment, and the subcutaneous thickening less.

Four photographs of the patient at the ages of 23, 34, 45 and 61 (present time) were shown on the screen. The patient, as above stated, dated the onset of the affection at about 45 years of age; it appears, however, from the photograph taken at that age that the disease had then been in progress for some time.

Extensive Myositis Ossificans.

By Theodore Thompson, M.D.

A. B., aged 43, farm labourer. When 8 years old he noticed his left shoulder becoming swollen and painful. It was kept in a sling for some time, and on taking it out the shoulder was found to be stiff. He was quite well till the age of 16, when he noticed pain and swelling at the bottom part of his back, but was able to go on with his work. At the age of 28 the left leg and thigh became stiff, so that he could only walk with difficulty. Later the right arm became affected so that he could not move his shoulder or elbow. He gave up work when he was aged 31, and has only done occasional jobs since. There is no family history of the disease. Most of the muscles are infiltrated with hard, bony growths, so that his back is perfectly rigid. His right arm is fixed at the shoulder. He is unable to abduct the legs, but can just manage to shuffle along on his feet. The masseters are infiltrated with bone, and the jaw is fixed. There is a curious deformity of both great toes which are markedly stunted. Double hallux valgus is present.

Skiagrams show muscles infiltrated with bone (fig. 1).

A portion of the right pectoralis major was removed under cocaine anæsthesia, and sections were made of (1) a portion of muscle near the calcareous area, and (2) a portion of tendon and hard, white bony substance.

Pathological Report by Hubert Turnbull, M.D.

(1) The non-calcareous fragment—fixed in formalin, stained in hæmatoxylin and eosin and van Gieson—consists of voluntary muscle, together with some tendon and lipomatous tissue (fig. 2). Very few of the muscle fibres are of normal appearance, with cross striation. The majority of the fibres show wavy longitudinal fibrils slightly separated from one another, with no cross striation. There is no cellular infiltration, nor fibrosis of the muscle. Some hæmorrhage is present, but is doubtless due to the operation for removal.

(2) The calcareous fragment—fixed in formalin, decalcified in nitric acid, &c., stained in hæmatoxylin and eosin and van Gieson—consists of tendon, lipomatous tissue, cartilage, bone and marrow; no muscle is

present (fig. 3). One surface of the bone lies upon tendon save at one or two spots where it rests upon portions of cartilage. This cartilage shows a zone of calcareous impregnation next the bone; on its aspect furthest from the bone it merges into tendon. One small portion of bone at its junction with the tendon shows the structure of coarse fibrillar or "membranous" bone. All the rest of the bone is fine fibrillar or "lamellar" bone. It contains Haversian canals and systems and shows



FIG. 1.

Radiograph of the right knee-joint, showing ossification in the muscles about the joint.

a mosaic of portions of bone, each of which has its own lamellar system. A study of these shows that there has been much alteration in the architecture of the bone, systems having been from time to time partially removed and new systems applied. Only a few osteoblasts are present on the free surfaces of the bone, and there is no visible osteoid zone.



FIG. 2.

Showing bundles of muscle-fibres. The small dark areas correspond to groups of red corpuscles.



FIG. 3.

To the right is lamellar bone with Haversian canals and systems. In the centre is a portion of fibro-cartilage which shows a dark zone of calcification at its junction to the bone. To the left is tendon, which has become detached here from the cartilage in the lower three-quarters of the field and from the bone in the upper quarter.

The growth appears, therefore, to be stationary at present. The marrow is represented by lipomatous "supporting tissue." No blood-forming cells are present therein.

Unfortunately, the sections do not give any indication of the processes which led to ossification. On examination of the patient it appeared to me that the bone was always at the junction of muscle with tendon or periosteum.

DISCUSSION.

Dr. E. I. SPRIGGS said the latter patient was, through the kindness of Dr. Rolleston, in one of his beds two or three years ago, when he was engaged in investigating the metabolism of creatinin and uric acid in some diseases of the muscles. The results had not been published, but he remembered that the disturbance in the excretion of creatinin was not great. The conclusion he drew was that the great fixation the man suffered from was at that time due to a comparatively small amount of bone, compared with the total bulk of muscle; for in diseases with great wasting of muscle, there was much diminution of the excretion of creatinin.

Dr. PARKES WEBER asked whether Dr. Thompson knew the results of microscopical examination in other recorded cases. He understood him to say that the newly-formed bone contained no ordinary bone-marrow elements. Perhaps the newly-formed bone had been examined at an early stage before any bone-marrow tissue had had time to develop in connexion with it.

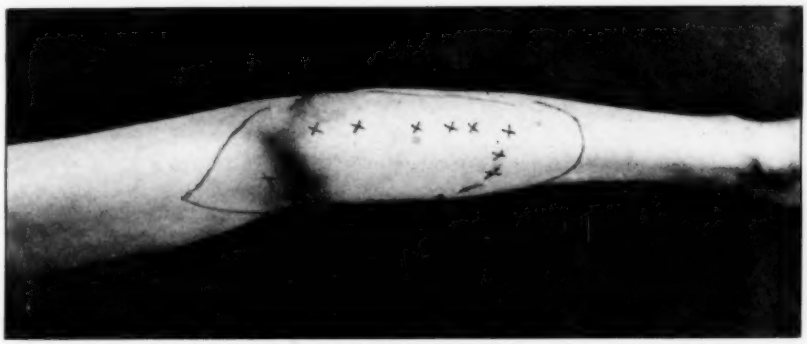
The PRESIDENT asked whether Dr. Thompson was satisfied that all the masses of abnormal bone were connected with muscles, or whether any of them were outgrowths from the periosteum.

Dr. THOMPSON, in reply, said he understood from Dr. Spriggs' experiments on the chemical side that he also thought it was not likely to be an inflammatory condition of muscles, but a degenerative condition. The German view seemed to be that it was an inflammatory condition, with secondary bone formation. In this case, bone seemed to invade the muscle from the periosteum; wherever muscle was attached to periosteum, the growth seemed larger and nearer to the bone, and got less as it invaded the muscle. That was much as was found in ordinary traumatic myositis ossificans. There were masses of bone which appeared much more like exostoses than calcified or bony muscle.

Cervical Ribs.

By THEODORE THOMPSON, M.D.

J. P., aged 21, bootmaker, has noticed weakness of right hand for past six weeks. For some time he has noticed a feeling of pins and needles on the inner side of right forearms. This area appears to him to be numb. He is unable to work on account of wasting of right hand, which he thinks is getting worse. He has four brothers and four sisters, none of whom have any similar trouble, nor is there any similar trouble



Photograph of inner side of right upper limb. Within the area bounded by crosses all cutaneous sensibility was absent, but deep sensibility was present. Within the continuous line, sensibility to light touch and tactile discrimination were absent.

among his relations as far as he knows. Definite wasting of thenar, hypothenar, and interossei muscles of right hand. The right grip less than left. Dynamometer: Right, 30; left, 120. There is loss of sensation to light touch over an area extending from about 1 in. above elbow to middle of forearm on its inner aspect (*see* fig.). Within this area is one showing loss to painful and thermal sensibility. There is no loss to deep pressure. This type of anæsthesia is typical of a peripheral nervous lesion, and areas of epicritic and protopathic anæsthesia are demonstrated. On the right side of neck the subclavian artery can be seen to be pulsating more markedly than on the left. On both sides a systolic thrill can be felt and a murmur heard over the artery. Radiograph of cervical spine shows double seventh cervical ribs.

DISCUSSION.

Dr. PARKES WEBER said he heard that Dr. Thompson's patient had four brothers and four sisters. It would be interesting if X-ray photographs of their necks could be taken, to see if cervical ribs were present. In a case of cervical ribs with atrophy of hand-muscles, which he (Dr. Weber) showed before the Medical Society of London,[1] on April 26, 1909, the patient's brother and sister were both found to have cervical ribs, though they had not any atrophy of hand-muscles.

Dr. THOMPSON replied that he described a family in *Brain*, some time ago, and a large number of members of it had wasted hands and cervical ribs. He had not been able to trace the other members of the family from which the present patient came.

[1] *Trans. Med. Soc. Lond.*, 1909, xxxii, p. 191.

Clinical Section.

April 8, 1910.

Mr. A. Pearce Gould, President of the Section, in the Chair.

Acholuric Jaundice.

By F. H. Jacob, M.D.

Slight yellowness noticed two years ago; weakness one year ago. First seen October 27, 1909. Has continuous slight jaundice with exacerbations, an enlarged spleen, great irregularity in size of red cells, and a number of nucleated red corpuscles.

(Patient too ill to be brought up from Nottingham.)

Dr. H. D. Rolleston regarded the case as one of acquired hæmolytic jaundice depending on increased fragility of the red blood-corpuscles. The case conformed to the general rule that the cases of acquired hæmolytic jaundice presented more severe symptoms than the hereditary and congenital cases. Another point of interest about this case was that during exacerbations the urine, which at other times was acholuric, contained bile pigment. For this reason it was perhaps more convenient to use the term hæmolytic jaundice rather than acholuric jaundice for this class of case.

Sarcoma of the Ear and Mastoid Region under Radium Treatment.

By N. S. Finzi, M.B.

This boy, aged 8, complained of earache in May, 1909, and was found to have a polypus. This was removed at St. George's Hospital, but immediately recurred. It proved to be sarcoma, for which the ear and mastoid region were operated on by Mr. Fedden at the beginning

of August. A recurrence was operated on, October 3, 1909; but a further recurrence took place, and this was treated by Coley's fluid in November, a large number of injections being given, but only two into the growth itself: there was temporary improvement, after which, nevertheless, the growth made steady progress. For the appended reports I am indebted to Dr. R. S. Trevor:—

Pathological Reports.

June 8, 1909: "Fibroma covered with squamous epithelium. The fibrous tissue shows myxomatous degeneration, and contains cysts due to blocked lymphatics, the endothelium of which shows definite proliferation."

August 7, 1909: "The stalk of the tumour consists of fibrous tissue. The tumour is covered with squamous epithelium, and is composed of myxomatous and fibrous tissue supporting blood-vessels. There are also areas of round and oval cells of endothelial type. The tumour suggests a combination of septic granulation-tissue and endothelioma of lymphatic origin."

October 2: "In this specimen the areas of large cells of endothelial type are very definitely marked; and, further, in places, proliferation has occurred to such an extent as to produce a definite picture of sarcoma. The sarcomatous part is of irregular-celled type. The growth is inflamed and œdematous."

When he came under my care on February 15, 1910, he had a large fungating bluish mass behind the ear about 1¼ in. in diameter, and also a mass from the meatus, both of which discharged freely; and there were large fixed glands below the ear and under the sternomastoid, also small glands at the apex of the posterior triangle.

Radium treatment was given as follows:—

Date	Pure radium bromide (mg.)	Filters					Total dose (hours)	Parts treated
		Silver (mm.)	Platinum (mm.)	Lead (mm.)	Rubber (mm.)	Lint (layers)		
Feb. 21	50	0·5	—	1·0	—	4	12½	Glands
	17	—	1·0	1·0	1·0	4		
,, 22	50	0·5	—	1·0	1·0	—	15	On meatus
	17	—	1·0	1·0	1·0	—	18	Behind ear
March 30	17	—	1·0	1·0	1·0	—	44	In meatus
	50	0·5	—	1·0	2·0	2	10	Lowest glands
,, 31	50	0·5	—	1·0	—	—	13	Middle glands
	50	0·5	—	1·0	—	—	21	Behind ear
	50	0·5	—	1·0	—	—	8	Apex of posterior triangle

In a week's time from the first application there was marked improvement, the growth and glands being smaller and much less tender, and this has since continued. The general condition of the boy has also improved, and on March 30 he weighed 4½ lb. more than on February 21. The treatment will be repeated every five or six weeks, and it is hoped to show the case again.

Malignant Growth in Neck treated with Radium, with Recurrence in Mediastinum.

By N. S. Finzi, M.B., and William Hill, M.D.

The patient, a man aged 30, noticed a lump in his neck in April, 1909, some alteration in voice in May, 1909, and difficulty in swallowing about a month later, when he was admitted to St. Mary's Hospital. There was then a hard mass at and around the angle of the jaw extending to the tip of the mastoid process behind and above, about 1½ in. in front of the angle of the jaw, and also about 1½ in. below this point. It was firmly fixed to the deep structures; the jaw and the skin and the lower fibres of the facial nerve were involved. It was considered inoperable, and grew rapidly until the end of July, when the radium treatment was commenced.

The growth then had nearly doubled in size, and extended a full inch behind the mastoid process to the place where a scar is seen, this

Date	Pure radium bromide (mg.)	Filters					Total dose (hours)	Parts treated
		Silver (mm.)	Platinum (mm.)	Lead (mm.)	Rubber (mm.)	Lint (layers)		
1909								
July 28 to ,, 30	50	0·5	—	1·5	—	12	55	Neck, three places
Sept. 29	50	0·5	—	1·0	2·0	—	14	Upper œsophagus
,, 30	50	0·5	—	1·0	—	12	29	Neck, three places
Oct. 23 to ,, 24	50	0·5	—	1·0	—	12	35	Neck, three places
Dec. 4–7	50	0·5	—	1·5	—	8	54	Neck, four places
1910								
Feb. 14	50	0·5	—	1·0	—	8	41	Neck, three places
,, 15	17	—	1·0	1·0	—	8	54	
,, 16	50	0·5	—	1·0	2·0	—	13	Middle œsophagus
March 22	50	0·5	—	1·0	2·0	—	13	Lower œsophagus
,, 23	50	0·5	—	1·0	—	8	26	Neck, two places
,, 24	17	—	1·0	1·0	—	4	43	Neck, lower

being caused by a removal of a fragment of the growth for microscopical examination; outlying glands extended down to the clavicle. The pathologist (Dr. Spilsbury) reports that the growth is an endothelial sarcoma. Intralaryngeal examination revealed complete recurrent paralysis of the left cord, with weakness of the right. Œsophagoscopy showed tumefaction of the upper $1\frac{1}{2}$ in. of the œsophagus without ulceration. Bougies were passed through the œsophagoscope in June, and the swallowing improved. Radium treatment was given both internally and externally.

Great improvement was noticed in a few days, which has continued steadily, except when patient omitted to come for ten weeks owing to an attack of bronchitis. Glands in the mediastinum were discovered in January, 1910; the radiogram shown was taken on February 14, 1910. We show the case—

(1) To demonstrate the possibility of dealing with large and deep malignant growths with radium;

(2) To show the necessity of thorough and radical treatment, which has not been sufficient in this case;

(3) To show the ease with which a mediastinal growth can sometimes be seen in an instantaneous radiogram.

A Case of Œsophageal Growth under Treatment by Radium.

By N. S. Finzi, M.B., and William Hill, M.D.

Male, aged 62, with a growth constricting the œsophagus for about $3\frac{1}{4}$ in. in the upper thoracic region, being especially tight in three places. Radium treatment as follows:—

Date	Pure radium bromide (mg.)	Filters			Total dose (hours)	Part treated
		Silver (mm.)	Platinum (mm.)	Rubber (mm.)		
Jan. 22 ...	63	—	0·5	0·5	3	Uppermost stricture
Feb. 9 ...	50	0·5	—	1·0	3	Second stricture
March 12 ...	50	0·5	—	1·0	$2\frac{3}{4}$	Uppermost stricture
April 3 ...	50	0·5	—	1·0	4	Lowest stricture
	50	0·5	—	1·0	2	Second stricture

The stricture, which would not at first admit a small radium tube, and with difficulty allowed a No. 5 bougie to pass, will now easily permit the passage of a large tube down to the third stricture or a No. 9 bougie right through. The growth is stated by Dr. Spilsbury to be endothelial sarcoma. The improvement was demonstrated by means of radiograms. Dr. Hill demonstrated on this patient the method of inserting the radium apparatus into the gullet by means of his new form of œsophagoscope.

DISCUSSION.

Dr. W. HILL added that in the first case there was a large growth, and he regretted he had not taken a cast of the neck, as he had done in a subsequent case. After one application the swelling diminished very materially, and after the second it almost disappeared. Unfortunately, there was a growth in the mediastinum, which probably accounted for the cough. There had not been any dysphagia since the application of radium to the œsophagus. The growth in the œsophagus was asymmetrical, and he did not know whether it was an invasion growth or whether the growth originated in the œsophagus. Possibly it was an inflammatory condition causing pressure. He inclined to the invasion view because of the persistent paralysis of the recurrent laryngeal nerve on the right side. The facial paralysis was definite but not great. All the symptoms had cleared up except those referable to the glands in the mediastinum. By means of X-rays as a guide, the radium-tube could be put in the same plane as the gland. He did not know whether much could be hoped for from the treatment, but Dr. Finzi was sanguine that further benefit would result; this would be shown by cessation of the cough. With regard to the case of œsophageal growth, when the patient was first seen it was very difficult to get a bougie of 2 mm. diameter through the stricture, which was 2 in. long and presented three constrictions. As the result of treatment a bougie with a diameter of 8 mm. could now be passed through. Although it might be stated that some of the improvement was mainly due to the passage of the bougies, recently the passage of those instruments had been stopped, so as to avoid confusion in judging of the effect of the treatment. It was not possible to place the radium quite close to the growth. A small tube was first used, and at the next application it was evident that the stricture was much improved in the upper part, because a large radium-tube could now be passed between the two clavicles. This large radium-tube was screened in $\frac{1}{2}$ mm. of silver, the total diameter being 9 mm. The stricture was in the upper thoracic region, the upper end extending nearly to the commencement of the œsophagus. The patient had put on some flesh, and the dysphagia

was immensely relieved; but when seen he had arrived at the stage when some operative measure, either gastrostomy or intubation by Mr. Symonds' method, had become necessary. He (Dr. Hill) was inclined to adopt the latter course, with the modification of using a rubber tube instead of Mr. Symonds' tube. Dysphagia was nearly always a bad sign.

Dr. FINZI explained that although in the first skiagram there was a very small tube, this did not mean that there was less radium; there was really more, 63 mg. of German radium as against 50 mg. of French radium. At the third application the man had gone back; but the tube had now been passed down to what was regarded as the last of the stricture areas, well below the line of the clavicles.

Rodent Ulcer Treated by Radium.

By CHARTERS J. SYMONDS, M.S.

SINCE this patient was exhibited (December 10, 1909[1]) the radium has been, with a short interval, steadily applied. The improvement has progressed, and since he was last shown the induration on the outer side has almost entirely disappeared; particularly noticeable is the change in the lower eyelid, which has become quite supple. The thickening connected with the lower part of the wound may, it is hoped, be attributed entirely to the cicatrization. A small piece was removed from the margin for microscopic examination. The miscroscopical appearances shown in the piece of skin removed from the margin over the malar bone shows the presence of characteristic cell-formation in the deeper parts. The cutaneous epithelium is intact, and beneath it are collections of deeply-stained rounded cells, in groups or in lines, but there are no cell-processes in this part. There are small masses of epithelial cells undergoing degeneration at various depths and, in the deepest part, can be seen two typical groups of epithelial cells. The section therefore demonstrates that, while the skin has recovered, there is still active disease in the deeper parts.

Dr. Iredell, who has been conducting the treatment in the actino-therapeutic department at Guy's Hospital, supplies the following note: "Two pieces of radium have been used in the treatment of this patient, one consisting of 9 cg. of radium of radio-activity 500,000 spread

[1] *Proceedings*, p. 74.

over a surface 3 cm. square, the other being a ¼ mg. of pure radium in a small glass tube. The former piece, when used for treatment, was covered with a piece of lead 1 mm. thick and wrapped in successive layers of paper lint and guttapercha tissue. The tube was also wrapped up in the same way, but the lead used was about one-fifth of the thickness. This piece was used only in treating the deeper parts of the ulcer. Treatment was begun in October, 1909, and has been continued up to the present, with one month interval. On an average three applications have been given each week, the length of an application being twelve hours.

DISCUSSION.

Dr. IREDELL emphasized the necessity of continuing the treatment for some time after the skin was apparently cured. The skin area on the zygomatic process had apparently been well for some time, but it was clear that in the deeper parts there was some microscopical rodent ulcer left. The same necessity of continuing the treatment occurred in the case of X-rays.

Mr. FEDDEN, in reply to Dr. Finzi, said that the case of malignant disease of the ear at first appeared to be a simple aural polypus; this was removed; but it recurred with very great rapidity, and he then did a complete removal, including the structures in the middle ear. Microscopically it was a fibroma; the fibrous tissue in some places was myxomatous, and contained small lymphatic cysts, suggesting that it was proliferating lymphatic endothelioma. In about a month the boy returned with a much larger growth, which was again projecting from the meatus. Mr. Fedden therefore did a complete mastoid operation, and found that the stalk of the tumour was growing from the deep part of the cartilaginous meatus, not from the middle ear at all. The portion of the meatal wall from which the tumour sprang was freely excised. Microscopically the stalk was composed of fibrous tissue, and in some places the appearance of the growth suggested endothelioma. After that he thought the boy would get well. The mastoid cavity healed up completely, and, everything appearing satisfactory, the boy went to a convalescent home; but a month later he returned with a mass projecting from the meatus, and commencing to fungate through the wound behind the ear. On opening it up he found a soft, breaking-down growth which was adherent to the dura mater, and could not be separated from it. It was freely invading the bone. He then treated the patient with Coley's fluid, and daily injections were given, extending over two and a half months, starting with ½ ♏ and working up to 11 ♏. While under treatment the mass of growth entirely disappeared, and the whole cavity again

became lined with squamous epithelium. Then, about ten days later, while he was still under treatment with Coley's fluid in large doses, a minute nodule was seen in the region where the growth originally started, and the dosage with Coley's fluid still being continued, the nodule increased in size until it was projecting from the meatus. The third report, sent by Dr. Trevor, was that the growth was a sarcoma, with œdema and inflammatory changes. On the whole, it seemed most probable that the growth was a lymphatic endothelioma. The sections had been preserved in case they might be required.

Mr. C. R. C. LYSTER said the cases were of special interest, and they reminded him of one of Mr. Lang's which had been under his care at Middlesex Hospital. It was reported to be a round-celled sarcoma at the back of the orbit. There was considerable protrusion of the eye, and he did not wish to treat the case. He treated it in the ordinary way with radium, and it disappeared so rapidly that, on the basis of past experience, he doubted the diagnosis. He investigated the history, which was that eighteen months or two years ago a section had been removed from the growth, and it was reported to be round-celled sarcoma. Last autumn another section was taken and examined by Dr. Beckton's method of granule staining. Granules appeared in non-malignant cells, and not in malignant cells. The case was now definitely reported to be non-malignant. This fitted in with his experience of radium treatment, which extended over eight years. He had never seen a deep-seated malignant growth beneficially influenced by radium. Superficial rodent cases he had seen influenced by radium, and he recalled cases which for seven years had not recurred. Rodent ulcer of longer standing, especially where bones had been affected, he had seen benefited by radium. Superficial squamous epithelial cancer was, apparently, not so amenable to treatment by radium, but he had seen such cases heal over and apparently get well. He hoped that some day these cases would be found not recurring. Deep cancer, $\frac{3}{4}$ in. from the surface, he had never seen influenced by radium at all. The particular kind of radium to use was as yet a moot point. In 1903 he suggested the use of impure radium, even pitchblende; and it was interesting to note that subcutaneous nodules of cancer treated with pitchblende for some time, up to two or three months, showed fibrous changes.

Dr. DUNBAR said, with regard to the little boy, that three weeks before Dr. Finzi saw him he (Dr. Dunbar) was sent for because he had a high temperature and a large mass behind the ear, which looked like an ordinary abscess, the surface being a scar. Next morning it had discharged an immense amount of foul-smelling pus. The swelling had gone down; a fortnight later it increased in size, and there was a large, bluish, fungating mass growing out of the scar, which had given way. He did not know where that discharge could have originated. Later the fungating mass was seen protruding from the meatus.

Dr. LAZARUS-BARLOW said the case in the orbit mentioned by Mr. Lyster he had diagnosed as a round-celled sarcoma two years ago. He looked at the section again yesterday, and from the histological specimen he again called it round-celled sarcoma. But in the cancer research laboratories during the last two or three years Dr. Beckton had been working out a method which, in the speaker's opinion, served to differentiate, with great accuracy, between malignant and non-malignant growths and tissues generally. A fresh piece of the mass in the eye was put through this method (which consists in staining for Altmann's granules), and granules were present in it in sufficiently large numbers to make it quite certain, as far as could be done at the present time, that his original opinion was wrong. It was a non-malignant growth, or at least non-malignant tissue, perhaps simply hyperplasia. The fundamental question therefore in connexion with the subject under discussion was that of diagnosis. It was a remarkable fact that the cases which had been shown that night, with the exception of Mr. Symonds', were of the indeterminate type of malignant growth, sarcoma or endothelioma—i.e., they belonged to a group which he defied anyone to distinguish with absolute certainty from inflammatory tissue. He would not be surprised, in the case of the boy of 8 years of age, if Dr. Trevor applied Dr. Beckton's method, that he would confess that it was quite possible the condition which he thought was endothelioma was not so at all in the sense of being a malignant new growth. And even if he assumed that the growth was an endothelioma, our knowledge of endothelioma justified the statement that it was not certain that this form of tumour was always malignant; there was a non-malignant form of endothelioma, and one of the greatest difficulties in the whole question was to decide whether a given mass, diagnosed as endothelioma, was malignant in the same sense as spheroidal-celled carcinoma of the breast or periosteal sarcoma, or whether it was in this respect more closely allied to a parotid or dura-mater tumour. He therefore regarded the diagnosis as doubtful in that case. With regard to the man aged 30 with the mass in the neck, he was again in difficulties. But it was regarded as endothelioma, and the portion of growth which had disappeared under radium was close under the surface. On the other hand, many mediastinal growths were disappearing. It brought one back to such cases as Mr. Charters Symonds'. It was known that X-rays and radium could produce an effect on rodent ulcer, a superficial growth; but that growth persisted in the deep parts even of rodent ulcers, and he thought that nothing more than this had occurred. In reference to the œsophageal case, he believed that physicians would agree that it was not unusual, even apart from the use of bougies, for cases of œsophageal carcinoma to allow larger masses of food to pass later in the course of the disease than at earlier periods. He would like to see this patient of Dr. Hill's in eighteen months' time. It must be remembered that radium treatment was

being pushed, but X-rays could cause cancer of the hand, and so little was yet known on the subject that a word of caution was not out of place. He knew, from laboratory work, that X-rays and radium could cause accelerated development of cells as well as retardation. It would be unfortunate if radium, like X-rays, also caused carcinoma.

Mr. EVE said he could bear out what Dr. Lazarus-Barlow said as to the susceptibility of sarcoma in the lymphatic glands to radium and X-rays. He had a case under observation in which the nature of the growth was verified by microscopical examination. It was an inoperable tumour in the neck deeply seated, and had almost disappeared under exposure to X-rays. In another case a small recurrent epithelioma of the roof of the mouth had disappeared under radium, and the result twelve months subsequently was permanent.

Dr. W. P. HERRINGHAM said he could cite a case of lymphosarcoma of the glands of the neck and mediastinum in which the cervical tumours disappeared under X-rays and the patient lost his symptoms (dyspnœa). He was just about to be discharged when the tumours recurred, in spite of the continuance of the treatment, and the patient died.

Dr. W. HILL, in reply, said that he had applied radium to the œsophagus in ten cases, and of these, seven had had several applications, the other three not having been deemed suitable. In seven, the dysphagia had enormously improved, and in none of them had gastrostomy been necessary. In several cases he had satisfied himself that the good effect was not due to the passage of a bougie. Mr. Godlee had used radium for cancer behind the cricoid, where the growth was a squamous-celled carcinoma, and he believed Mr. Godlee used one-fourth of the usual amount of radium, or of that used at Guy's. But the activity of the Guy's radium was only a fourth the activity of that used by Dr. Finzi. In all the observations it was desirable for the dosage or strength to be stated. He had not seen it do good in squamous-celled carcinoma. Cancer in the œsophagus killed, if left alone. He did not expect to show the patient alive in eighteen months. He did not believe radium would cure an advanced case of malignant disease of the œsophagus, but some of the cases had obtained relief from dysphagia before they died.

Dr. FINZI, in reply, said he brought forward the man with a recurrence in the mediastinum to show that it was possible to influence a growth which had extended to a depth of fully 3 in. from the surface. With regard to curing growths of doubtful nature, the very fact of recurrence was evidence in favour of malignancy. He cited three cases from his own practice. In a case of epithelioma of the lip a section was obtained; it was removed by a surgeon, and it recurred in the glands. Those glands were removed, and further recurrence took place. The recurrence was partially removed, and what was left was treated by radium and disappeared. In a case of carcinoma of the

uterus, confirmed microscopically, in which recurrence occurred at the top of the vagina, the recurrence had disappeared after treatment by radium. There was a case of a small local growth of carcinoma of the rectum, and that had also disappeared after radium treatment. He could not say if recurrence would occur in these cases, but the patients were at all events getting an extra lease of life.

Disappearance of Chronic Ascites in Alcoholic Cirrhosis after repeated Paracentesis and Laparotomy without Omentopexy.

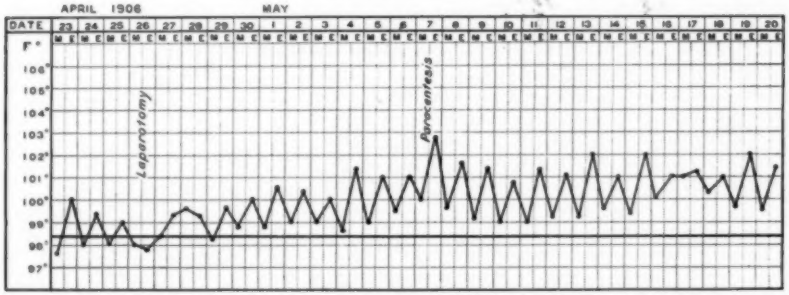
By F. Parkes Weber, M.D., and E. Michels, M.D.

The patient, R. A. P., a London barman, aged 44, was admitted on the medical side of the German Hospital in April, 1906, for chronic ascites due to alcoholic cirrhosis of the liver. He had been accustomed to drink twelve to twenty glasses of beer a day; seldom spirits. No history of venereal diseases, except gonorrhœa many years ago. He had usually enjoyed good health until the present illness commenced six to nine months before admission. First his belly and then his legs began to swell. His belly was tapped three times before admission. In the hospital the patient's state of general nutrition was medium. There was some dullness at the bases of the lungs; the heart appeared healthy; the urine was of rather high specific gravity and diminished quantity, but free from albumin and sugar. The temperature varied between 98° F. and 100° F. Paracentesis abdominis was performed on April 6 and April 20, about 12 litres of ascitic fluid (specific gravity 1011 to 1013) being removed on each occasion. After the tapping the liver could be felt, rather hard, and extending three finger-breadths below the ribs. The spleen could be felt at the costal margin.

The patient was then transferred to the surgical side of the hospital for omentopexy, but when Dr. E. Michels opened the abdomen on April 26 (9 litres of ascitic fluid evacuated, of specific gravity 1012), the omentum was found quite atrophic and could not be fastened to the abdominal wall; an attempt to fasten the spleen to the abdominal wall also failed, as it could not be brought down sufficiently. After this the temperature became somewhat irregular, and in May often reached 101° F. to 102° F. in the evenings (*see* temperature chart, p. 168). The peritoneum filled up again, and was tapped for the last time on May 7,

7,400 c.cm. of a slightly bloody ascitic fluid (specific gravity 1006) being removed. On May 24 there was practically no ascites, and the diuresis was very satisfactory. From May 30 the temperature usually remained below 100° F., and the patient left the hospital on July 25, 1906. In February, 1907, Dr. Michels had to operate for a traumatic hernia in the laparotomy scar below the umbilicus and evacuated a peritoneal "pseudo-cyst" containing bloody fluid (encapsuled remains of the ascites).

At present (February, 1910) the patient looks better nourished than he did in 1906, four years ago. He still takes a good deal of alcohol and has decided tremor of the hands. He has no ascites, but his liver reaches down to the level of the umbilicus and his spleen can be distinctly felt at the costal margin. His urine, of rather high colour, is



free from albumin, sugar and bilirubin, but contains excess of urobilin and urobilinogen.

In all probability, judging from some other cases, the moderate pyrexia during May, 1906, was of favourable significance in regard to disappearance of the ascites.

Case of Cyanosis with Venous Obstruction.

By W. P. Herringham, M.D.

H. C., aged 24, labourer. History of five years' cyanosis of hands and feet after "rheumatism"—viz., pains in knees, ankles, and feet—which laid him up twelve weeks. Sixteen months ago an abscess broke

in right supraclavicular fossa, and discharged for several weeks. Since then never so well as before; cyanosis of ears, unable to stoop, a choking sensation at base of neck, and shortness of breath. Four months ago ulcers came on the legs without injury. Denies syphilis and gonorrhœa; Wassermann's reaction on blood negative.

Present condition: Cyanosis of face, ears, forearms and hands, legs and feet; dilated veins over upper part of chest; impaired resonance over manubrium sterni, reaching 3 in. to right of mid-line and 2 in. to left of it; auscultation, *nil;* X-rays, a rather wide median shadow, not pulsatile. Heart natural in size, no murmurs; pulse regular. Lungs: impairment in front as above, breath sounds weak over same area; behind impairment (P − 1) all down left side, breath sounds natural. No clubbing of fingers. Hard cords in line of saphena veins. Enlarged glands in neck, axillæ, and groins. Blood-count: reds, 5,500,000; whites, 6,800 (lymphocytes, 36 per cent.). Old tuberculin, $\frac{1}{100}$ c.c., subcutaneously gave a marked reaction.

Dr. PARKES WEBER said that, in spite of the heart being not obviously enlarged, he had come to the conclusion that the case was probably one of tuberculous glands in the mediastinum, with chronic tuberculous pericarditis. There were, he believed, cases on record in which such a condition was known to produce similar symptoms.

Formation of Cholesterol Gall-stones containing Typhoid Bacilli within Sixty-eight Days of the Onset of Typhoid Fever.

By ARTHUR F. HERTZ, M.D., and F. S. ADAMS.

M. H., AGED 24, has always suffered from constipation, her bowels never being opened more than twice a week, and then only with the aid of purgatives. Since the birth of her only child, three years ago, she has occasionally suffered from a dull, aching pain in the epigastrium; it begins immediately after food has been taken, and is occasionally relieved by vomiting, but it has never been sufficiently troublesome to cause her to take advice. She has never previously had typhoid fever.

On December 22, 1909, she was admitted into the Brook Fever Hospital, with typhoid fever, on the twelfth day of the disease. Her temperature fell to normal on January 2. From January 14 to January 24 she had a relapse, in which the rash returned.

On February 1 she complained for the first time of pain in the epigastrium and back, and she vomited. The gall-bladder was found to be enlarged and slightly tender. There was no jaundice or pyrexia. The pain persisted, and attacks of bilious vomiting occurred from time to time. The bowels were opened by aperients and enemata. On February 18 the temperature rose to 99·4° F., and the patient was sent to Guy's Hospital.

On admission her temperature was 99·8° F., and her pulse-rate 120. She complained of pain in the region of the gall-bladder, from which it passed round the right side to the back; she said that it was constantly present, but occasionally became much worse. The abdomen was not distended: the upper part of the right rectus muscle was very rigid, but the enlarged and tender gall-bladder could be felt. There was no tenderness elsewhere. The urine was normal. The heart-sounds at the right base were feeble, and the respiratory movements of the right side of the chest were difficult. Suppurative cholecystitis was diagnosed. Mr. Philip Turner operated that same evening (February 18). There was no free fluid in the peritoneal cavity, and no peritonitis, except in the immediate neighbourhood of the gall-bladder, which was distended, tense, and adherent to the surrounding structures. The gall-bladder was opened, and, after about 4 oz. of colourless, clear fluid had escaped, about ½ oz. of pus was evacuated. About twenty-five faceted gall-stones were then removed from the gall-bladder. They varied in size from ⅛ in. to ¼ in., and consisted of slightly pigmented cholesterol; they were of the usual hardness. The clear fluid and the pus both contained a pure culture of *Bacillus typhosus*. The same organism was isolated from the centre of some of the gall-stones. The gall-bladder was drained, and the patient made an uneventful recovery.

As this was the patient's first attack of typhoid fever, and the *Bacillus typhosus* was found in the centre of the gall-stones, it is clear that they must have been formed between the onset of the typhoid fever on December 12 and the operation on February 18, a period of sixty-eight days. The first symptoms of cholecystitis coincided with the discovery of the enlarged gall-bladder on February 1, but the infection of the gall-bladder must of course have occurred at an earlier date. It

is impossible to say when the cholecystitis began, but the formation of the gall-stones must have taken less than sixty-eight days and probably not more than fifty days.

Numerous cases have been recorded of gall-stones following typhoid cholecystitis and of the *Bacillus typhosus* being found within them. In six cases[1] only were the stones found at periods of less than six months (five months, eight weeks, five weeks, five weeks, four weeks, and four weeks respectively) after the onset of the typhoid fever. In only two of these were typhoid bacilli found in the bile or pus contained in the gall-bladder, and in no case were they isolated from the stones. Moreover, nothing definite is said about the size of the stones in any of these cases, though in three they are described as small. Our case is thus remarkable as giving, for the first time, almost indisputable evidence of the rate at which gall-stones may form.

Mr. GARNETT WRIGHT said he had recently had a somewhat similar case, in a girl aged 17, who, on March 12, 1909, had a typical attack of typhoid fever, which ran its course in a month, and had an attack of biliary colic followed by jaundice on April 25 in the same year. She had a similar attack of colic on May 12, and several slighter attacks in the next few days. On May 15 an operation was performed; the gall-bladder was inflamed and contained about a dozen small, round, mulberry-shaped stones. Typhoid bacilli were cultivated from the gall-bladder, but not from the stones. He thought it probable that the formation of the stones followed that particular attack of typhoid fever.

Pulsating Exophthalmos.

By GWYNNE WILLIAMS, F.R.C.S.

MALE, aged 29. Patient has had dilated veins over the left eye ever since he was a baby. When aged 9 he had a fall on to his head and suffered from concussion. The left eye has always been "weak," but it has only become prominent during the last 11 months. On two

[1] Budd: "Diseases of the Liver," 3rd ed., 1857. J. Douglas: *Med. Record*, New York, 1909, lxxv, p. 536. Gilbert et Girode: *Compt. rend. de la Soc. de Biol.*, Par., 1893, ser. 9, v, p. 958. C. E. E. Hoffmann: *Virchows Archiv*, 1868, xlii, p. 219. P. L. Legendre: *Bull. de la Soc. anat. de Paris*, 4e sér., 1881, vi, p. 193. Ranvier: *Ibid.*, 2e sér., 1863, viii, p. 432.

occasions he has had subconjunctival hæmorrhages, at the ages of 18 and 25 respectively.

The left eye is pushed forwards and outwards, the conjunctiva is injected, and towards the inner canthus there projects from between the lids a piece of chemosed conjunctiva. General limitation of movement. No diplopia. Vision $\frac{6}{18}$; J. 4. Disk: Edges a little blurred, no pulsation in the veins. Projecting from the upper and inner angle of the orbit is a soft pulsating swelling which is continuous with a mass of thickened and dilated veins on the forehead. On auscultation of this swelling a whistling murmur can be heard; it is systolic in time, but its duration varies with the pressure of the stethoscope. The eyeball also pulsates, especially noticeable when slight pressure is put upon it. Compression of the left common carotid diminishes the exophthalmos, and stops the pulsation.

Three Rare Tumours of the Base of the Tongue (Two of them Endotheliomata), with Remarks on the Surgery of Malignant Disease of the Tongue.

By Frederic Eve, F.R.C.S.

The rarity of endothelioma of the tongue, the uncertain position of these tumours in relation to certain forms of epithelioma, and our almost complete ignorance as regards their course may justify the publication of these cases.

Case I.

Mrs. T. S., aged 61, was seen by me in July, 1902. She had noticed discomfort in the throat for six months, but had no pain or dysphagia. There was a prominent growth on the base of the tongue towards the left side, and some enlarged glands could be felt behind the angle of the jaw. I operated with Dr. Jekyll, of Leytonstone, shortly afterwards. A preliminary tracheotomy was performed. The external carotid on the same side as the disease was first tied and the enlarged glands removed. The submaxillary triangle was also cleared. The floor of the mouth was opened from below, the tongue brought out, and the right half completely removed. The patient made a good recovery and is now, at the date of writing, nearly eight years after the operation, quite well. The specimen, preserved in the Royal College of Surgeons' Museum (2273L), showed a smooth, well-defined tumour springing from the extreme base of the tongue on the left side (fig. 1). It rose abruptly from the surface, its anterior edge projecting ¾ in. at its most prominent part; its diameter was about 1¼ in. The surface was not ulcerated, and was covered with smooth mucous membrane. The tumour extended outwards to the pillars of the fauces just below the tonsil, inwards nearly to the middle line, and backwards almost as far as the epiglottis. A section showed a smooth, somewhat gland-like appearance.

Microscopical appearances: The epithelium over the surface of the growth was intact (fig. 2), and had no obvious relation with the tumour itself. Beneath the epithelium was the normal adenoid tissue and some channels, evidently dilated lymphatic vessels; a few contained lymph coagula, mostly granular. The growth itself lay deeper down. It was

composed of large, irregular branching and anastomosing columns and masses of cells (fig. 3), and of smaller irregular groups lying in the connective-tissue interspaces. The general appearance of the growth in its outlying parts was that of a carcinoma of the breast. In places the cell-masses showed a definite relation to the channels, presumably lymphatic; they formed bands or cords around them, and occasionally



FIG. 1.

Endothelioma of the tongue.

a rounded bud of cells projected outwards from their walls. Strands of cells were also observed in lymphatic channels. Towards the centre of a few cell-masses a well-defined central cavity had been formed by degeneration. Some cells showed colloid metamorphosis, which led to the formation of small rounded spaces. The cells of the tumour were remarkably uniform; they contained a well-stained round or oval nucleus



FIG. 2.

The intact epithelium covers the growth, which consists of columns of rounded cells with well-stained rounded nuclei. A cavity, formed by degeneration, is seen at the centre of some of the columns. (Low power.)



FIG. 3.

The figure shows irregular, branching and anastomosing columns of cells.

with fairly abundant protoplasm, the outline of which was nowhere visible; at no part could a space be seen between individual cells. There were no cell-nests; and no keratinization was observable.

The growth was an endothelioma associated with dilatation and possibly overgrowth of lymphatics (lymph-endothelioma). The lymphatic glands were not examined with the microscope.

Case II.

Mrs. H., aged 62. She had complained of discomfort and a feeling as of a foreign body in the pharynx for two months, but had no pain. On examination there was a prominent disk-shaped growth occupying



Fig. 4.
Endothelioma.

the left half of the extreme base of the tongue. Some enlarged glands could be felt in the superior carotid triangle on the same side.

July 10, 1909: I operated with Dr. Le Quesne and Dr. Anderson, of Tring. A preliminary laryngotomy was performed. The whole tongue was then removed by Syme's, or so-called Kocher's, operation, after dividing the mandible in the middle line. Pains were taken to divide the hyo-glossi and genio-hyo-glossi close to the hyoid bone; and especially to divide the base of the tongue as far back as possible.

A fortnight later the glands and fat, together with the internal jugular vein, were removed from the whole of the left side of the neck, after dividing the sterno-mastoid muscle. Some of the glands behind the angle of the jaw were $\frac{1}{2}$ in. to $\frac{5}{8}$ in. in diameter, but were not adherent to the surrounding structures.

Convalescence was uneventful, and when I examined her on February 5, 1910, the patient was quite well and free from recurrence. The specimen, now in the Royal College of Surgeons Museum, showed a disk-shaped tumour with a well-defined prominent edge and a roughened surface covered with fleshy papillæ, but not ulcerated (fig. 4).

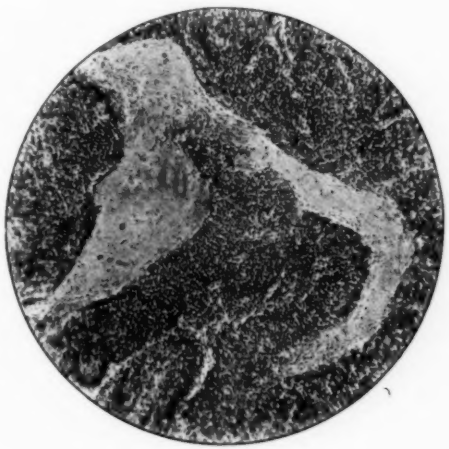


FIG. 5.

The figure shows a blood-vessel surrounded by a cord-like mass of endothelial cells. (Low power.)

The growth extended from just behind the circumvallate papillæ backwards to the glosso-epiglottidean folds, inwards to the middle line, and outward nearly as far as the lateral edge of the tongue. The section of the growth was pale and homogeneous; its base was ill-defined and evidently infiltrated the substance of the tongue.

In this tumour also the epithelium formed a thin even layer over the growth, which extended right up to it. The striking feature of the growth was the presence, especially towards its surface, of numerous large blood-vessels filled with altered blood. Around these the cells formed

fairly well-defined columns or cords (fig. 5), separated by thin trabeculæ of connective tissue. In other parts of the tumour the growth occurred in strands or masses, and between these were rounded clumps of adenoid tissue. In the deeper part of the sections a few cell-nests were present near the edge of the masses, but they showed no keratinization; the cells consisted of round or oval well-defined and deeply-stained nuclei of an endothelial type (fig. 6). The growth may be classed as a perithelioma. The lymphatic glands were definitely infiltrated, and their microscopic appearances closely resembled the primary tumour. There were many large blood-vessels surrounded by cords and columns of cells having

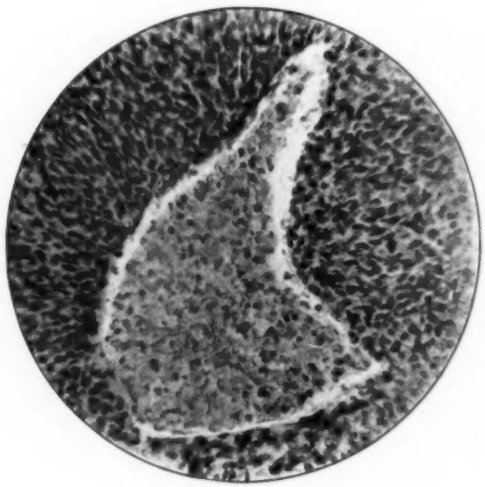


FIG. 6.

Shows a blood-vessel surrounded by very uniform cells with large nuclei. (Higher power.)

well-defined wavy margins; these bands also surrounded the connective-tissue trabeculæ, which were thickened. At one spot were whorls of cells or cell-nests, similar to those which may be seen in an endothelioma of the brain.

Remarks.

These two cases of endothelioma presented many features in common. In each the growth occupied the extreme base of the tongue, and formed

a very prominent disk-shaped mass which was not ulcerated on the surface; glands were distinctly enlarged in the superior carotid triangle in both. The symptoms had been discomfort or a feeling as of a foreign body in the fauces, and were so slight that the disease attained considerable size before it was discovered. The microscopic character of the tumour in the first case was of a lymph-endotheliomatous type, the growth consisting of cord-like masses of endothelial cells in places surrounding dilated lymphatic channels. In the second case it was peritheliomatous. There were very numerous and large vascular channels surrounded by cords of endothelial cells.

Notwithstanding the early and marked enlargement of glands, the fact that the first patient has survived for nearly eight years leads one to hope that free and early removal of this disease may be followed by satisfactory results. But it should be pointed out that the outline of this tumour was unusually well defined. I may say that the endotheliomatous nature of this tumour was apparent to me when I removed it in 1902, and I sent it to the Royal College of Surgeons for preservation.

The occurrence of endothelioma in the base of the tongue can be readily understood. This portion of the organ is made up of a considerable amount of adenoid and lymphatic tissue, and has been called the lingual tonsil; it also contains many mucous glands, and in this respect resembles the salivary glands, in which endothelioma is the commonest form of growth.

In the "Archives of Middlesex Hospital," iii, 1904, Dr. Lazarus-Barlow has reported four cases described as examples of endothelioma of the tongue. They were met with in 105 cases of malignant disease of the tongue, between the years 1900 and 1903, both inclusive. All the patients were males, their ages being respectively 42, 44, 51, and 56. In two instances the disease occurred at the base of the tongue and laterally, as in my own cases; and in the other two the growth was close to the tip. Deep erosions of the superficial portions of the growth characterized all the cases. In this respect, therefore, Dr. Lazarus-Barlow's cases differed materially from those described above. In every instance there was enlargement of the inframaxillary glands. The disease ran a rapid course in three cases; and the second case was lost sight of. The first case lasted one year from commencement to death, the third case two years, and the fourth case fourteen months. At the post-mortem examination in Case I, an extensive growth was found in the mandible together with growths in the submaxillary glands and liver; in Case III

there were nodules in the submaxillary glands and lungs; and in Case IV growths in the submaxillary glands and liver. In two cases the growth was of a lymphangiomatous type, and in the other two it was peritheliomatous.

Case III—Epithelioma without Keratinization.

Mr. B., aged 52. He had noticed soreness and a swelling at the base of the tongue three months before he consulted me on July 21, 1909. There was a prominent growth projecting from the extreme base of the tongue on the right side (fig. 7). Its surface was slightly papillary, but not ulcerated. Its edges, abruptly raised $\frac{1}{4}$ in. to $\frac{3}{8}$ in. above the base of the tongue, were overhanging. The growth sprang from the outer two-thirds of the extreme base and extended outwards as far as the tonsil, and backwards to the glosso-epiglottidean folds. In fact, the extreme posterior edge passed downwards along the pharynx to a level behind the anterior surface of the epiglottis.

July 24, 1909: Operation with Mr. Mark Hovell. The tongue was removed by Syme's method after a preliminary laryngotomy. I also removed the pillars of the fauces together with the tonsil; and although posteriorly I got behind the limit of the disease, I did not feel that I had been able to remove a sufficient margin of healthy tissue. On August 9 the whole of the right side of the neck was cleared of glands and fat, the internal jugular vein being tied below and stripped upwards with the glands. Many of the glands in the superior carotid triangle were very large, but were not adherent. The sterno-mastoid muscle was divided.

January 26, 1910 (six months later): I examined the patient and found a small raised non-ulcerating growth on the scar in front and to the right of the epiglottis, and extending to the antero-lateral wall of the pharynx. The margin of the right half of the epiglottis itself was also distinctly thickened. After consultation with Mr. Butlin and Mr. Hovell it was decided to remove the larynx, together with the recurrent growth. I performed this operation on February 10. The incisions were confined to the middle line and left side of the neck, as the skin on the right side of the neck was a good deal thinned by scarring, and by the free removal of tissue at the previous operation. The left external carotid was first tied; then the trachea was divided below the cricoid (without a preliminary tracheotomy), and the larynx was separated by dissection from below upwards. The next step was to divide the hyoid bone in the

middle line, turn it upwards and outwards with its attached muscles, and then open the pharynx freely. The recurrent growth was widely removed in one piece with the larynx. A nasal tube was kept in for about ten days. The patient left for home quite well three weeks after the operation.



FIG. 7.
Epithelioma of the tongue.

The primary growth in the tongue was made up of anastomosing columns and strands of cells (fig. 8) supported by narrow connective-tissue trabeculæ. The cells contained large nuclei, and the outline of their protoplasm was in parts well-defined. Several whorls of cells but no prickle-cells were observed; many of the cells were vesicular, but there

was no keratinization. The glands were not examined. At first I thought that this growth was also an endothelioma, but the well-defined outline of the protoplasm of the cells, and the fact that many of them were vesicular, led me to have fresh sections prepared, which included the surface of the growth. It then became apparent that the columns of cells extended down from the surface in the intervals between large papillæ; the epithelial covering of these had, however, broken away in the process of cutting the sections. The tumour was therefore an epithelioma springing from the cells of the Malpighian layer of which component cells remained undifferentiated. It can readily be understood



FIG. 8.

The figure shows anastomosing masses of cells with irregular outlines. There are whorls of cells, but no keratinization is seen.

why this form of growth occurs at the base of the tongue where the epithelium is not keratinized, and is practically similar to that of a mucous membrane.

This case is included in the paper for various reasons. It appears to me a rare form of malignant disease of the tongue, and, unless careful discrimination is made, might readily be mistaken for an endothelioma. Further, its position at the base of the tongue, its abrupt steep margins, and the absence of ulceration render its naked-eye characters very similar to the cases of endothelioma I have described above.

In Butlin and Spencer's excellent work on "Diseases of the Tongue" (1901, p. 313) an apparently similar case is quoted and figured (fig. 29). It occurred in the practice of Mr. Spencer. The patient, a girl aged 22, had observed an ulcer opposite the left lower molar teeth for two months. In two and a half months from its first appearance the growth had involved half the tongue throughout its whole length, the floor of the mouth, the surface of the tonsil, and the adjacent portion of the soft palate. In spite of early operation, she died within ten months of the commencement of the disease. The growth consisted of masses and columns of cells infiltrating the muscular fibre, without forming cell-nests. I may say that I have to thank Mr. Shattock for very kindly studying the microscopic sections from these cases with me; and he concurs in my description of the appearances.

Remarks.

A few remarks may be made on the surgical aspects of the cases. In the first case half the tongue was removed through a lateral incision in the neck after ligaturing the external carotid and removing the lymphatic glands. I had not, at that period (1902), adopted Syme's, or Kocher's, operation of dividing the mandible in the middle line. The obvious disadvantage of removal of the tongue from below is that it leaves a large wound in the neck which must necessarily become septic, as it communicates with the mouth. And when the whole tongue requires removal I have not found it easy to expose the opposite base.

In the second and third cases the tongue was excised by severing the mandible, and the glands were removed at a second operation. This I consider by far the best method of removing any growth in the tongue of considerable size, especially if it extends as far back as the anterior pillars of the fauces; and still more so if it involves the floor of the mouth. For, contrary to a high authority, I do not consider that any growth implicating the floor of the mouth can be satisfactorily removed without dividing the mandible. The great advantages of Syme's operation are: That the lingual arteries are easily secured far back, after division of the hyo-glossi; the tissues of the floor of the mouth can be cleanly detached from the mandible; and the genio-hyo-glossi, hyo-glossi, &c., together with the tongue itself, can be severed at their attachments to the hyoid bone. Whatever may have been written to the contrary, I consider that, exclusive of quite small growths, the muscles of the tongue should be divided near their several origins, due

regard being paid to the situation of the growth and the direction of its extension. When the growth is ulcerated, and especially if foul, it seems to me important that the tongue should be excised first. For it appears likely that a wound in the neck may be infected with septic matter from the lymphatics of the tongue; and it is possible that the wound may also be inoculated with carcinoma. In any case the patient's general condition is improved by the removal of a septic mass from the mouth.

The disadvantages of Syme's procedure are in connexion with the union of the two halves of the mandible. On four occasions I have had to move the wire which united them, owing to the existence of a sinus; and such a sinus may necessitate inconvenient delay in the performance of the second operation on the neck. Delayed union has also occurred in my practice owing to the absorption of bone around the wires, and loosening of the fragments. This may also lead to irregularity of the teeth. These disadvantages can be entirely avoided by having a dental splint prepared beforehand, which can be fitted at the end of the operation.

The third case related is an example of a growth extending very far back along the anterior wall of the pharynx, to a level posterior to the anterior surface of the epiglottis. It also passed upwards on the lateral aspect of the fauces and involved the tonsil. I removed it by Syme's method, but found great difficulty in getting sufficiently far behind it, and recurrence occurred in the pharynx and epiglottis after eight months. In this case I think it would have been preferable to perform the operation on the affected side of the neck first, removing the glands and tying the branches of the external carotid by Dawbarn's method. Later, the whole tongue might have been removed by incisions limited to the sound side of the neck, after a method recently published by Mr. Butlin[1] for such cases. This procedure was adopted for the removal of the recurrent growth, but since the epiglottis was involved it was necessary to combine with it laryngectomy. An illustration is thus afforded of the usefulness of this method for extensive growths implicating the base of the tongue, fauces and pharynx, because if it be found after opening the pharynx that the larynx is affected, there is no difficulty in modifying the operation originally planned so as to include a laryngectomy with excision of the tongue. A partial removal of the larynx is probably useless. Butlin has found that if the disease is attacked from the affected side of the neck (viz., that on which the anterior triangle was

[1] "Operative Surgery," edited by F. F. Burghard, 1909, ii, p. 224.

cleared), a hole communicating with the pharynx may be left which retards the patient's recovery for many weeks.[1]

Regarding the removal of the glands of the neck, it is scarcely necessary to say that not only the glands but the fat and loose connective tissue should be removed from the affected areas. For some years, in this and similar operations I have stripped up the fat and connective tissue from the surface of the muscles with gauze pads. The method is expeditious, makes a clean dissection, and is attended with little hæmorrhage.

It is very often necessary to clear the *whole* side of the neck, for it may be found that enlarged glands extend under the sterno-mastoid into the posterior triangle, especially in its upper half. The usual incision is made from the mastoid process to the clavicle, but instead of carrying it along the anterior border of the muscle I carry it over its middle, so that the whole width of the muscle and the surface behind it can be readily exposed. A second incision is made from the centre of the vertical one to the chin, and the flaps outlined are turned upwards and downwards respectively.

In most cases I divide the sterno-mastoid just below the exit of the spinal accessory nerve so that the halves of the muscle can be turned downwards and upwards without hindrance. The internal jugular is tied low down, divided, and the connective tissue cut through just above the clavicle. The internal jugular, glands, and fat are then dissected up by gauze stripping, a look-out being kept for the thoracic duct on the left side. I have found it convenient to clear the submaxillary triangle from above. After ligaturing the facial artery and vein, where they lie on the jaw, a horizontal incision is made through the connective tissue just below the horizontal ramus. The lymphatic glands, fat, and submaxillary salivary gland are then stripped downwards and backwards. Wharton's duct is tied before division, as I think it may infect the wound from the mouth. After dissecting the contents of the submaxillary triangle downwards and backwards with gauze and scissors, only the facial artery at its origin and the common facial vein remain to be tied. Finally, the dissection of the internal jugular upwards to the base of the skull is completed, and the proximal end of the vein is secured. In those cases in which glands in the posterior triangle are extensively involved, I make an incision, from the centre of the first vertical incision, backwards and obliquely upwards

[1] Op. cit., p. 225.

towards the spine of the axis and expose the posterior triangle by reflecting the flaps marked out by it.

I am in agreement with those who think that, as a rule, it is only necessary to remove the glands on the affected side, unless those on the opposite side are actually enlarged. The exceptions to this rule are, however, numerous. They include all growths in the tongue originating in or approaching the middle line, those deeply infiltrating the under surface of the tongue, those arising in the tip of the tongue, or implicating the floor of the mouth at or near the frænum.

DISCUSSION.

Dr. LAZARUS-BARLOW congratulated Mr. Eve on two things: first, on having got hold of the first two female cases he (the speaker) had ever heard of; secondly, on finding, as a surgeon, that the pathologists agreed with him in the diagnosis. They were two of the most remarkable cases he had ever seen. He defied anyone who did not recognize that they were endotheliomata to say they were not cases of squamous-carcinoma from the microscopical appearances on the screen. To those who had given special attention to endotheliomata they were quite clear. The first case was lymphatic endothelioma, the second hæmoperithelioma; the third case, differing from Mr. Eve, he should also call an endothelioma if he found an absence of prickles between the cells. The real diagnosis of the third case lay between endothelioma and a pure Malpighian squamous-celled carcinoma. He believed that in every squamous-celled carcinoma, even those consisting of basal cells alone, those prickles would be found in a good preparation. That was, in his opinion, the test whether it was squamous carcinoma or endothelioma. As one whose work caused him to see much cancer, he would like to urge surgeons, particularly in cases of carcinoma of the tongue, to operate even more widely than they dared at present. Of late years better results were obtained in carcinoma of the cervix, simply because nowadays the whole uterus was removed with the adnexa and a portion of the vagina in the extensive Wertheim's operation. He believed that the statistics of cancer of the tongue would not be much improved until surgeons applied the same principle in this region.

Mr. W. G. SPENCER congratulated Mr. Eve on showing the cases, especially from the clinical point of view, because the first two followed closely on the original type of endothelioma which had occurred commonly in the parotid, and also in the submaxillary salivary glands. He had had a case similar to Mr. Eve's at the base of the tongue. The prognosis was favourable—very different from that in the third case. Dr. Lazarus-Barlow had freely used the term "endothelioma," and he (the speaker) had looked again at Dr. Lazarus-Barlow's account of four cases of endothelioma of the tongue, and he thought

they were identical with the third case of Mr. Eve's and were epitheliomas, in which there was very little keratinization and scarcely any nest-cells. The clinical history of the four cases showed that they were rapidly malignant, infecting the glands. Dr. Lazarus-Barlow, in a previous debate, spoke of endotheliomas as being innocent tumours in some places. Mr. Eve referred to the operation which Professor Kocher had again revived, of dividing the jaw. It was really Sédillot's operation. Syme repeated the operation twice, thirteen or fourteen years later, but was unsuccessful in both. With regard to the union of the jaw, he had tried that, but there seemed no certainty about its union, however carefully one wired it, or what one did with the splint. It was well, if possible, to get out the portion of the tongue under the jaw without dividing the jaw. In a very malignant case it was necessary to remove the mylo-hyoid muscle, but the floor of the mouth should be preserved as much as possible, as a great difference was made in regard to the recovery of the patient. Where that muscle was preserved, in six months the patient got back speech and facility in swallowing saliva, which were both very great boons. He had always removed the glands at the second operation; if done at the first operation he thought the second operation would generally upset the neck. He thought Mr. Eve spoke somewhat lightly of extending the operation deeply into the root of the neck. But in that region there was the junction of the jugulars with the subclavian veins, and he thought the risk of the operation was greatly increased thereby. There was much less anxiety in a case in which it was only necessary to remove the submaxillary set of glands, and particularly those underneath the sterno-mastoid muscle close to the mastoid process. It was in that situation he saw so many of the cases recur in days gone by.

Mr. CARLESS said he was glad to hear Mr. Spencer raise the question of the conservation of part of the tongue, if practicable, in growths involving only the posterior aspect. Some years ago he operated on a case of epithelioma of the base of the tongue, probably originating in the epiglottis, and subsequently showed the patient at the Medical Society of London. He performed a trans-hyoid pharyngotomy through an incision reaching from the symphysis menti to the thyroid cartilage, dividing the hyoid bone and opening the pharynx through the base of the epiglottis. He took away the epiglottis and a V-shaped wedge of the substance of the tongue, including the whole of its posterior part, but leaving the anterior. The vessels and nerves coming in from either side were left untouched. A preliminary tracheotomy was of course required. After healing was completed the patient, instead of having arytæno-epiglottidean folds to protect the larynx, had glosso-arytænoid folds, which served a similar purpose and enabled him to swallow fluids comfortably without anything going into the larynx. The case was exhibited two years after operation, and there was then no sign of local recurrence, although the growth was a well-marked epithelioma. Later he had recurrence in the cervical glands, and unfortunately left it too late to permit of their removal. Amongst the many points raised by Mr. Eve's paper he could only touch on one or two. As to whether the tongue

or the glands should be operated upon first, he thought that in the great majority of cases there could be no question that the tongue should be dealt with first and the glands subsequently. He agreed with Mr. Spencer's warning as to the danger of unnecessarily opening up the root of the neck. There was no question about the necessity of wide removal of glands, and in their limits the wider the better; but one ought to encroach with the greatest care upon regions which were dangerous, and where complications, even in skilled hands, might arise.

Mr. EVE, in reply, thanked Dr. Lazarus-Barlow for lending the weight of his authority to the verification of the pathological diagnosis, but he still thought that the third case was an example of epithelioma originating in the Malpighian layer of the epithelium, and in which the cells remained undifferentiated; there was no keratinization owing to the locality involved. In the lingual tonsil the epithelium was of the nature of mucous membrane, and there was not the same tendency to keratinization as elsewhere on the tongue. He was interested in Mr. Spencer's remarks; in regard to extending the incision low down in the neck, he found it necessary to do so by force of circumstances in many cases. Enlarged glands were so often found extending downwards along the internal jugular. He agreed with Mr. Spencer as to the importance of removing glands beneath the upper half of the sterno-mastoid, inferior to the mastoid process; it was for that reason he divided the sterno-mastoid muscle, and turned it upwards and downwards so as to clear out the subjacent structures.

Clinical Section.

June 10, 1910.

Mr. A. Pearce Gould, President of the Section, in the Chair.

Three Cases of Enlargement of the Tubercle of the Tibia (Schlatter's Disease).

By R. C. Elmslie, M.S.

Case I.—Male, aged 15. About October, 1908 (aged 13), fell upon the left knee; from that time he had pain in the knee, worse after walking or kneeling, and some swelling. In March, 1909, the tubercle of the left tibia was enlarged and tender, the movements of the knee being natural and painless. There was a quarter of an inch wasting in the calf, none in the thigh. The skiagram showed the epiphysis of the tubercle to be separate from that of the head, and the amount of the ossification in it very small. It was thought that there was a patch of tuberculous disease in the deep part of the tubercle, and an exploratory operation was performed. No disease was found, and the epiphysis of the tubercle was found not to be actually separated but to consist almost entirely of cartilage. The swelling and occasional pain in the tubercle have persisted.

Case II.—Male, aged 14. On February 10, 1910, was kicked on the right tibia, in the region of the tubercle; there was no accident to the left side. At the beginning of March he felt pain in the region of the tubercle of the left tibia. He came to the hospital for this on April 2. At this time the tubercle was very prominent on the right,

less so on the left; there was no enlargement of the joint or limitation of movement. The skiagrams show that the epiphysis of the tubercle is separate from that of the head on either side.

Case III.—Female, aged 13. In September, 1907 (aged 10½), came to the hospital on account of pain and swelling in the knee; no accident. A back splint and belladonna plaster were applied and the condition improved, but a little swelling in the region of the tubercle of the tibia persisted. In February, 1910, she returned to the hospital with both



Schlatter's disease. Case II: Skiagram of left knee.

knees painful; the tubercles of the tibia were then symmetrically enlarged. The skiagrams show that the epiphysis of the tubercle on either side is continuous with that of the head, but there is an additional faint shadow, apparently of an additional epiphysial centre, lying in a superficial mass of cartilage. This girl has, from the age of 9 or 10, done a great deal of kneeling and scrubbing, and this has always caused an increase of the pain and swelling.

This condition has not attracted much attention in England, and, when seen, is often wrongly diagnosed. Some cases are explored under the impression that tuberculous disease is present, as occurred in Case I; others are treated as syphilitic. These cases do not quite bear out the pathology suggested by Schlatter, that there is a fracture or partial separation of the epiphysis of the tubercle due to muscular pull. It seems that the condition is a chronic inflammation of an exposed epiphysis due to a number of slight injuries. It is probably much more common than is at present believed.



Schlatter's disease. Case III: Skiagram of right knee.

DISCUSSION.

The PRESIDENT (Mr. Pearce Gould) said the cases were very interesting, especially as they were accompanied by good skiagrams. Two showed a separation of the epiphysis of the tubercle of the tibia from that of the head of the tibia, while in the third it was continuous with it in a more normal manner.

It would be interesting to hear the experience of others, and as to whether they had found the tubercle of the tibia formed by an extension of the epiphysis of the head of the bone—the normal arrangement—or by a separate epiphysis.

Mr. CYRIL NITCH said that during the last five years he had seen eight such cases and had had them skiagraphed at frequent intervals. All his cases, with one exception, were due to a definite injury produced by vigorous contraction of the quadriceps extensor, and in none of them did the patient actually fall upon the knee. Shortly after the accident a few spicules of bone appeared in front of the displaced tibial "beak," and two or three months later these developed into a definite bony process continuous with the epiphysis and within and parallel to the patellar tendon. The formation of this bony process was, in a way, analogous to the development of such a traction—epiphysis as the great trochanter, for it was well known that in certain of the reptilia—e.g., the lizards, in which the legs were at right angles to the body—the femora did not possess trochanters, but when the legs were brought in line with the long axis of the body, as in man, trochanters were developed owing to the traction exerted by the gluteus medius and minimus. In some of his cases pain was a prominent symptom and lasted for a considerable time, from a few months to a year or longer. The first case he had seen was impressed upon his memory because the senior surgeon of the hospital to which he was attached took the condition for tuberculous caries and incised the swelling, but he only found a little glairy fluid and a few spicules of new bone beneath the patellar tendon. These were scraped away, and perfect recovery ensued.

Mr. ELMSLIE, in reply, said he gathered that Mr. Nitch agreed as to the pathological lesion being chronic inflammation due to trauma. He believed the condition was quite common, and he proposed to look at apparently normal children to see how common it was. The cases he showed had come before him in one year in a small out-patient department.

Fracture of the Humerus at the Site of an Innocent Cyst.

By R. C. ELMSLIE, M.S.

E. O., AGED 9, on May 12, 1909, fell whilst playing, with his right arm doubled under him, and fractured his humerus immediately below the surgical neck. He had fallen on the arm about three weeks previously without hurting it. In the ordinary routine a skiagram was taken, and it was found that at the site of the fracture, which was comminuted, but incomplete, the shaft of the humerus was expanded, a clear cystic space being revealed (fig. 1). This was thought to be an

endosteal sarcoma. On May 24 an exploratory operation was performed; the shell of bone was found to be splintered, but partly united by fibrous tissue; on removal of a splinter from the outer side a cavity was found extending for nearly 2 in. in the vertical direction. It contained a viscid, blood-stained fluid. The interior of the cyst was scraped, and sections of some of the tissue removed were immediately cut with a freezing



FIG. 1.

Cyst of the humerus. Skiagram at the time of the fracture, May 21, 1909.

microtome. The section showed spicules of bone surrounded by osteoclasts and large intervening areas of young fibrous tissue—exactly the appearances which are found in fibrous osteitis. Unfortunately the rest of the tissue was mislaid, and no permanent section secured. The wound was sewn up and healed well; no splint or after-treatment was adopted. After his discharge the patient was not seen again until April, 1910.

The arm is now strong and well, the humerus is still expanded at the site of the cyst, and the skiagram shows the presence of a cystic space, with, however, sclerosis of the surrounding bone (fig. 2). The case illustrates the very great importance of exploratory operation upon tumours thought to be sarcoma of the bones. The pathological process present appears to be that variously described as fibrous, fibro-cystic, or non-calcifying plastic osteitis.



FIG. 2.

Skiagram taken eleven months after the fracture, April 27, 1910.

DISCUSSION.

The PRESIDENT said that a few years ago he had a similar case at the Middlesex Hospital in a boy rather older who was sent from the country with the report that he had sarcoma of the humerus, which required amputation. There was a history of injury, and he had an enlargement of

the upper half of the shaft of the humerus. It was somewhat tender and irregular, and on taking a skiagram the bone was found to be converted into a thin shell. The upper half of the shaft of bone was considerably dilated. It did not seem to correspond with sarcoma, and he determined to cut down upon it to see what it was. He found a very thin shell of bone, which contained a little glairy blood-stained fluid, and he could not separate any lining membrane. He made a free opening into it, scraped it out, and then wiped it out with carbolic acid. He closed the wound, draining the cavity for a short time. The patient recovered with a very useful arm. The case was closely similar to the present one. One of the skiagrams of Mr. Elmslie's case before the operation was much more like central sarcoma than his own case was—more opaque. All must be impressed with the striking resemblance of this case to sarcoma, and it was fortunate that Mr. Elmslie was not misled. Such cases were very important, because they were apt to lead to errors in diagnosis.

Dr. A. C. JORDAN said that the only way of distinguishing between cystic disease and myeloid sarcoma of a bone was by a microscopical examination. In the May number of the *Archives of the Röntgen Ray* there was a plate illustrating both conditions. Whenever this appearance was shown the case should be looked upon with great suspicion, as it most usually turned out to be one of endosteal sarcoma.

Mr. LOCKHART MUMMERY had seen a similar case some time ago. The patient was a little girl aged 8, and the X-ray photographs showed what appeared to be endosteal sarcoma in the upper end of the humerus. It was not until the arm was amputated that it was discovered to be a cyst. There was a large blood-cyst in the upper end of the humerus lined by thin glistening membrane, the cavity of the cyst being filled with about 1½ oz. of blood and glairy fluid. The wall was examined, and was found to consist of fibrous tissue; there was no sarcoma at all. Such cases showed the importance of cutting into such tumours before amputation.

Jaundice with Areas of Unpigmented Skin.

By H. D. ROLLESTON, M.D.

A MAN, aged 59, has had jaundice, which came on gradually, since December, 1909, and has progressively lost strength and weight; he has been 14 st., and now is 8 st. 9 lb. He has suffered from pruritus. There is a secondary nodule close to and to the right of the umbilicus, and probably there is a growth in the portal fissure producing absolute obstructive jaundice. Over the thorax, abdomen, and back there are a number of elliptical or circular areas, usually about the size of a

shilling, which are relatively unpigmented as compared with the remainder of the skin. In the centre of nearly all these areas there is a small cutaneous tumour, in some instances sessile and resembling a mole, in others pedunculated and somewhat resembling molluscum fibrosum. There are a few relatively unpigmented areas without any enclosed tumour, and there are numerous moles without any surrounding halo of relatively unpigmented skin. There is no alteration of the general jaundiced hue around the nipples. While the patient has been under observation these relatively unpigmented areas have been seen to undergo alterations in size; some of them have diminished and become irregular in shape, some of them show a general darkening, and some



Photograph showing areas of relatively unpigmented skin in a case of obstructive jaundice.

of them appear to have increased in size. There is no evidence of any cicatrization in the relatively unpigmented areas. Dr. Parkes Weber has suggested to me that these relatively unpigmented areas are spots of latent congenital leucodermia, being deficient in the normal pigmentation only, and that they have been brought into prominence by the general jaundiced hue of the skin in which they share. For the photograph I am indebted to Mr. F. Damon.

Addendum.—After the patient was shown, the areas of relatively unpigmented skin became much less obvious, probably from increasing biliary staining of the skin. The patient died on June 25. The

necropsy, performed by Dr. R. S. Trevor, showed that there was a primary squamous-celled carcinoma surrounding the cardiac orifice of the stomach and extending up in the submucosa of the œsophagus. There were numerous secondary growths in the peritoneum, in the neighbourhood of the stomach, and the lesser omentum was much thickened. The hepatic ducts and the cystic duct were thus occluded by the invasion of growth from without; the intra-hepatic ducts were greatly dilated, and the gall-bladder was distended with mucus. The left ureter showed ureteritis cystica. From examination of microscopical sections of the skin, Dr. Trevor found that the normal pigment in the rete Malpighii could be traced up to the margin of the relatively unpigmented areas, on which it appeared to be absent. This confirms Dr. Parkes Weber's view as to the existence of leucodermia. One of the small pedunculated tumours in the centre of a relatively unpigmented area was shown microscopically to be a fibroma.

DISCUSSION.

Dr. F. PARKES WEBER said that the circular areas of unpigmented skin, though "unpigmented" in the ordinary sense of the word, were nevertheless jaundiced, like the rest of the skin. The patient's skin was not only yellow from the presence of bile-pigment, but also, with the exception of the "unpigmented" areas in question, it contained an abnormal amount of black pigment. Whilst the "unpigmented" areas were pale yellow, the rest of the skin had a very dirty as well as yellow appearance. The "unpigmented" areas represented doubtless areas of a kind of congenital leucodermia. They probably at one time had been hardly noticeable, but by contrast became very obvious when the rest of the skin became (owing to the presence of some visceral disease) abnormally pigmented. An interesting point, which perhaps made Dr. Rolleston's case unique, was that in the centre of some of the "unpigmented" areas there was a raised "white (unpigmented) mole," of congenital origin, or at least which had been there as long as the patient could remember.

Dr. JAMES GALLOWAY said he was inclined to agree with Dr. Parkes Weber's most ingenious explanation of the unusual patches in Dr. Rolleston's case. He had never seen a case of quite the same character, but it was well known that in molluscum fibrosum there was a large disturbance of pigmentation in the skin, usually in the form of excess of pigment. In widespread molluscum fibrosum it was remarkable the amount of pigmentation which could be seen, not only in the neighbourhood of the fibrous tumours, but in other parts of the body. He thought Dr. Weber's explanation that there might be, in congenital defects of the kind, complete absence of pigmentation, and even

inability of the skin to develop the ordinary skin pigment, in any circumstances, would account for this unusual case.

The PRESIDENT asked Dr. Rolleston if it was right to say that none of the nodules in the pale patches were secondary malignant growths. He took it that they were independent of the malignant disease from which the man suffered. So far as he could form an opinion, the little nodules in the unpigmented patches were not secondary malignant growths; and he thought one was debarred from associating the conditions too closely, because there were certainly two large and many smaller pale patches on the man's back, without any growth at all.

Dr. ROLLESTON, in reply, said he was somewhat sceptical as to the existence of a melanodermia of ordinary jaundiced patients, apart from that due to bile-pigment or its derivatives. He knew it was described, especially in cases of family cholæmia in which there were areas of brownish pigmentation of the skin though there was comparatively little jaundice. It would be difficult to prove that pigmentation of the skin which occurred in a patient with ordinary obstructive jaundice was due to pigment other than that derived from the bile. As bearing on the rarity of this skin condition, he might add that Dr. P. H. S. Hartley had told him that he had seen this condition in two cases of jaundice. He agreed with the President that the small tumours in the centres of the relatively unpigmented patches were innocent and not metastases.

Congenital Hairy Mole.

By CYRIL NITCH, M.S.

W. B., FEMALE, aged 9 months. Born with a large brown patch on the right side of the forehead and scalp. When one month old an excessive growth of hair commenced. At the present time the mole is about 3½ in. square and covered with a profuse growth of long, curly hair. I propose treating it with solid carbon dioxide, and showing the case again at a later date.

DISCUSSION.

Dr JAMES GALLOWAY said that he had no experience of the treatment of such a large hairy mole by means of solid carbon dioxide. He knew, however, that this method of treatment had been carried out with much critical observation at Charing Cross Hospital, and he had seen some results of the treatment. There could be little doubt that this method was a useful adjunct

in dealing with various forms of telangiectases, especially of the superficial type. In such an extensive case as this, involving the cutaneous tissues to their full extent, he thought that little good would result. The nævus could not be removed without the formation of a scar. The process must necessarily be painful, and the result would not be likely to be an elegant one from the cosmetic point of view. He would suggest to Mr. Nitch that he should exercise his ingenuity and devise some other method of dealing with this deformity.

Mr. NITCH, in reply, asked where, if it were excised, he should get the skin wherewith to graft it.

A Case of Cardiolysis.

By F. J. POYNTON, M.D., and WILFRED TROTTER, F.R.C.S.

R. O., MALE, aged 17.[1] Our object in showing this patient again is to add to the accurate knowledge of the after-history of patients who have undergone this operation—a comparatively new one in the surgery of the heart—for it is highly important not only to obtain a clear idea of the value of the operation as a surgical procedure, but also to place in its correct perspective the relation of the operation to the course of the disease for which it was undertaken. So far as this case is concerned it appears to us that the operation has been in itself a successful one, but subsequent developments have shown that the health of the patient has deteriorated from other complications.

During the summer of 1909 the improvement, so definite last year, was more than maintained, and the patient was able to interest himself by scoring at cricket matches and to lead quite a comfortable existence. In November he developed a cold and cough with pain in the left side, and in December was admitted to University College Hospital with signs of pleurisy with effusion on the right side. The effusion slowly increased and a pint of blood-stained fluid was removed by aspiration. After this there was steady gain in weight, and the temperature, which had been raised, fell to normal. He left hospital on January 15, 1910.

It says much for the improved condition of the heart that in spite of the respiratory complication there was only the slightest œdema, as shown by pitting of the extremities on pressure. The triple rhythm had

[1] Previously shown at the May meeting, 1909. Operated upon in October, 1908. (*Proceedings*, 1909, ii, p. 243.)

disappeared, the first sound was good in quality, and the veins in the neck were not full. With the pleural effusion there was considerable friction. In February he developed bronchitis, and a recurrence of fever with considerable wasting followed, and he was again admitted. His present state is briefly as follows: He is thinner than last year. The liver is easily felt; the spleen is just palpable; the veins in the neck are not full; there is some œdema. The triple rhythm is evident at times, but the heart is acting steadily. There is now no fever, but definite signs of thickened pleura at the right base and harsh breathing over the right upper lobe in front.

When we showed this patient in 1909 we emphasized the unusual history of his pericardial adhesions and the absence of endocarditis, and, though we lacked proof, suspected the tuberculous nature of the affection. The recent illnesses strengthen this belief, although examination of the pleural effusion and repeated examinations of the sputum have failed to show tubercle bacilli. The opsonic index at the time of the effusion was 0·88. It may be pointed out that up to the present no recurrence of mechanical difficulties as a result of leaving some of the periosteum of the ribs behind has been detected.

We incline to the view that the case is one of multiple serositis, probably of tuberculous origin, in which the relief of the embarrassed heart has done much to improve the patient's condition, but the disease itself has made decided advance in the respiratory tract.

Trigeminal Neuralgia of Left First and Second Divisions treated by Injections of Alcohol.

By Wilfred Harris, M.D.

C. S., married woman, aged 42, has suffered for the last five years from severe neuralgia on the top of the left side of her head and in the left side of the face and jaw. The pain commences on the crown of the head, and spreads as a shooting, cutting pain around the eye and into the face and nose. The attacks recurred frequently during the day, and were made worse by eating. Nothing abnormal found on examination.

May 12: Injected left supra-orbital notch with ♏x of 80-per-cent. alcohol, causing complete anæsthesia to all forms of sensation on the left side of the forehead and top of head, as far back as the crown. The

pain ceased after this on the top of the head, but still continued on the crown and in the face. May 14: Injected the left foramen rotundum with ηxv of 80-per-cent. alcohol, causing immediate complete anæsthesia of the whole distribution of the superior maxillary nerve on cheek, nose, upper lip, gum, and palate. May 17: Still complete anæsthesia as above, but darting pain still present over a small area on crown only. This spot was then injected subcutaneously with ηxv of the strong alcohol, and since that date there has been no further pain at all.

Dr. DE HAVILLAND HALL said the method of treatment was applicable to other nerves also. At the last meeting of the Laryngological Section Dr. Dundas Grant showed a patient with dysphagia from tuberculous disease of the larynx, in whom he had injected the superior laryngeal nerve with the same solution, and with immediate relief. The young woman presented the objective signs of tuberculous laryngitis, but she was afterwards able to swallow in comfort.

(?) Hyperplastic Tuberculosis of the Pelvic Colon.

By P. LOCKHART MUMMERY, F.R.C.S.

THE patient, a female aged 27, always had excellent health until the present illness, which commenced two years ago, after a severe fall in the hunting-field. She was thrown and the horse rolled on her, but she was able to ride home. As a result of the accident she was much bruised, and was laid up in bed for some weeks. Fourteen days after this she had a considerable hæmorrhage from the bowel. She was carefully examined at the time, but no satisfactory explanation of the hæmorrhage appears to have been forthcoming. This hæmorrhage ceased, and she resumed her ordinary life and again went out hunting. In July, 1908, she was suddenly taken ill with profuse hæmorrhage from the bowel, accompanied by severe pain and strangury. Her temperature was high, varying between 102° and 103° F. She had no vomiting; the stools contained large quantities of blood and jelly-like mucus, and it was said that pus in considerable quantities was seen on several occasions. There was slight diarrhœa, but this was never a marked feature; on the other hand, constipation of a severe character appears to have been more usual. These symptoms persisted, and the patient became so ill that in October, 1908, a surgeon was asked to see

her and diagnosed a pyosalpinx. He performed laparotomy (the patient was five months' pregnant), but found nothing to account for the symptoms beyond some adherent lymph on the pelvic colon, and some thickening of its walls. The abdomen was closed without anything being done. The patient had a rigor after the operation, and miscarried the next day. The temperature at this time was 103° F., and the patient was desperately ill. Several vaccines were tried, but do not appear to have done any good. There were frequent rigors, and the temperature became markedly intermittent. She suffered from severe abdominal pain, more especially when the bowels acted. In February, 1909, the temperature became hectic in character, with frequent rigors, and the blood in the stools still continued. Anti-dysenteric serum was injected without any effect.

On April 20, 1909, she was operated upon in St. Thomas's Home; a right-sided colotomy being performed. As a result of this operation, the patient's symptoms improved for a time. At first the bowel was washed through from the colotomy opening to the anus daily, but this soon became impossible as the water all returned via the colotomy wound. Soon after leaving the Home the colotomy ceased to act satisfactorily, and the major portion of the stools passed *per anum*. The bleeding from the bowel and the high temperatures with occasional rigors still continued.

Previous history: For some years previous to the present illness she had occasionally had slight bleeding at stool, which was attributed to piles, but no notice was taken of it, as it caused her no inconvenience. Two years before her illness she had lived in Ceylon for about eighteen months, and, while there, she had suffered with bleeding from the bladder for a few days. The cause of this hæmaturia was not ascertained. There was nothing else of note in her history.

Her condition when she first came under my care on November 1, 1909, was as follows: She was very anæmic and looked extremely ill. There was marked wasting, and she only weighed 6 st. 7 lb., whereas her proper weight was 9 st. 6 lb. She had frequent and severe pain in the abdomen and rectum, which appeared to be brought on by an action of the bowel. The pain was so severe that it had to be relieved by the administration of morphia daily. The stools were liquid, with scybalous lumps. Blood was present in clots, and there were large lumps of red, jelly-like mucus. There were about two stools daily.

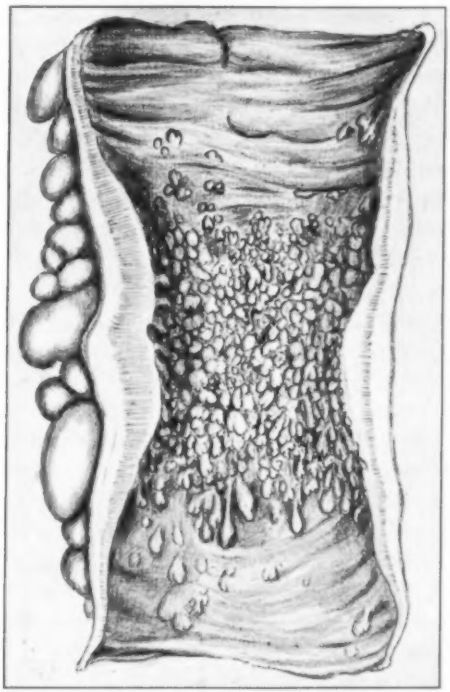
An examination of the abdomen did not reveal anything abnormal. In the rectum the opening of a recto-vaginal fistula could be felt low

down on the left-hand side; otherwise it felt normal. A sigmoidoscopic examination made without an anæsthetic showed some follicular ulceration, but not sufficient to account for the symptoms. The colotomy opening would just admit the tip of the little finger, and most of the stools passed *per anum*. It was found quite impossible to wash the colon out from the colotomy opening. Several examinations, both microscopical and bacteriological, were made of the stools, but did not throw any further light on the case. Repeated examinations of the blood also failed to aid the diagnosis. Although the bilharzial parasite is not endemic to Ceylon, it was thought possible that the patient might be suffering from bilharziosis, and several examinations were made to discover the ova, but without success. Liver abscess was also suspected, and the liver was explored with needles under an anæsthetic, but without finding any pus. While the patient was under the anæsthetic I again examined the bowel with the sigmoidoscope, and was able to see higher up than had previously been possible. I discovered a number of small polypi surrounding the lower end of a tight stricture, situated somewhere about the middle of the sigmoid flexure. A diagnosis was made of stricture, due probably to pericolitis or hyperplastic tuberculosis, and it was decided to operate if the patient's condition could be improved sufficiently to afford a reasonable chance of her surviving the operation.

In the meantime, with the object of cleaning up the colon, a pure culture of the Bulgarian lactic-acid bacillus was injected daily into the colon through the colotomy opening. By January, 1910, the patient was sufficiently improved in health to warrant the performance of the operation, although she still had rigors about twice a week. I operated on January 8, assisted by Captain Spencer, R.A.M.C.; the anæsthetic, chloroform, being administered by Dr. Blumfeld. Subcutaneous infusion of physiological saline solution was given during the whole period of operation.

On opening the abdomen it was found that the central portion of the pelvic colon was represented by a hard, rigid tube. There were few adhesions, but a number of enlarged inflamed glands both in the mesentery and along the great vessels. The sigmoid flexure was resected between clamps, about 6 in. or 8 in. being removed. The gap in the meso-sigmoid was first sewn up and then the ends of the bowel anastomosed end to end with a double row of sutures, the inner of which took up all the coats. The abdomen was then closed in three layers. The operation was well borne, and the patient, in spite of her emaciated

condition, did not show any symptoms of shock. The bowels were opened on the second day by an injection of olive oil and magnesium sulphate, introduced by the colotomy. The patient made an uninterrupted recovery and rapidly put on flesh. The temperature, for the first time in eighteen months, came down to normal and remained there. The patient went home in three weeks from the time of the



Drawing of stricture cut open, showing thickening of walls and polypi.

operation, and by the beginning of March was quite well and weighed 8 st. 6 lb.

Description of the specimen: The specimen consists of about 7 in. of the pelvic colon, in the centre of which there is a tubular stricture about 4 in. in length. The stricture was so tight that it would not admit a lead pencil except with difficulty. When laid open it was seen

that the whole of the mucous membrane was covered with small, pedunculated polypi, the largest of which was rather smaller than a pea. These polypi extended throughout the stricture and for some distance on either side. There was one very small ulcerated portion of mucous membrane near the centre of the stricture, otherwise the mucosa was not ulcerated. The walls of the stricture on section were, in the fresh specimen, quite ¼ in. in thickness and extremely hard. The thickening of the bowel wall was uniform throughout the strictured portion.

Sections of the specimen were reported upon by the Laboratories of Pathology and Public Health as follows: A section of the wall of the stricture shows general hyperplasia of the bowel wall. This is especially marked in the mucosa, which shows extreme hypertrophy of the follicles, with much hyperplasia and round-celled infiltration of the stroma—i.e., a definite adenomatous formation. *Pari passu* with these progressive changes the gland tissue shows marked degeneration of its epithelium, resulting in the follicles being distended with mucoid secretion. The inner muscular coat is inflamed. Its vascularity is increased, and large vessels filled with blood are seen.

A further section cut from the centre of the stricture was reported on as follows: Here the glandular tissue is reduced in amount and the inflammatory new formation is excessive, due it would seem to ulceration, since the glandular elements are replaced by organized granulations. Sections stained by the Ziehl-Neelsen method were searched for tubercle bacilli, but with a negative result. Portions of the mucosa from the fresh specimen immediately after its removal were made into an emulsion and injected into guinea-pigs at the Lister Institute. A month later the guinea-pigs were killed and examined, but no evidence of tuberculosis was found.

I believe the condition to be one of hyperplastic tuberculosis of the colon. The appearances of the specimen suggest this, as also do the history and the microscopical findings. It is true that the most careful search has failed to demonstrate any positive evidence of the tubercle bacillus, but it is well known that in quite a large proportion of cases of hyperplastic tuberculosis of the colon it is impossible to find tubercle bacilli. The possibility of the condition being one of bilharzia of the colon was thought of, but in the first place neither the bilharzia parasite nor the *Schistosomum japonicum* is known to exist in Ceylon, and, further, no trace of ova could be discovered either in the stools or in any of the specimens. The apparent explanation of the septic infection and rigors from which the patient suffered seems to lie in the

microscopical abscesses which can be seen scattered about in the walls of the stricture. At any rate, no other septic focus was discovered, and the temperature became normal directly after the stricture had been resected.

DISCUSSION.

Mr. GORDON WATSON said Mr. Mummery was to be congratulated on the way in which he made the diagnosis, and the successful issue to which he had brought the case. He (the speaker) was not familiar with any other specimens of hyperplastic tuberculosis of the sigmoid flexure, therefore he did not know what such a condition ought to look like. But on looking at the specimen and the microscopic sections he was surprised, if it was tubercle, not to see some evidence of loss of mucous surface or of breaking down on the surface of the polypoid tags of the mucous membrane, or any evidence of tubercle on the peritoneal surface. He had carefully examined the microscopical sections, and where the hyperplasia of the mucous membrane existed there did not seem to be microscopic evidence of anything resembling tubercle.

Dr. ROLLESTON said he had seen and examined microscopically cases of hyperplastic tuberculosis of the colon, and it might often be very difficult to prove that they were tuberculous. In one instance the chief evidence was that there were definite tuberculous changes in an adjacent lymphatic gland, for the wall of the colon showed hyperplastic inflammation but no histological evidence of tuberculosis. Mr. Mummery's description of his case raised the questions: need the word "tuberculous" be retained in the title, and would not "hyperplastic colitis" be sufficient and more accurate? But perhaps Mr. Mummery hesitated to describe a new disease. Probably some of the cases at present included under the heading of hyperplastic tuberculosis of the colon might be due to some other chronic infection. Further, in Mr. Mummery's case, considering the eighteen months' duration, there was scarcely enough thickening in the colon to correspond with the naked-eye appearances of hyperplastic tuberculosis. In cases of hyperplastic tuberculosis of the cæcum, the commonest situation for this lesion, this thickening might be enormous and might obstruct the lumen of the caput cæci. It had been suggested that most of the cases of primary carcinoma of the cæcum which had remained free from recurrence after operation, and had not been submitted to microscopical examination, were in reality examples of hyperplastic tuberculosis of the colon.

Mr. CARLESS said Dr. Rolleston had anticipated the question he was about to ask—namely, as to the condition of the lymphatic glands of the sigmoid. In hyperplastic tuberculosis of the cæcum the glands were often found to be enlarged. He had in mind the case of a girl who had had a nocturnal temperature of 102° to 104° F. for some time. On operation he found extensive disease of the cæcum, ascending colon, and ileum; the peritoneum was much roughened and ragged, and behind the cæcum and along the ascending colon there were

masses of tuberculous glands. He removed the lower end of the ileum, as also the affected portion of the large intestine, and did an ileo-colostomy. The glands were left. The temperature dropped at once to the normal, and never rose again, and uninterrupted recovery ensued. The diagnosis of thickened hyperplastic growths of the colon was a very interesting question, on which more information was needed. He was satisfied that there were some conditions which were not tuberculous, and which would get well without anything more than an exploratory operation. He had in mind at least two such cases, in both of which the condition was thought to be carcinoma, and in both the patient recovered perfectly. One involved the cæcum and ascending colon, and the other the sigmoid flexure. In the former case the patient was an old Indian officer, whose general condition was so unsatisfactory that he did not care to attempt the removal of a mass so fixed and adherent; he closed the abdomen after doing an ileo-colostomy to relieve the possibility of obstruction. Subsequently the local practitioner put the patient on a course of trypsin and afterwards of amylopsin, and the condition cleared up entirely.

Mr. R. P. ROWLANDS said he had met with four cases of non-malignant stricture of the colon during the last two years, particulars of two of which were published in the *Lancet* of April 30, 1910. In both of these he had made a mistake in diagnosis, thinking them to be malignant. In one he found what he thought to be a growth, a greatly thickened colon, as long as his forearm, leading from the rectum to the splenic flexure. Above that the colon was enormously distended with fæces. The patient was very ill, too ill for an extensive resection, and he therefore joined the ileum to the front wall of the rectum 5 in. from the anus. She was much better now (nearly two years after the operation), but she was not well; she had occasional diarrhœa, vague abdominal pains, and was more or less of an invalid. Unfortunately, he was not able to cut away any portion of the diseased bowel, and therefore microscopical examination was not done. In the second case the same mistake was made, but the stricture was more localized, and therefore he decided to remove it. He did not know until he had cut into the specimen after resection that it was not malignant. On cutting into the free border of the bowel afterwards, the mucous membrane was found to be smooth; only the muscular and peritoneal walls were involved, and they were very thick. Microscopic examination showed no evidence of tubercle, and the mesentery, which was greatly enlarged, showed small abscesses near sacculi, but no sign of growth. He removed all the enlarged glands, and the patient made a good recovery. He had known two other cases in which the strictured sigmoid was removed under the same impression and with satisfactory results. These were patients of Mr. Dunn and Mr. Steward, at Guy's Hospital. He asked if dissection had been made of the mesentery and appendices epiploicæ in Mr. Mummery's specimen, to see if there were any sacculi there. He would not have discovered sacculi in his cases had it not been for Dr. Telling's paper; the orifices into the bowel were very small. The first patient was aged 60; another was aged 47; and a third,

aged 26, was at present in Guy's Hospital, and Mr. Dunn removed, as he thought, a malignant growth of the sigmoid, though microscopically it did not prove to be of this nature.

Mr. MUMMERY, in reply, said such cases were distinct from ordinary tuberculous lesions, and more comparable to the curious cases of tuberculosis of the larynx and lymphadenoma. The reason for calling these cases tuberculous rested on the statement that in some 50 per cent. tubercle bacilli could be found in the submucous layers. But even in those cases ulceration of the mucous membrane was exceptional, and, when it was present, generally occurred not at the site of greatest stricture, but above it, suggesting that the ulceration was secondary to the stricture. About two years ago, for his Jacksonian Essay, he collected 100 of those cases, and, as Dr. Rolleston said, nearly three-quarters of them were at the cæcal angle, or the lower end of the ascending colon, and in only 50 per cent. could one prove the tuberculous origin by inoculation or by microscopical examination of the specimens. But the resemblance between them made it probable that they were closely connected. There must have been many other organisms present to produce the temperatures which the patient had in this case. There were records of four cases in which the entire colon was converted into a structure as hard as a gas pipe. In one case he operated himself; one was operated upon by another surgeon, who mistook it for diffuse carcinomatosis. The other two cases which resembled it were both microscoped, and one of them gave the test for tuberculosis, both by inoculation into rabbits and by finding the tubercle bacilli. The condition must be very rare, as he could find records of only four cases. In this case two guinea-pigs were inoculated, both were killed a month afterwards, but both were apparently in good health, and post-mortem examination of the guinea-pigs revealed no disease anywhere. The experiment was done at the Lister Institute. There were many glands in the meso-sigmoid and in a large part of the abdomen. He did not attempt to remove the glands, as he was satisfied it was not a malignant condition. The glands had not caused trouble since, and he believed they had disappeared. The only gland which was cut showed nothing but fibrosis and small-celled enlargement. He had not dissected the specimen to see if there were sacculi in the fat; the reason he did not was that he was under the impression that the cases in which there was pericolitis due to multiple diverticula of the colon were over middle age. He thought it was due to old constipation. Apparently the whole history dated from a hunting accident, which was followed by severe hæmorrhage from the bowel two days afterwards.

Case of Phlegmonous Gastritis from Hydrochloric-acid Poisoning: Vomiting of the Complete Mucous Membrane of the Pyloric Half of the Stomach.

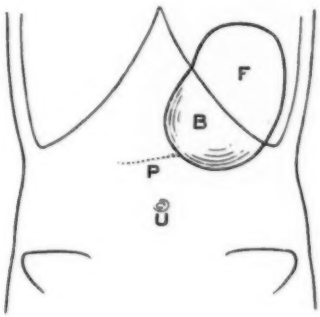
By S. A. Clarke, A. F. Hertz, M.D., and R. P. Rowlands, M.S.

W. P., aged 41, in a fit of depression, swallowed about 6 oz. of spirit of salts[1] on April 13, 1910, and immediately afterwards he drank a cup of tea. On Dr. Clarke's arrival, a few minutes later, he was given half-pint doses of sol. pot. bicarb. until the vomit was alkaline. The vomited matter was a dark brownish fluid containing mucus and blood. There was no sign of burning on his lips or tongue, but a whitish slough all over the back of his throat. His pulse being almost imperceptible, he was put to bed, packed with hot-water bottles, and given a saline injection and ice to suck. He did not complain of any pain in the throat, but had a burning sensation and great tenderness in the right side of his abdomen. After his bowel had been washed out he was given nutrient enemas every four hours. He continued to vomit a good deal of blood-stained mucus for two days. All this time his throat was much inflamed, and he had difficulty in swallowing even a teaspoonful of water. Each time his throat was swabbed, he vomited about a teacupful of stinking pus and debris. This continued for about ten days, after which he was able to swallow freely. A fortnight from the date of the poisoning he began to take a little more milk and Benger's food, about 4 oz. every two hours. At the end of the third week, as he was free from pain and there was no tenderness, he was allowed a teacupful of milk and Benger's food every two hours from 6 a.m. till 10 p.m., with a little sponge-cake and bread-crumbs. His bowels were kept open by enemas every second day. He got up on to the sofa at the end of the third week, and was given a little jelly, junket, and pounded fish with his milk. His appetite was good, and he thoroughly enjoyed everything he ate. He vomited occasionally a little clear fluid, and sometimes the milk. The urine, which at the beginning of the second day became scanty and very high coloured, by this time had become normal, and the fæces, which had been black for about two weeks, became normal in colour. He lost flesh very rapidly, and continued to do so throughout.

[1] Spirit of salts is about the same strength as acidum hydrochloricum (B.P.).

On May 13, after a meal, to which he added thin toast (without leave), he felt sick and vomited up the cast of his stomach. He was again kept in bed for four days. All solid food was knocked off, and his diet consisted again of milk and Benger's food. From the time that the cast was vomited he lost his appetite, and was troubled a good deal with flatulence and abdominal discomfort. He now took milk in smaller and smaller quantities. He vomited occasionally, and continued to lose flesh and get weaker, but remained free from pain. On June 6 he was sent to Dr. Hertz at Guy's Hospital.

When first seen at Guy's on June 6, the stomach could be palpated just beneath the left costal margin; after milk had been drunk the stomach was seen to rise up and was felt to harden periodically, but no definite peristaltic waves were visible. An X-ray examination confirmed



F, fundus; **B**, body of stomach palpable below the costal margin; **P**, obliterated pyloric half of stomach; **U**, umbilicus.

the conclusion made from the abdominal examination—namely, that the pyloric half of the stomach was practically obliterated, and that there was obstruction in the passage of food into it. Even on turning the patient on to his right side no bismuth could be made to pass beyond the globular cardiac end. In a skiagram taken by Dr. A. C. Jordan a narrow shadow could be seen passing from the globular fundus to the right, which probably represented the remains of the pyloric half of the stomach (*see* fig.).

A stomach tube was passed six hours after the bismuth meal of ½ pint of bread and milk with 2 oz. of bismuth oxychloride, no food or drink having been taken in the interval; 36 oz. of brownish, clear fluid

were evacuated, with a good deal of bismuth, but only traces of food; some more bismuth was removed on now washing the stomach out.

A test-meal of ¾ pint of tea, with bread and butter, was given on June 10 at 9.30 a.m. The stomach was emptied at 10.45 a.m., and 2 pints of fluid were evacuated; only traces of bread were visible. Dr. J. H. Ryffel kindly examined the fluid and made the following report: "The gastric contents are acid to litmus, but not to Congo red or Gunzberg's reagent. They contain 0·0036 per cent. combined HCl, and the ether extract gives a good thiophene test for lactic acid, so that the acidity is mainly due to this acid; butyric acid is also present; the biuret reaction is a definite pink. Proteolytic ferment is present, as shown by its rennin action on milk, but the digestion of fibrin is very slow."

A piece of mucous membrane removed from the cardiac sac of the stomach in the course of the subsequent operation was examined by Dr. Gordon Goodhart. The mucous membrane was congested and full of small, inflammatory cells. No oxyntic cells could be found; this explains the almost complete absence of HCl from the gastric secretion. The submucous tissue was also congested and full of inflammatory cells, and there were numerous hæmorrhages into it. In some places the muscular coat was invaded by young connective-tissue cells.

The cast vomited by the patient is 6 in. long; it forms a bag, which is 2½ in. in circumference at its smaller end and 8 in. at its larger end; the opening into it at the smaller end is about 1½ in. in circumference; it probably represents the position of the pylorus; a number of smaller fragments were vomited simultaneously. Dr. Gordon Goodhart has kindly made sections of the cast. Most of it is so disorganized that its origin cannot be recognized. Some epithelial cells are, however, present near its inner surface, and on the deeper surface the remains of numerous blood-vessels, some of considerable size, could be distinguished. A few well-preserved fragments of muscular tissue are also recognizable on its outer aspect. It may therefore be concluded that the cast consists of the whole thickness of the mucous membrane, with some of the submucous tissue and muscular coat of the 6 in. of the stomach nearest the pylorus. Owing to cicatricial contraction the lumen of this part of the stomach has become almost completely obliterated, but it allows fluids to be forced through it with extreme slowness by the very active contractions of the intact fundus. The mucous membrane of the fundus gives rise to an excessive quantity of secretion, but it is much weaker than normal gastric juice, containing traces only of hydrochloric acid

and ferments. As the mucous membrane of the fundus normally secretes a very active juice, it must also have been injured by the spirits of salts.

NOTE ON THE SUBSEQUENT OPERATION BY R. P. ROWLANDS, M.S.

At the operation on June 13 the abdomen was opened through the upper part of the left rectus. The cardiac part of the stomach was so distended that it was difficult to get a view of the pyloric part and of the duodenum, and it was found impossible to bring the posterior wall of the stomach down through the rent made in the transverse meso-colon, although the patient had not had any food by the mouth for fourteen hours. A stomach tube was passed, and about 2½ pints of coffee-coloured liquid and debris, but no gas, escaped. It was now quite easy to examine the stomach thoroughly. The pyloric part was hard and narrower than the first part of the duodenum, which was normal in every way. It looked like a narrow and thick-walled piece of small intestine; it was fixed to the posterior wall of the abdomen. The narrowing was most marked 5 in. from the pylorus immediately to the right of the distended cardiac pouch. From this point a tough thick band extended to the under surface of the right lobe of the liver. The gastro-colic omentum had been drawn up at the same spot, and was adherent to the front wall of the stomach and to the lesser omentum. Otherwise the anterior wall of the stomach was free from adhesions. There were many enlarged and very hard glands in the lesser omentum and about the head of the pancreas. There were several recent adhesions on the under surface of the transverse meso-colon towards the right, and the meso-colon was generally shrunken so that it was rather difficult to make a free opening within the loop of blood-vessels. Owing to the adhesions of the pyloric part of the stomach to the back wall of the lesser sac, it was difficult to bring any of the cardiac pouch down through the mesenteric aperture. With the aid of clamps posterior gastro-jejunostomy was performed. The opening in the stomach was 3 in. long, and extended obliquely downwards and to the left to the greater curvature, about 3 in. to the left of the right border of the cardiac pouch. The upper end of the opening in the jejunum was 3 in. below the duodeno-jejunal junction. The mucous membrane of the stomach was inflamed, thin, and very friable. The sero-muscular coats of the stomach were also thinner, and far more friable than natural, so that the sero-muscular suture tore out at several places. The edges of

the rent in the meso-colon were sewn to the jejunum just below the anastomosis by means of three interrupted sutures. The patient bore the operation well, and was free from pain and able to take ordinary food when last seen early in August.

A Case of Pulmonary Tuberculosis with large Cavity treated by a New Method; also Notes of other Cases in which Ether was tried.

By R. H. Hodgson, M.D.

Case I.

Male, married, aged 36, came under treatment in February, 1908, with the following history: He is of a consumptive family, two sisters and one brother, all grown up, having died of phthisis, and three remaining sisters are all delicate. His grandmother also died of this disease. In August, 1906, the patient was treated for enlarged liver, atonic dyspepsia, and dilated stomach; for the last he had a tube passed daily for a fortnight. After two months he was told he was cured and was ordered to Bournemouth, where he stayed until January, 1907. A month later the pain returned, accompanied by intense irritation of the sole of his right foot, which lasted from 3 p.m. to 6 p.m. daily. A few weeks later he went to Westcliff, where, for a time, he felt better. He had a constant cough with expectoration of about 8 oz. a day, and frequent night-sweats, so severe that they used to wake him up and necessitate a change of night-shirts half a dozen times in a night. He considered that his weight had not varied for nine years, and that it was 9 st. 7 lb. His appetite was extremely bad, and he had not tasted meat for months, with the exception of Christmas day. The diet consisted principally of a small quantity of milk and fruit. The sight even of food, more especially of fat, made him feel sick. He was extremely weak and breathless, and could scarcely walk a few paces without resting; his sight was dim. He had hectic flushes, his fingers were much clubbed, and his nails, more especially on the third finger of each hand, were much incurved, and the bases congested. His tongue was dirty, with red tip and edges. His temperature was 101·7° F., pulse 120, respiration 28, and weight 9 st. ¾ lb. A number of his teeth were

missing, and the remainder were bad. Both apices were dull on percussion, but the remainder of the right lung was fairly resonant. The left was quite dull from the clavicle downwards, and there was evidence of a large cavity on that side; pectoriloquy and amphoric breathing were well marked, and there was loud moist sounds. He was sent to bed; his sputum, examined by the Clinical Research Association, reported to contain a considerable number of tubercle bacilli. On February 22 he was, for the first time, put under ether for twenty minutes; the same night he had only three sweats and very little cough. On the morning of February 23 he enjoyed his breakfast. On February 24 he was put under ether for thirty minutes, and that night he had but one slight sweat; his expectoration was considerably reduced. On the night of February 25 he did not sweat, and felt better than he had done for months; the next day he was put under ether for thirty-three minutes, followed by only one slight sweat. He was put on a diet of boiled pork, of which he ate heartily, three pints of milk, seven eggs, vegetables, and bread and butter. So fond was he of pork, not having been able to consume fat for so long, that he often partook of it two or three times in a day. Before the treatment was commenced the patient could lie upon either side with fair comfort, but during the first three days of administration he always appeared to have a list to the affected side. After three or four days, however, this inclination disappeared, and he could lie with comfort in any position.

March 3: Dullness of left lung, much more circumscribed. March 6: By this day the cough had practically ceased. March 7: Right lung perfectly clear. March 12: Clubbing of fingers and congestion of bases of nails completely disappeared. March 26: Brought up some phlegm slightly streaked with blood. After first few days' treatment patient was up and about his room. After about one week he was out walking from five to fifteen miles a day, when I thought it advisable to limit the walks to twelve miles a day, which he did with comfort. No rise of temperature followed these walks. It is now eighteen months since this patient has had any administration, and twelve months since he took any medicine, yet he keeps his health and has been at his office for two months.

From February 26 to July 7 the patient was put under ether daily for periods lasting from half an hour to an hour, and from July 7 to August 13 at irregular intervals of two or three days. The object of continuing the administration was to cause the complete obliteration of the cavity, and for this same reason many drugs were added to the

ether. The expectoration, which was about 8 oz. at the commencement, was reduced to less than 1 oz. within a week, 1 dr. within a month, and by the third month was about 20 gr., after which it decreased to between 3 gr. and 15 gr. He now, on most mornings, brings up 5 gr. and nothing more the rest of the day. This sputum contains tubercle bacilli. The only alteration I find in him during the last eighteen months is that the once absolutely dull left lung is now becoming gradually more resonant. His respiration, which is only 8 per minute, does the work of the lungs most efficiently. So rapid was the alteration of the fingers under the inhalations that I could almost daily see the lateral bulging disappearing, and the roots of the nails, which stood far above the level of the fingers and contributed largely to the curving of the nails, soon sank to their normal position.

The fact that the man has not altered for the worse, but rather for the better, during the last two years points, I consider, to the conclusion that he has been rendered immune against the advance of consumption.

Case II.—Male, aged 35, suffering from bronchitis and tuberculous pleurisy, was sinking from obstruction of his bronchial tubes. Was perfectly well of his bronchitis in one week, and his pleurisy in about three weeks.

Case III.—Male, aged about 40, with tuberculous pharyngitis and laryngitis, sent home from hospital, was unable to swallow even water; he took ether, and in three days' time could drink a whole tumbler of milk at once. Nourishment not sufficiently pressed, patient's condition so extreme, wasted, and feeble that he died in a week.

Case IV.—Male, aged 27, with phthisis and general tuberculosis. Chest nearly well in two months; abdomen much improved in four months. Removed to the country and lost sight of.

Case V.—Male, aged about 70, with advanced heart disease, dilated cavity; dying in his chair. Ether administered; he revived and lived nine months; found dead in bed.

Case VI.—Female, aged about 55, with uræmic coma; no urine passed for a day and a half. She was given ether, became partially conscious, and excreted 1½ pt. the same evening.

Case VII.—Male, aged 17, had tuberculous disease of the kidneys and was in the first stage of phthisis, and passed scarcely 1 pt. of urine in twenty-four hours. Lungs cured in a fortnight, and kidneys cured in two months.

My experiments with ether have taught me: That, contrary to the general belief, ether may be advantageously inhaled in pulmonary tuberculosis, bronchitis, tuberculous pharyngitis and laryngitis, heart disease, and certain kidney diseases, provided it be administered without the admixture of exhalations from the lungs: That ether when thus administered is a safe anæsthetic: That Clover's inhaler, when used

with the bag, is most dangerous: That the danger only creeps in when the ether becomes impure by admixture with the patient's exhalations. That as a general anæsthetic ether is not dangerous: That the 400-odd deaths which took place under anæsthesia last year might have been obviated if ether had been used in the manner here recommended: That there is no other apparatus extant which does not admit of re-breathing: That the sputum of bronchitis is thinned, rendered much more translucent, rapidly reduced in quantity, and more easily expectorated, when pure ether is inhaled; the reverse of which obtains when the ether administered has become only slightly adulterated by admixture with the patients' exhalations: That the same admixture appears to be the cause of excitement during the administration of ether for the purpose of operation: That patients treated with ether exhibit increase of muscle tissue, and do not become fat and flabby.

The instrument above referred to is one made by the Hospitals and General Contracts Co., Mortimer Street, and is an instrument devoid of all regulating valves, stops, plugs, &c., beyond two automatic valves for directing the current through the apparatus.

Sequel of a Case of Pigmentation of the Circum-oral Skin and of the Buccal Mucosa in Pernicious Anæmia.[1]

By H. D. Rolleston, M.D.

This patient remained fairly well for some months after he was exhibited before the Section, but in April of this year he became extremely anæmic, and was admitted into St. George's Hospital and died in May from progressive asthenia. Pigmentation of the buccal mucosa in pernicious anæmia is extremely rare. Moorhead,[2] who recently recorded an example, was able to collect four cases only from literature, including my case. The necropsy, performed by Dr. R. S. Trevor, who has provided the following account, shows that the case was one of pernicious anæmia, and that the suprarenals were healthy:—

"At the necropsy, performed forty and a half hours after death, the body was thin and wasted. The eyelids, nose, cheeks, lips, and chin were covered with pigmented spots, resembling freckles. Spots of pig-

[1] Shown at a meeting of the Clinical Section on Oct. 8, 1909 (*vide* p. 9).

[2] *Brit. Med. Journ.*, 1910, i, p. 865.

mentation were present on the inside of the left cheek, opposite the tooth-line. The pigment in the mouth was much darker and more chocolate-coloured than that on the face. The penis was infantile in appearance and there was a complete absence of pubic hair. Both testes were in the scrotum, but were extremely small. The median scrotal raphe was deeply pigmented.

"Thorax: The pleuræ were normal; the lungs (right 15 oz., left 14 oz.) were pale, bloodless, and œdematous. The infratracheal glands are pigmented but normal in size. Pericardium normal. Heart, 10 oz.; the ventricles are slightly dilated: in both is a small quantity of thin, yellow, gelatinous 'chicken-fat' clot. Valves normal. The heart-muscle shows marked fatty degeneration with tabby-cat striation. The surface of the heart is loaded with lemon-yellow coloured fat. The aorta is small, delicate, and thin, and looks as though it ought to belong to a person half the age of the deceased. The condition of it may well be called hypoplastic. Under the intima there are plaques of early fatty change.

"Abdomen: Fat plentiful. Liver (3 lb. 4 oz.) is enlarged and reaches well below the rib margin. It is brownish-yellow in colour and of normal consistence. It gives an immediate and most marked reaction with Perl's test for free iron. Gall-bladder normal. Spleen (8 oz.) is slightly larger than normal, but does not project below the ribs post mortem; it gives an immediate and well-marked reaction with Perl's test. It is dark red in colour and firm on section. Pancreas is normal; negative reaction with Perl's test. Kidneys (5 oz.) are pale and brownish-yellow in colour from fatty change. They give a most marked reaction with Perl's test. Adrenals normal. Bladder normal.

"Alimentary canal: The mouth is free from obvious sepsis, and the gullet is normal. The stomach is dilated and its mucous membrane covered with sticky mucus. On washing this away, numerous small, sharply-punched-out ulcers or oval erosions with bile-stained floors are brought to view. The intestines are free from ulcerations. Fifty-two inches from the ileo-cæcal valve there is a large Meckel's diverticulum adherent by an adhesion attached to its free end, which is hammer-headed in shape, to the cæcum. The small intestine above the point of commencement of the diverticulum is much dilated and communicates with the intestine below the diverticulum by a small strictured opening. The diverticulum is practically the continuation of the ileum traced upwards from the ileo-cæcal valve, and into this blind tube, as it were, the upper portion of the small intestine is implanted laterally, the

communication being very small. The colon and the vermiform appendix were normal.

"Bone-marrow: The marrow of the sternum of one of the ribs and of a piece of the shaft of the right femur was examined. In each instance it was deep red in colour. In one part in the rib it had a slightly gelatinous appearance. In the marrow of the femur a few yellowish streaks were visible. The nervous system was not examined.

"Microscopic sections were prepared from the liver, spleen, kidney, suprarenals, mucous membrane of the cheek, and from the femoral marrow. In the liver and kidney the most noticeable feature is the widespread deposit of hæmosiderin. In the liver this pigment is almost universally distributed in the cells throughout the lobule; being, however, most abundant in the cells nearer the periphery. In the kidney the pigment is chiefly present in the cells of the convoluted tubules. In both the liver and kidney sections the absence of fatty degeneration to any marked extent is noteworthy. In the spleen the reticulum is prominent, and there is a very high grade of endothelial proliferation, but active phagocytosis cannot be made out. The sections show the spleen to be crowded with red cells, which are strikingly pale and degenerate. In other places much granular pigment is present, lying along the trabeculæ of the reticulum. The sections of the pigmented patch in the buccal mucosa show the pigmentation to be in the dermis, in oval, pigment-carrying cells, which lie grouped in the papillæ. It is not an exaggeration of the normal pigment of the rete Malpighii. The pigment does not give any reaction with Perl's test, and is therefore of a different nature from that seen in the viscera. Its dark colour favours the view that it belongs to the melanin group. The suprarenal glands are normal.

"The bone-marrow from the femur shows for the most part a typical erythroblastic reaction of megaloblastic type. The non-nucleated red cells present show every variety of size and shape. Myelocytes are scanty. There are large numbers of marrow-cells or megaloblasts present as well as phagocytic cells of large size, in which numerous red cells can be seen to be incorporated. In the purely red areas of the femoral marrow the fat has disappeared. In sections from a portion showing yellow streaks to the naked eye the fat is present and the erythroblastic reaction is less marked. In one such area a hyperplastic nodule is present in which myelocytes seem to be fairly numerous, and the red cells appear to be relatively scanty. This nodule has the appearance of a patch of leucoblastic reaction."

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VOLUME THE THIRD

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1909-10

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1910

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October 29, 1909.

Dr. LEONARD WILLIAMS, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

To Redress the Balance.

To the sciences of balneology and climatology it has always been something of a reproach that their use in therapeutics is purely empirical. That, unfortunately, is a reproach the justice of which cannot be gainsaid, and I have thought that I should be doing something to merit the honour which has been conferred upon me in electing me to be the first President of that Section of the Royal Society of Medicine which concerns itself with these subjects, if I endeavoured in this address to throw some light, however tentative and theoretical, upon the rationale of the methods which we employ—if I attempted, in short, to redress the balance between science and empiricism, which has too long shown so decided a bias in favour of the latter.

The science of medicine in general is necessarily so occupied with detail that the constant tendency of its devotees is to miss the broad outlines; "we cannot"—as the saying is—"see the forest because of the trees." In order to obtain a prospect of any value to our present purpose it is essential that we should, in the first instance at any rate, forget the existence of the trees and make our survey as wide and comprehensive as possible. With this end in view I would first venture to remind you of what, though obvious, is too frequently forgotten—namely, the extent to which man, and more especially woman, is subject to what are called the cosmic influences. It is axiomatic to say that man cannot escape the ascendancy respectively of day and night, of forenoon and evening, nor of the cyclical effects of the seasons; and it

must be equally apparent to the seeing eye that the lower down in the evolutionary scale we descend, the more completely do we find him the slave of these silent, immutable, and relentless forces. So subject, indeed, is primitive man to these cosmic influences, so completely does he hibernate and otherwise conform to the cosmic laws, that the phases of his life sink almost to the level of the alternation of generations which characterizes certain insects. The higher in the scale he ascends the more independent of these influences does he become, until he arrives at the high state of civilization in which, so long as his health is preserved, he may be considered as practically beyond their sway.

Here I may pause to point out one of the many reasons why woman is, and always must be, essentially and indefeasibly inferior to man—namely, because she is, by reason of her very womanhood, the docile slave of a lunar monthly revolution, from whose tyranny she can never escape.

So long, then, as civilized man retains his health, he can afford to disregard these cosmic influences. He may defer the principal call upon his digestive powers to the hour when his frame is exhausted and the sun is set; he may do his best work in the night season; he may labour in winter, and in summer play—these, and other gauntlets of defiance, he may cast with impunity in the teeth of the cosmos, but only so long as his health is maintained. No sooner does he fall a victim to disease than he becomes once more the puppet and plaything of the cosmic powers. Obedient to the rhythm of twilight and dawn, his temperature rises by night and falls by day; the hour of sunset enwraps and constricts him, as it would a flower, and by the powers of darkness, like a little child, he is dismayed. Though we cannot explain these subtle and ubiquitous agents, we are nevertheless obliged, all of us, to submit to them, and the wise amongst us are those who recognize them and turn them to account.

The same may be said of climatic influences. These are, after all, but a subsection of the cosmic influences, which become invested with additional weight and added subtlety when we consider that they include the factors of heredity and environment—the factors, that is, which indelibly stamp upon races, nations, families and individuals the characteristics which distinguish them from one another. The importance of the part which climatic influences have played, are playing, and are about to play, in the evolution of man towards his higher destiny, it is impossible to overlook and would be difficult to overstate. The differences between the white man and the black, between Asiatics

and Europeans, between the Latins and the Saxons, between the northerner in each nation and his not far distant southern brother, are admittedly, and even obtrusively, climatic in origin. Indirectly climatic, through heredity, is the persistence of these differences through every change of individual milieu; and directly climatic are the modifications which change of milieu imposes upon families and races. In illustration of this last point I need only mention the fair skin and the red hair which long residence in northern climes has conferred upon some Jewish families, and the stereotyping, by the climate of the North American continent, of the descendants of the motley crew of its widely-dissemblant annual European recruits into the hatchet-shaped face and wiry frame of the Red Indian aborigines. The mills of God grind slowly, but they grind exceeding small; and anyone who will study the effects of the climatic factor in determining racial differences will have no trouble in persuading himself of their reality nor of their inexorable and eternal power.

When we take the cold and cosmic northern Scot—cosmic only because we are supposing him to be an invalid—and transport him to the shores of the Mediterranean, we are tearing him from the environment which, acting through countless generations, has made him what he is—fair of skin, and of temperament cautious, taciturn, slow, in order to subject him to the environment which, acting through the same period of time, has produced his antithesis—the typical meridional, swarthy, impulsive, voluble, quick. Such a change must obviously involve an alteration in the man's whole being. The nature, as well as the force, of the stimuli to which he normally reacts is changed, and every cell in his body experiences sensations so new as in their sum to amount to a revolution. We have only to consider that his altered environment would, in a few generations, transform this Scot into that Italian, to realize the ferment—one might almost say the chaos—which this sudden transplantation must effect upon the workings of his internal economy. It is as if we were suddenly to place a seaweed in a greenhouse.

We are as yet unacquainted with, we can at best vaguely conjecture, which it is amongst the various elements which go to form what we call climate that takes the leading part in producing or initiating changes so profound. A great French climatologist, when asked "What is climate?" replied "Humidity"; that is true, but in a limited sense only. It is true that humidity supplies the key-note to the subjective effects of climate, and therefore to its immediate results (a climate, for

example, is sedative or bracing, according to whether its humidity is high or low), but it is not true of its remote or permanent effects. Of humidity it cannot be said that it affords any explanation whatever of the racial and national distinctions which have just been touched upon —distinctions of which, if they are to teach us anything, climatic considerations must take full and explicit cognizance.

To contemplate these distinctions is to be immediately struck by the paramount part played by the element of colour. The widest differences in the degree of dermic pigmentation are associated with the most striking diversities in other directions, osseous, nervous, intellectual, temperamental, and it is not unreasonable to suppose that the factor which produces the one should be to some extent responsible for the others. This factor is admittedly the influence of the sun, to which we may accordingly look for some assistance in explaining the complicated phenomena with which we are at present concerned. To read the riddle of the sun as it affects human beings is a task as much beyond my compass as it is beyond my powers, but this, at any rate, I would venture to say of it, that the key to that riddle, if it is ever to be found, must be sought, primarily, in the large, important, and unduly neglected organ which we call the skin. The functions of the skin, we are told, comprise protection, sensation, secretion, heat-regulation, and respiration. A little consideration will, I think, suffice to persuade us that it is endowed with at least one other, and that other no less important than any of the foregoing. The amount of pigment in the skin varies directly with the degree of sunlight to which its wearers are exposed. A large amount of pigment is thus obviously protective, and the question which immediately arises is, "Protective of what?" Of mere sensation it cannot be: first, because the skin in its power of developing callosities and increasing the growth of hair is already adequately protected in that direction; and, secondly, because ordinary sensation being so unevenly distributed over the integument, we should expect the protective pigment to be patchy, even as the protective callosities and protective hairs are patchy, in their distribution. In order to explain the uniformity of pigmentation we must find some function of the skin as a whole which demands protection from the excessive action of the sun's rays, and this, it seems to me, can only be done by investing the integument with the properties of a sensitive plate, stimulation of any portion of which will produce reflex activities in some distant organ. Such a conception has the merit of illuminating some obscure places, and serves in a measure to explain some of the phenomena upon which we are engaged. The

pigmentation itself, being merely protective, the variations in its degree would necessarily be accompanied by variations more or less constant in other directions, and this we know to be the case. The integument thus becomes the receiving plate for stimuli which are carried to organs which preside over modifications of structure, and of these organs certainly the most important are those glands, the thyroid, the suprarenals, the pituitary, the testicle, and others, the study of whose internal secretions has recently opened up such a wide and fascinating field for physiological inquiry.

So far as their importance in the matter of external conformation is concerned I need do no more than refer to the consequences of the absence of thyroid secretion in the causation of cretinism and most forms of infantilism, and to the responsibility of the pituitary for acromegaly and most forms of gigantism. That cutaneous stimulation provokes a corresponding stimulation of these internal secretory glands, and that one, at any rate, of the purposes of pigmentation is to protect these glands from an excess of such stimulation—such is the view which I am venturing to suggest to you. The considerations in support of this view are many, but time compels me to limit myself to a few only.

It is well known that the white man cannot live for more than a generation or two in the tropics, and it is equally well known that the black man cannot live for more than a generation or two in the temperate zone. In the one case this is due to excessive stimulation from which the fair skin is unable to protect the internal secretory glands; in the other it is due to inadequate stimulation: the feeble and infrequent sun's rays cannot penetrate the pigmented skin in a degree sufficient to keep the glands up to the level of their normal activities. In each case individuals may survive, families very seldom, and races never. It is significant that the individuals among the whites who most successfully adapt themselves to tropical conditions invariably develop a degree of pigmentation which in some cases is truly surprising, and it is pertinent to note that disorders of the internal secretory glands, the suprarenal, the thyroid, and the ovary are generally characterized by abnormalities of dermic pigmentation.

If, now, we bring the argument down to a particular case and consider the effects of climate upon one of these internal secretory glands, the thyroid, we shall find that the subjects of thyroid insufficiency, as evidenced by rickets [7], adenoids [13], nocturnal enuresis [14], simple goitre [11], and some forms of chronic arthritis [9], preponderate enormously in the temperate zone, and especially in those parts of the

temperate zone which are characterized by a cold, damp climate, which is another way of expressing a climate which is conspicuously devoid of sunshine. The same diseases, and others which are almost certainly the results of the insufficient defensive action of these internal secretory glands, are known to occur with obstinate frequency in the poorer populations of the great cities—populations, that is, whose unfortunate state deprives them of many things, but especially, and markedly and perennially, of the influence of the sun's rays.

I am, however, very far from wishing to suggest that solar stimulation is the only form of stimulation which, acting on the cutaneous sensitive plate, is capable of keeping the glandular secretions at the level of their optimal activities and their normal interbalance. I have little doubt, for example, that the action of the air itself when allowed to reach the integument plays an important part in this matter, and that the preposterous habit of wearing flannel next the skin, which is so common among the inhabitants of cold, damp climates, inasmuch as it prevents the air from coming in contact with the cutaneous surface, is responsible for much of the physical and mental degeneracy which characterizes those who descend to this practice.

We may obtain some further support for the view which I am suggesting by considerations drawn from another branch of our work—namely, balneology. And by balneology I must be understood to include massage, heat, light, and the other accessories to balneary treatment proper of which the spa physician rightly avails himself.

About the year 1840 an ignorant and illiterate Silesian peasant of the name of Priessnitz attained to world-wide reputation as a healer of all forms of disease, acute and chronic, specific and metabolic, medical and surgical, by the employment of methods which we now call hydrotherapeutic. The undoubted success of these methods, more especially in certain classes of disease, led to their study and investigation by serious physicians in Germany and elsewhere, with the result that they have gradually been placed upon a scientific footing. And yet not quite. Much has been done to sift the corn from the chaff, to provide indications and contra-indications, to devise improvements and discard superfluities, but up to the present no one has succeeded in offering any acceptable explanation as to the physiological paths by means of which these results are obtained. One, in the pride of his ignorance, will assert that it is all due to change, another that it is the flushing effect of concomitantly ingested waters, a third that it is all a question of suggestion. It is not necessary to take these so-called explanations seriously,

and I mention them only for the purpose of pointing out that they contain that element of half-truth which is ever the worst travesty and the greatest enemy of the whole truth. The problems which await solution are serious problems, which cannot thus summarily be brushed aside. We have to explain, and until and unless an explanation is forthcoming we must continue to suffer the reproach of empiricism to which I referred at the outset; we have to explain, I say, how it is that substantially the same methods of treatment influence such diverse maladies as rheumatism, scrofula, anæmia, neuralgia, obesity, cardiac insufficiency, renal inadequacy, hepatic inadequacy; and even such mutually exclusive diseases as gout and rheumatoid arthritis. The balneary procedures which are employed in these cases are very properly subject to considerable modifications according to the type of invalid and the nature of his malady, but they have, at any rate, this one great feature in common, that they stimulate the cutaneous surface. To say that this stimulation of the cutaneous surface acts by increasing metabolism does not advance us much further, for it does not explain which of the functions of the skin it is which is instrumental in effecting the alteration in the blood-plasma, which is surely the method by which the improvement is ultimately brought about. If, as I have already suggested, we invest the skin with the functions of a sensitive plate receiving impressions—solar, aerial, tactile, thermal—from without, which it transmits as stimuli to the internal secretory glands—the thyroid, the suprarenal, the pituitary, and their congeners—we have at once the key to that improvement in the state of the blood-plasma upon whose cardinal importance in all matters metabolic Dr. Harry Campbell [1] rightly insists.

That we do not know very much about the action of these internal secretions is all too true. We know something about each of them individually, perhaps, but we know little or nothing about the factor in the situation which is in all probability the most important—namely, the interdependence of these secretions and the result of upsetting their normal relations to one another. The difficulties which are liable to occur at the menopause afford some clue to the importance of this side of the question, but precise information is wanting. The materials at hand are nevertheless sufficient to afford us some assistance. What we know of the thyroid, for example, amply suffices to raise our estimate of its tribute to the blood-plasma to the level of one of the most essential of the many important constituents of that complex fluid. Its complete absence means cretinism in infancy and myxœdema in adult life; that

we know, but we are only just beginning to realize the effects upon the organism when this internal secretion, though present in sufficient amount to protect us from these gross and obtrusive diseases, is nevertheless so diminished as gravely to interfere with the normal metabolic processes. I venture to suggest that investigation in this difficult but highly attractive field would reveal the fact that many of the diseases which we now regard as having nothing in common except their amenability to balneary therapeutics would be found to be the dissimilar children of the same parent, and that parent—thyroid inadequacy.

To illustrate this point I select the example to which I have already referred—namely, that two diseases which are to all appearances mutually exclusive (namely, gouty arthritis and rheumatoid arthritis)—may in some measure, at any rate, be brought within the same category by regarding them both as due to perversion or insufficiency of the internal glandular secretions. Of rheumatoid arthritis it is necessary that I should premise that I regard the condition so described as liable to be provoked by a great number of causes, that it is in fact not so much a disease as a symptom, and it is therefore necessary that I should specify what, in the present connexion, I include under this name. The variety to which I wish to refer is that which occurs with such frequency in women at or about the menopause—the variety, that is, to which Nathan has given the name of metabolic osteo-arthritis. That this is due to a disturbance of the inter-relation between the internal secretions is obvious, and is, indeed, admitted by all recent observers [5]; that it is due to a failure of activity on the part of the thyroid is a thesis which has been sustained by numerous observers [6] in France, and recently very ably by Dr. Menard, of Paris [8]; that clinical results strongly support this pathology of the affection is evidenced by the decided amelioration which ensues from the exhibition of thyroid extract, a matter to which I can add the testimony of my own experience. In the case of gouty arthritis, not only do we find it in subjects who bear the stigmata of thyroid inadequacy—the subjects, that is, of premature baldness, of neuralgia of all sorts, of constipation—but the disease is admittedly one of inadequate metabolism. Now, conditions of inadequate metabolism cannot co-exist with a normally acting thyroid, for, again to quote from the elegant and attractive writings of Dr. Harry Campbell, "the thyroid secretion plays the part of a bellows, causing the vital fire to burn more fiercely" [2]. Moreover, the admitted causes of gout, an excess of meat foods and alcoholic drinks, are likewise admitted as poisons which gravely depress the functions of the thyroid.

Here, then, are two diseases which, though widely dissimilar in many and perhaps in the most important respects, have nevertheless three points in common: both are characterized outwardly by an arthritis, which affects the same joints in much the same manner; both can be shown, and have been shown, to be accompanied by an insufficiency of thyroid secretion; and in both stimulation of the skin by baths, douches, heat, light, produce an amelioration which experience proves to be beyond all question or cavil. There seems to be no escape from the conclusion that so far as the arthritic element is concerned the responsibility must rest with the thyroid; nor that the amelioration which all concur in conceding to the balneary procedures is brought about by the stimulation of that gland.

Before leaving the question of the thyroid there are some other facts to which it seems pertinent to refer. The first of these is that, since the discovery of radio-activity, every spa in Europe has been busily engaged in demonstrating that its waters contain a certain measure of this peculiar, unknown property, quite irrespective of the results which may ultimately be shown to attach to the exposure of the human body thereto. Now, out of the chaos of hypothesis, romance, and heroics which have already encrusted the subject, one fact, and one fact only, has so far emerged, and that is the fact established by a French observer to the effect that radio-active waters produce an enlargement of the thyroid gland. Another point of interest in this connexion is furnished by the relation of the thyroid gland to the salts of calcium. It is known that one of the functions of the gland is to fix the salts in the body, and further investigations into the matter have shown, as indeed one would expect, that the exhibition of these salts stimulates thyroid activity. One of the first signs of increased thyroid activity is diuresis, and I may mention parenthetically that when I was a practising balneologist I learned to gauge the measure of the ultimate success of my prescriptions by the degree of urinary activity which they evoked. Now, in the matter of ingested waters, it has always been difficult to understand why it is that the renal spas—Contrexéville, Vittel, Martigny, Evian, Wildungen—whose waters chemically contain nothing more diuretic than small quantities of calcium salts, should nevertheless have the power, which they undoubtedly possess, of provoking a very largely increased urinary output. In the light of the above-mentioned facts, it is evident that this power is derived in a large measure from the stimulation of the thyroid gland which these calcium salts excite.

Of interest also in association with the thyroid gland are those spas

which lay claims to the cure of sterility in women. These claims are strongly supported in too many instances to permit of reasonable doubt as to their justice, and Franzensbad, Kreuznach, Rheinfelden, Schinznach, Eaux Bonnes, St. Sauveur, and Salies de Béarn, to mention a few only, all enjoy a reputation in this direction which is unquestionably well founded. Some of them, indeed, include instances which are historical. But here, again, the causative factor has always remained a mystery. Where local douches are employed it is easy to understand that they may be helpful, but then not only may local douches be employed at home, but local douches were unknown at many of these spas until long after their reputation in the cure of sterility had become firmly established. They have, however, all of them made a special feature of general baths, general douches, and the other methods of stimulating the cutaneous sensitive plate, by which means I suggest that the internal secretory activities are stimulated. That it is the thyroid which is chiefly appealed to by such means is rendered probable by the very conspicuous involvement of the skin and its appendages in myxœdema and in degrees less pronounced of thyroid inadequacy. That the thyroid is a gland whose activities are essential to pregnancy is well known [15] for one of the most efficient means of treating sterility is by properly regulated thyroid treatment [4], and the reason for this was provided by Gaskell [3], when he showed that the thyroid gland of vertebrates is derived from the uterus of the palæostracean ancestor.

From consideration of the thyroid, too, we may obtain much instructive information concerning another subject which looms large in the purview not only of every health-resort physician but of every practitioner of medicine, a subject of which it is safe to say that more nonsense of a pseudo-scientific kind has been written than upon any other topic in the whole range of medicine—I mean diet. This is neither the time nor the place to embark upon a discussion of the matter, but this, at any rate, I am prepared to assert, that the explanation of all the numerous meat-excluding dietaries, whether they be based upon the absurd and happily discredited theory of the toxicity of uric acid, or whether upon the puling and degenerate sentimentalism which shrinks from taking animal life, are in reality due to the fact that their originators, promulgators, apostles, and disciples all suffer from diminished activity of the thyroid gland upon which meat foods are known to act so as still further to diminish its activity.

But while thus drawing some illustrations from the thyroid gland I must not be understood to suggest that this is the only secretory

organ which is stimulated by the various methods under consideration. Such a narrowing of the issue is very far from my intention. The other glands which we include in the same category are probably of at least equal importance, but the available facts concerning them are at present scarcely such as to lend themselves to illustration. A few, nevertheless, there are.

To return for a moment to the question of dermic pigmentation, to which I referred at the outset, it is obvious that, judging from pathology, it is to the suprarenals that we must look for the greatest power in this direction. It is well recognized that when these glands are profoundly disturbed, as in Addison's disease, the fairest of northern skins is made to assume the dusky tint of the mulatto. But the suprarenals do not bear the sole responsibility for pigmentary activity. We know that utero-ovarian disturbance may produce chloasma, and that Graves's disease and rheumatoid arthritis are almost invariably accompanied by abnormal pigmentary phenomena. Nor is it, in truth, necessary to interrogate pathology on this subject, for physiology is ready with an illustration which is sufficiently striking. The blood, which we know to be manufactured in some of these internal secretory glands, has a pigment peculiar to itself, and significantly enough its colour is precisely that which is employed by the photographer in order to protect his sensitive plate against the action of the actinic rays of the sun. That one, at any rate, of the purposes of the red corpuscles is to afford a similar protection to some of the internal organs must be perfectly obvious to anyone who considers the matter [12].

But it is useless to multiply instances of the association of individual internal secretory glands with the subjects with which we are engaged, for I regard as infinitely more important to a true appreciation of the whole question a realization of the power which these glands exercise over one another. That they stand to one another in a position of physiological antagonism, and that the smooth and sufficient working of each is essential to the prevention of excess or perversion in one or some others, and thus to the plasmic equilibrium of the whole body, is now well recognized by all observers in this field [10]. This reciprocal action of the various internal secretions, this interglandular synergy—the process, that is, by which each contributes its due share, and neither more nor less than its due share, to the blood-plasma—constitutes a factor of the utmost importance in the production and maintenance of what we call health, the condition which all can appreciate but none can define. And, in my desire to emphasize this aspect

of the matter, I would venture even further and suggest to you that in the exact proportional representation of these glands in the individual economy is to be found the secret of those subtle yet salient differences of character and temperament which distinguish the man of action from the man of thought, the scientist from the poet, the ascetic from the voluptuary. That cosmic influences determine the production of certain of these types is suggested by the fact that so many of any particular type tend to appear on the world's stage at about the same time, as witness the Augustan and Elizabethan periods in literature, the Napoleonic in men of action, and the Victorian in men of science; and that climate may have a determining voice in shaping the cosmic influence is rendered probable by the fact that a large proportion of the great men of any particular epoch are drawn from the same country. It is a fascinating, even though it be a futile, speculation to think that with a little more of this internal secretion, or perchance a little less of that, Sir Alfred Austin might have been a Shakespeare and General Boulanger a Cæsar; that with an infinitesimal difference in the admixture of these elusive essences Charles II might have been a Puritan and Bismarck an honest man. A more practical application of the same idea, more especially having regard to Gaskell's work on the thyroid, is that the determination of sex in the fœtus may be found to be governed by internal secretory considerations.

But to return to the questions of climatology and balneology, from which I have already too widely strayed, it might conceivably be urged that inasmuch as the pharmacist supplies us with reliable preparations of these internal secretory organs, the practice of balneary and kindred procedures will ultimately become displaced by organotherapy. This is a proposition which I find it very difficult to accept. If it is possible to foretell anything about the future of medicine, it is surely safe to predict that in moving away from mere drug-therapy, as we are steadily doing, we are following an orderly scientific evolutionary movement. Our ideals are less and less in the direction of curing disease, for in its presence, when once established, we realize that we are for the most part merely enlightened onlookers, and our endeavours tend in consequence more and more in the direction of preventing, evading, and forestalling disease. It is our aim rather to correct nascent or acquired tendencies than to cure established ills, and when we have succeeded in educating the public to this view of our mission the day of the climatologist, and perhaps more especially of the balneologist, will be at hand. Of the medical triumphs of the immediate past the most conspicuous

and enduring are those which have been won in the field of public prevention, and it is my firm belief that the most conspicuous triumphs which await us in the immediate future will be won in the field of individual prevention. In this field the most effective weapons are those which we are here to study, to advocate and, if need be, to defend; and so long as we pursue our ideals in the true spirit of scientific inquiry, so long will this Section of the Royal Society of Medicine continue to increase in prosperity and to advance in dignity, in worth and influence.

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The Hygiology of Naples.

By John J. Eyre (Naples and Salsomaggiore).

Naples is the most populous town in Italy, and occupies probably the most beautiful situation of any city in the world. It is built at the foot of, and on the slopes of, several hills named Posilipo, Vomero, and Capodimonte, which rise in an amphitheatre on the north-west side of the Bay of Naples. These hills are entirely volcanic, and consist of tufa intermixed with yellowish fragments of trachyte and overlaid with alternate layers of lapilli, pumice-stone, sand, and pozzolana, materials almost all used from ancient times for building purposes.

The city is divided into two unequal parts by the hills of Capodimonte, Sant Elmo, and Pizzofalcone, which ends abruptly at the Via Chiatamone, near the Castel dell' Ovo. To the east lies the oldest part of Naples, which is also the business quarter, and, up to the time of the grave cholera epidemic of 1884, contained many very narrow and tortuous streets of great density of population. Since 1884, however, most of these have been demolished, and have been, or are being, replaced at a higher level by wide and airy streets, at a cost to Naples of about 150,000,000 fr. The west and most healthy part of the city, in which nearly all the principal hotels, pensions, and villas, as well as the Villa Nazionale (public gardens) are situated, extends from the Pizzofalcone to Mergellina and the hills of Posilipo.

The above-mentioned hills afford considerable protection to the city, especially its western part, against the west, north-west, and north winds; but, being exposed to the south, and being only in part protected by the Pizzofalcone on the east, it has no protection from the south-west and south-east winds—which, however, are true sea breezes—and only partial protection from the east winds.

Climate.

The first meteorological observations made in the Royal Observatory at Capodimonte (149 m. above sea-level) go back to the year 1821. They were initiated by Charles Brioschi, and have been distinguished into three series. The first, which extended from January, 1821, to September, 1832, included Brioschi's observations; the second, due

to the astronomers Nobile and Leopold del Re, embraces the period 1836-65; the third extends from 1866 to 1900, and to it have contributed Professor F. Brioschi and Dr. Angeletti and Dr. Contarino. This last series has been fully and ably dealt with by Professor Alberti, of the Royal Observatory, in his "Climate of Naples," and to it the writer is indebted for the following facts. It is necessary to mention that the observations of the temperature were made at 9 a.m. and 9 p.m., those of the atmospheric pressure, humidity, cloudiness, &c., at 9 a.m., 12 noon, and 9 p.m.; and that the mean annual temperature of the low part of the city is about 2° F. higher than that of Capodimonte.

Temperature of the Air.

The mean annual temperature of Naples is 60·5° F. (*see* table, p. 16); that is, about 10 degrees higher than that of London. It reached 61·8° F. in 1882, which in this period (1866-1900) represented the hottest year, and it descended to 59·1° F. in 1871 and again in 1883. Hence the absolute mutability of the mean annual temperature has the small value of 2·7° F. Excluding 1893, when the mean annual temperature was equal to normal, the other years can be distributed in a group of twenty whose deviations were positive, and in a group of fourteen whose deviations were negative. The sum of the former is + 11·7° F., and that of the latter is − 11·9° F. Hence the mean value of a positive deviation is + 0·58° F., and of a negative − 0·85° F. One thus sees that while the rise above the normal mean is more likely to occur than the descent under it, yet the fullness of the descent is greater than that of the ascent in the warmer years. The arithmetical mean of the whole of the deviations, all considered as positive, gives the so-called mean mutability of the annual temperature as 0·68° F. The mean relative mutability regarding the individual seasons was as follows:— Winter, 1·5° F.; spring, 1·0° F.; summer, 0·7° F.; autumn, 1·1° F.

Considering now the mean monthly temperature, one finds that July is the hottest month (75·4° F.), and then follows August (75·1° F.) On the other hand, in the coldest month, which is January (46·8° F.), the temperature has never fallen below 23° F., and the lowest mean of this month was 41·1° F. in 1893. Further and stronger proof of the mildness of the Naples climate is obtained by examining the mean daily temperature. The days have been very rare (nine) in which this has fallen below 32° F., and those have been still more rare (four) in which it has exceeded 36° F. On one occasion alone in the whole period

TEMPERATURE, HUMIDITY, CLOUD, RAIN AND WIND.

	Temperature			Mean relative humidity	Mean vapour tension in mm.	Cloudiness in tenths of sky covered	Cloudless days	Rainy days	Mixed days	Covered days	Rain, amount, in mm.	Wind, general direction, in decreasing scale	Wind, hourly velocity of, in kilometres
	Mean	Mean maximum	Mean minimum										
	F.	F.	F.										
January ...	46·8°	51·7°	42·9°	71·8	6·16	5·3	10·89	12·4	8·22	11·89	97·54	N.E., N., S.W.	4·14
February ...	49·0°	54·0°	43·9°	70·6	6·43	4·9	10·44	10·1	7·90	9·66	63·17	N.E., S.W., N.	3·77
March ...	51·2°	57·2°	46·3°	69·0	6·98	5·2	10·00	11·7	9·04	11·96	73·12	S.W., N.E., W.	4·72
April ...	56·7°	63·1°	51·1°	69·6	8·47	5·5	8·19	11·9	10·37	11·44	75·67	S.W., W., E.	3·67
May ...	63·8°	71·0°	57·4°	67·1	10·59	4·5	11·81	8·2	11·19	8·00	53·42	S.W., W., E.	2·98
June ...	70·3°	77·7°	63·7°	66·8	13·18	3·3	16·70	5·7	9·41	3·89	31·64	S.W., W., W.S.W.	2·82
July ...	75·4°	83·3°	68·6°	63·3	14·80	1·9	23·67	2·7	6·18	1·15	19·12	S.W., W., W.S.W.	2·48
August ...	75·1°	83·0°	68·6°	63·5	14·76	2·1	22·59	4·2	6·85	1·56	23·88	S.W., W., W.S.W.	2·21
September ...	70·6°	77·7°	64·9°	68·0	13·52	3·4	16·00	6·9	9·26	4·74	69·56	S.W., W., N.E.	2·10
October ...	62·9°	68·8°	58·2°	72·3	11·10	4·6	11·85	12·2	10·37	8·78	125·63	S.W., N.E., S.W.	2·78
November ...	54·8°	59·9°	50·7°	72·9	8·37	5·0	10·33	13·3	8·63	11·04	112·08	N., N.E., S.W.	3·57
December ...	50·0°	53·4°	45·3°	72·7	6·80	5·4	9·48	13·9	8·78	12·74	120·70	N.E., W., S.W.	4·41
Annual mean	60·5°	66·7°	55·1°	69·0	10·10	4·3	161·95	113·2	106·20	96·85	865·53	S.W., N.E., W.	3·30

(1866-1900) has the daily temperature maintained itself under 32° F., and it was January 23, 1869, in which it snowed and the maximum reached 31·7° F. In this period the extreme maximum temperature of 99·1° F. occurred on August 30, 1881, and the extreme minimum of 23·9° F. on February 18, 1895; hence the absolute mutability was 75·2° F. With regard to the mean values, to the maximum and minimum annual temperature correspond the figures 92·1° F. and 30·6° F. respectively; hence the mean mutability is 61·5° F. The maximum annual temperature has fallen twice in June, fifteen times in July, eighteen in August, and once in September, with the dates limited between June 23 and September 5; while the minimum was met with fifteen times in January, ten in February, five in March, once in November, and six times in December, with the dates limited between November 25 and March 16. Therefore the maximum annual temperature occurs more frequently in August than in July, though July is the hottest of all the months; and the minimum in January, which is the coldest month. It is to be noted, also, that the maximum daily temperature exceeding 95° F. was met with only in July (four times) and August (twice). The minimum daily temperature equal to or below 32° F. was met with by preference in January and February, never in April. The total number of days in the period 1866-1900 in which the temperature fell to or below 32° F. was 86, which gives a mean of 2·4 days in the winter months—that is, between November 25 and March 26.

Finally, we may note that the mean temperature of the six winter months (November to April) is 51·4° F., which is about equal to that of Mentone (51·7° F.) and greater than that of Rome (49·2° F.) and of Cannes (50·8° F.), and that the mean daily range in the same period of the year is only 9·8° F. This is an undeniable proof of the equability of the Naples climate in the coldest months.

Humidity of the Air.

Tension of Vapour and Relative Humidity.—The annual percentage of humidity is 69, which is greater than that of Rome (65). However, the relative humidity of the six winter months (71) is nearly equal to that of Rome (70), and is less than that of Cannes (73). January and the last three months of the year are the most humid (71·9); July and August are the driest (63·4) as well as the hottest months. On the other hand, the mean tension of vapour has its highest value (about 15 mm.) in July, and its lowest in January (6.16 mm.). From summer

to winter and from spring to autumn one has for relative humidity the excursions − 7·79 and + 5·58; hence the difference of the mean relative humidity in autumn and in winter is only 0·6. The maximum annual relative humidity occurs most frequently in January and December; the minimum is found in all the months save November and October, most frequently in July, and exceptionally in June and December. The annual maximum of vapour-tension is met with, as a rule, in July and August, the minimum in January and February. With regard to these observations, however, Professor Alberti justly remarks "that they have slight value when one thinks, on the one hand, of the hours of observation (9 a.m., 3 p.m., and 9 p.m.), and, on the other, of the hours in which the true daily hygrometrical maximums and minimums take place." This, it is hardly necessary to add, holds good also for the hygrometrical observations recorded in all other meteorological stations.

Hygroscopic Richness.—In the following table this factor is given for each month and the year. The relative hygroscopic richness

Hygroscopic richness	Jan.	Feb.	March	April	May	June	July	August	Sept.	Oct.	Nov.	Dec.	Year
Effective	82·9	86·5	94·3	114·7	143·7	179·2	201·7	201·1	183·6	150·4	113·0	91·7	136·8
Maximum	110·0	116·1	130·4	159·4	205·7	258·0	308·0	298·9	260·2	198·8	148·1	119·3	181·9
Relative	75·1	74·5	72·4	72·0	69·8	69·5	65·5	67·3	70·6	75·7	76·3	76·8	75·2

indicates, as is known, the true relative humidity of the air, for, in the formula, which expresses it, one takes into consideration the atmospheric pressure; and the effective hygroscopic richness, according to Professor Cantoni, with greater exactness than the absolute humidity f, and more conveniently than the relative humidity $H = \frac{f}{F}$ (F maximum tension of vapour at the temperature corresponding to f), indicates the relation in volume between the aqueous vapour and the dry air which together compose the unit of volume of the humid air.

Fogs never occur in Naples and mists are very rare.

Cloudiness, Rainfall, and Evaporation.

Cloudiness.—Differing from the preceding observations, these began from the year 1874, because before that year the cloudiness was

expressed qualitatively and not quantitatively in tenths of the sky covered. This mode was adopted owing to the decision of the Meteorological Congress held at Vienna in 1873. The annual mean absolute cloudiness in tenths of sky covered is 4·3, and the mean for the six winter months is 5. The annual average number of cloudless days is 162, of mixed or cloudy days 106, and of covered days 97. The maximum number of cloudless days (24) occurs in July, and the minimum (8) in April. The maximum number of mixed days (11) is met with in May, and the minimum in July. The maximum number of covered days (13) is met with in December, and the minimum (1) in July. The year 1874 was the richest in cloudless days (191), and 1879 in covered days.

Rainfall.—The annual mean rainfall is 865 mm. (35 in.); that of the six winter months is 542 mm. The greatest number of rainy days is met with, as a rule, in December (13·9) : then come November (13·3) and January (12·4). The greatest rainfall occurs in October; then come December, November, and January. The annual number of rainy days is 113; that of the six winter months is 73. Snow rarely falls in Naples, inasmuch as in the period 1866 to 1900 it fell only forty-six times—namely, fifteen times in January, twelve in March, nine in February, and once in November and in April. Thunderstorms occur, as a rule, on seventeen days in the year, and their number in the six winter months is 6·15.

Evaporation.—Owing to different causes, this observation was limited to the period 1886 to 1900. The yearly mean evaporation of water is 730 mm.; the maximum (100·1) occurs in August, and the minimum (34·4) in February.

Wind and Atmospheric Pressure.

Wind.—The observations concerning the frequency of the wind include all the period 1866 to 1900; those concerning the velocity only the years 1876-85 and 1891 to 1900, for previous to 1876 the velocity was recorded, making use of Mannheim's scale; from 1876 to 1885 the anemetrograph of Brusotti-Parnisetti was used, and the registers gave the kilometric velocity in the hour preceding the moment of observation. In 1891, finally, the registering anemometer of Brassart was adopted, and in the registers was again entered the hourly velocity as in the decennium 1876-85; one has, thus, the elements for deducing both the mean hourly velocity and the mean sum of the kilometres recorded in a day for every direction and for every month. From the tables compiled

from these observations one sees that during the four months November to February the most frequent winds are the N. and N.E., which are the cold winds, and then the S.W. In the other months the S.W. takes the first place and is followed by the N.E. in March, October, and November, and by the W. in the months from April to September. The least frequent winds in every period are the N.N.W. and the E.S.E., and especially the S.S.E. If, instead, one considers the frequency of the same wind in the different seasons, for the winds of the first quadrant (north-east) it decreases from winter to autumn, from autumn to spring, from spring to summer. Precisely the contrary happens for the winds of the third quadrant (south-west). For those of the other two quadrants the minimum frequency is, as a rule, verified in the winter, save for the E. and N.N.W., which blow more frequently in the winter. The regularity with which the frequency of the winds of the first and third quadrants vary during the year is remarkable. In both cases one has a curve with a single maximum and a single minimum, the one convex towards the axis of the seasons and the other concave. Consequently for the winds of the first quadrant one has a minimum in July, and for those of the third a maximum in the same month. The frequency of the winds of the fourth quadrant, and especially of the second, varies within very restricted limits. With regard to the velocity of the winds, it is greatest in the winter, above all in March, and the maximum number of calms are met with in September. The minor number of calms appertain, instead, to the direction W.S.W., the annual mean velocity of which is the maximum of all (6·5 km. per hour), as is also its monthly velocity in January (11·4 km.) The maximum velocity recorded in the above-mentioned periods also belongs to the same direction and reached 64 km. on January 20, 1881. The north and north-east winds which are, as we have said, the most frequent winds from November to February, have respectively during this period a mean velocity of 2·4 km. and 4 km. per hour. In the table on page 21 are given the mean hourly velocities of the individual winds for every month and the year, as well as their maximums and minimums.

Atmospheric Pressure.—The annual mean atmospheric pressure at the Royal Observatory is 748·55 mm. The maximum occurs in the winter, more frequently in December than in January, and more frequently in the latter month than in February; the minimum is met with in the first quarter of the year, and especially in January, then comes March.

MEAN HOURLY VELOCITY, IN KILOMETRES (MONTHLY AND ANNUAL), OF THE INDIVIDUAL WINDS, THEIR MAXIMUMS AND MINIMUMS. FROM 1876 TO 1885 AND 1891 TO 1900.

	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.W.W.
January	2·53	3·83	4·75	3·31	2·73	3·04	2·73	4·37	3·81	5·53	5·68	11·48	4·64	2·74	3·07	2·59
February	2·09	4·51	4·88	5·65	3·18	2·29	2·64	3·09	1·80	4·00	4·98	6·28	4·12	5·44	2·61	2·86
March	3·18	6·12	5·31	4·83	1·84	4·48	4·21	3·69	3·90	4·58	5·67	6·46	5·69	6·67	3·93	5·03
April	1·14	2·14	3·43	3·59	1·98	2·74	2·47	3·25	2·88	4·92	6·33	8·72	5·22	5·24	3·54	1·20
May	2·46	2·78	2·27	2·06	1·37	1·48	2·04	2·55	1·59	3·11	4·94	5·06	3·84	4·20	4·06	4·00
June	1·54	1·59	3·03	2·22	1·15	2·86	1·48	1·03	1·31	3·37	4·94	6·51	3·36	4·14	3·22	3·36
July	1·47	2·07	2·68	1·46	1·32	2·58	1·73	1·37	1·91	3·26	4·32	4·85	3·53	2·90	3·54	0·69
August	1·16	1·49	2·28	1·13	0·79	1·72	0·90	0·80	1·85	4·38	5·32	5·52	3·21	2·59	1·98	0·30
September	0·84	1·10	1·32	1·94	1·24	2·42	1·32	1·12	1·76	2·94	3·93	4·02	2·70	3·17	1·95	1·93
October	1·48	2·45	2·32	1·96	1·75	2·15	2·33	2·93	1·93	3·29	4·52	5·12	2·89	3·76	2·91	2·81
November	2·37	3·38	3·69	3·25	2·29	3·64	3·19	3·56	2·12	3·55	5·74	6·64	4·24	3·81	3·26	2·54
December...	2·66	4·28	4·69	4·32	4·22	3·75	4·89	3·18	2·39	6·77	6·61	7·40	5·41	3·97	2·49	3·63
Year	1·91	2·98	3·39	2·98	1·99	2·76	2·49	2·58	2·27	4·14	5·25	6·50	4·07	4·05	3·05	2·58
Maximum velocity ...	3·18 (Mar.)	6·12 (Mar.)	5·31 (Mar.)	5·65 (Feb.)	4·22 (Dec.)	4·48 (Mar.)	4·89 (Dec.)	4·37 (Jan.)	3·90 (Mar.)	6·77 (Dec.)	6·61 (Dec.)	11·48 (Jan.)	5·69 (Mar.)	6·67 (Mar.)	4·06 (May)	5·03 (Mar.)
Minimum velocity... ...	0·84 (Sept.)	1·10 (Sept.)	1·32 (Sept.)	1·13 (Aug.)	0·79 (Aug.)	1·48 (May)	0·90 (Aug.)	0·80 (Aug.)	1·31 (June)	2·94 (Sept.)	3·93 (Sept.)	4·02 (Sept.)	2·70 (Sept.)	2·59 (Aug.)	1·95 (Sept.)	0·30 (Aug.)

Water Supply.

Up to 1885 the water supply of Naples was derived from the Acqua della Bolla coming from the slopes of Vesuvius, and from the aqueduct of Cannignano, constructed in 1525. By reason of the impurity and poverty of this supply, however, the necessity was felt for a long time of introducing a pure drinking water into the city. Even in the times of Don Pietro di Toledo (1523-54), owing to the study of the remains of the Roman aqueducts which supplied Benevento, the principal towns of Campania Felix and the numerous villas of Posilipo, Nisida and Baia, the idea occurred to the engineer Lettieri of providing Naples with the water of the Serino; subsequent to this period many discussions took place regarding this prospect, but it was not till 1882 that a definite scheme presented by the water-works company to the municipal authorities was approved by the latter. And in March, 1885, the aqueduct was solemnly inaugurated with the distribution of the Serino water in the city, which has greatly contributed to render it the most healthy city in Europe regarding the mortality from zymotic diseases.

Among the springs which are very numerous in the little valley of Serino in the Apennines, about 60 km. from Naples as the crow flies, two were selected for the supply—namely, that of Acquaro, 373 m. above sea-level, and that of Urciuoli, 320 m. above sea-level. These two sources, united together at Urciuoli, provide a constant supply per day of 170,000 cub. m. of water. In carrying out this work due care was taken to separate completely the spring from the surface waters, thus admitting only the spring waters into the reservoir. The aqueduct, 60 km. long in its first part, extends from this reservoir to the hills of Cancello (242 m. above sea-level), about 20 km. from Naples. It has an area of 1·81 sq. m., and a depth beneath the soil of at least 1 m., thus permitting the water to preserve the constant temperature of about 53° F., which it has at its source. In this part of the aqueduct, which is in masonry, it has been necessary to construct, in virtue of the nature of the ground, twenty aqueduct bridges, two reversing syphons, and, inasmuch as between the discharging reservoir at Serino and that of reception at Cancello there results an excess of discharge of 78·4 m., three falls have been intercalated, of which the one at Cancello is 37·4 m. At this fall ends the aqueduct of free carriage and begins that of forced carriage, consisting of three enormous syphons, one 700 mm. in diameter, and the other two of 800 mm. in diameter. That conveys the water directly to the reservoir at Scudillo, and has a length of 22 km.; these,

having a length of 18 km., convey it to the reservoir of Miano. From here an aqueduct in masonry, about 2 km. long, conveys the water to the reservoirs at Capodimonte, near the Royal Observatory.

In a forced distribution it is necessary to construct reservoirs having a store of water, and, above all, for regulating the working of the network of distribution in the different phases of consumption. Hence the idea was carried out of having two divisions of distribution—one for supplying the higher parts of the city, and the other for the middle and low parts. Two reservoirs have been constructed—namely, that of Scudillo and that of Capodimonte. The former consists of three parallel galleries excavated in the tufa, and has a capacity of 20,000 cub. m.; the latter consists of five parallel galleries also excavated in the tufa, and has a capacity of 80,000 cub. m.

The distribution of the water in the city is made by means of three main pipes which start from the Scudillo reservoir, and two main pipes which start from the Capodimonte reservoir. From these five main pipes depart all the secondary ones, which, taken as a whole, form a length of 210 km. for the city and the suburbs. All these pipes communicate in such a way that they form two closed networks so as, in case of rupture, to assure a constant supply in all the parts of the city, excepting in the division where the rupture has taken place. The network of the high service is united with that of the low by means of special cisterns which discharge the water into the latter.

At the present time, besides the city and the suburbs, about forty of the communes of the Province of Naples make use of this water. This enormous work has cost 37,000,000 fr., which has been well spent, for it provides Naples and its neighbourhood with, as has been proved by frequent chemical and bacteriological analyses, an extremely pure water, very agreeable to the palate and deliciously cool even in the summer months.

The Sewerage.

The old sewerage of Naples corresponded to fifty-four secondary basins into which the whole surface of the city was ideally subdivided, each one served by a main sewer which discharged itself into the bay between Maddalena in the east and Mergellina in the west. The sewers formed a network of about 150 km. in extent, and were constructed gradually with different sections and different falls. Consequently in many parts they caused stagnation of the sewage and contamination of

the subsoil. The programme for the new system of sewerage was as follows: To cut off from the city the surface water coming from the surrounding hills; to free from the urban lido all the sewage, and to utilize this for the fertilization of the land; to prevent infiltration and contamination of the subsoil.

After many studies and warm discussions the following general plan was established by the municipal authorities, and has been in great part completed: (1) To separate the inhabited part of the city from the surrounding campagna by means of two canals, called the "collectors of the hills," which would follow the perimeter from the custom-house at Posilipo, Castel St. Elmo, Capodimonte, Arenaccia, to the Piazza della Maddalena. These collectors, being destined to convey only the rain-water from non-inhabited zones, could discharge themselves into the sea at the eastern and western extremities of the urban lido. (2) To convey all the lurid liquids to the plain between Licola and Patria, which is the most suitable for a sewage farm, raising its level by means of the sand which is to be obtained from the high dune that separates it from the sea. And therefore to construct a large emissary, called the *Emissario di Cuma*, which provisionally would discharge itself into the sea near Cuma. In order that this emissary, about 16 km. in length, should not be too large, a second one was considered necessary, which would serve for the storm-water, and in cases of rupture could substitute the Cuma emissary. This canal, called the *Scaricatoio di Coroglia*, having to convey the overflow of the Cuma emissary only during very heavy storms, could discharge itself into the sea at the extremity of the promontory of Posilipo — that is, beyond the bay proper.

In relation to these emissaries were co-ordinated all the urban sewers which reached the west end of the city at Piedigrotta, where the Cuma emissary begins at, 12·4 m. above sea-level, a height which was necessary for giving this the desired fall. As, however, only the drainage of the higher parts could naturally reach this level, it was found necessary to raise mechanically the sewage of the lower quarters, and, owing to the great expense which otherwise would be incurred, to permit the surface water to discharge itself into the sea along the urban lido.

Hence the whole of the city (about 900 hectares) was divided into three zones—namely, the high, middle, and lower zones. The *high zone* (354 hectares), of a height exceeding 22 m. above sea-level, is drained by the *promiscuous system*. All the sewage and the rain-water of this

zone are collected in a single main sewer, called the high collector, which discharges itself into the Cuma emissary. On occasion of very heavy rains part of the sewage of this collector, by means of suitable communications, discharges itself into the Coroglio emissary and, in cases of exceptional storms, also into the sea along the urban lido, when, by reason of the great dilution of the sewage, it cannot be considered injurious.

The *middle* zone (176 hectares), of a height which varies between 22 m. and 12 m. above sea-level, is drained by the *separate system*. The sewers are of double section, the upper destined for the storm-waters and the lower for the sewage proper. This discharges itself into the upper section of the *middle collector*, which has a level of 4·5 m. at Piedigrotta, where it is pumped into the Cuma emissary. In case of the breaking down of the pumping apparatus the sewage can discharge itself into the Coroglio emissary, which begins at Piedigrotta, at the same level (4·5 m.). The rain-water collected in the lower section of the *middle collectors* and in the *urban pluvial collectors* discharges itself ordinarily into the Coroglio emissary and into the sea at Maddalena, and, only in cases of exceptionally severe storms, also along the urban lido through suitable drains.

The *lower zone* (238 hectares), of a height inferior to 12 m., is drained by the *separate system*. The sewers of this zone are likewise of double section and unite at Marinella, where two pumping apparatus pump the sewage into the lower section of the middle collector. Another pumping apparatus is employed for the sewage of the low-lying quarter of Mandracchio (about 6 hectares). The rain-water of this zone discharges into the bay at the mouth of the Sebeto and along the Riviera di Chiaia.

The cost of the whole project, when completed, will be 23,000,000 fr. Since June, 1896, the high collector, with its affluents and the Cuma emissary, have been working very satisfactorily both from the hydraulic and sanitary points of view, as has been proved by numerous experiments.

Vital Statistics.

The population of Naples, estimated to the middle of the year 1908, was 593,146, and the birth-rate in the decennium 1897-1906 was 29·4 per thousand persons living per annum, which is higher than that of London (27·7) in the same period. Inasmuch as the universally recognized criterion of the healthiness of a given town is the general, infant, and

zymotic mortality, we shall now proceed to compare the general, infant, and zymotic mortality of Naples with that of other large cities.

The death-rate (all causes) per thousand persons living in the decennium 1897-1906 was at Naples, 23·5; Rome, 19·1; Paris, 18·6; Berlin, 17·3; Vienna, 19·5; St. Petersburg, 25·0. Hence it will be seen that in this period the Naples death-rate was higher than that of any of these cities, except St. Petersburg. This comparatively high death-rate is due, as we shall see, not to a high zymotic mortality, but to the high mortality from chronic diseases in the poorer quarters of Naples, depending mainly upon poverty, overcrowding, and inanition, factors that do not exist in the west end of the city, which from every point of view is the only part suitable for invalids, and forms practically a separate town.

The death-rate of infants under one year of age in the decade 1897-1906 was at Naples 155 per thousand births. This death-rate is lower than that of any town in England over 200,000 inhabitants, except London (148) and Bristol (137). The other English towns vary from 160 at Bradford to 188 at Salford.

The death-rate per thousand from the six principal epidemic diseases in the years 1902-1907 was in Naples, 0·45; Rome, 0·84; London, 1·18; Paris, 0·61; Berlin, 0·65; Vienna, 0·82; and St. Petersburg, 3·03. The foregoing figures show that in this period Naples had the lowest zymotic mortality of any of the cities mentioned. Therefore, the result of this investigation is that Naples has a comparatively high general mortality, a low infant mortality, and a very low zymotic mortality.

Perhaps the most striking way to demonstrate the great hygienic improvement which has taken place in Naples since 1878-83 is to contrast the zymotic mortality of this period with that of 1902-07. Well, the former was 3·7 per thousand, and the latter, as we have seen, only 0·45 per thousand. In the same periods the London zymotic mortality was respectively 3·30 and 1·18 in the thousand. The phenomenal reduction in the Naples zymotic mortality has been due to the introduction of the new water supply and of the new sewerage system, to the demolition and widening of the overcrowded, narrow, and tortuous streets in the old part of the city near the harbour, and to improved hygienic regulations. And it is all the more remarkable when one takes into consideration the very large proportion of poor people living in east Naples, their habits, extreme poverty, and overcrowding, which is said to exceed even to-day that of any other large city in Europe.

Naples as a Winter Station.

For a given place to claim for itself the title of a climatic station, it is necessary that in it a delicate person can be out of doors with comfort during the greatest possible number of hours in the day. This condition can be fulfilled only when one has a clear sky, a moderate temperature, and comparative freedom from cold and high winds. To these must be added the personal susceptibilities of the patient, as to the form of the climate, and the ability of the physician in obtaining his confidence and directing the treatment. In deciding upon a health-resort the water supply, hygienic conditions, &c., should be also taken into consideration.

Now, so far as Naples is concerned, we have seen that it is a healthy city, above all regarding infectious diseases, that it possesses a moderate temperature and a dry climate, that the sky is very often cloudless, and that there is a comparative absence of cold and high winds, especially in the western and healthiest part, which is from every point of view the most suitable for invalids. With regard to the other factors we have also seen that the water supply, which is on the constant system, is very pure, clear, agreeable to the palate, and deliciously cool even in the summer months; and we may add here that the accommodation and cuisine in, and the sanitation of, the best hotels and pensions are very good.

We shall bring this paper to a close by offering some suggestions to those invalids who intend wintering in Naples, and indicating briefly the maladies which are benefited by its climate. The most suitable time for an invalid to arrive for the winter in Naples is towards the end of October or early in November, and he or she can remain till the middle of May. The next question to decide is the residence—whether it is to be in an hotel, a pension, or a private apartment. Inasmuch as it is rather difficult to find suitable apartments or furnished villas at Naples, there is no doubt that an invalid will, as a rule, be much better off in a good hotel or pension. There are plenty of these establishments in Naples, either facing the sea or on the hills, where, as we have said elsewhere, the accommodation, &c., are good. From nearly all of them which have a southerly aspect one has charming views of the bay, Vesuvius, Capri, and Posilipo; and they are within from two to ten minutes' walk of the beautiful public gardens which skirt the lido for over half a mile, and are called the winter gardens of Naples. Here even during the coldest months an invalid can frequently sit or saunter about for hours, breathing the sea air and enjoying the sunshine

and the glorious view of the bay; or, if the sun be hot, one can sit under the shade of the ilex trees which clothe a large part of the gardens, and equally breathe the sea air and enjoy the view.

It is absolutely necessary for an invalid to have an apartment with a southerly aspect, and, unless there is central heating, as in all the best hotels, with fireplaces or stoves in the rooms, so that the air can be heated when it falls to 60° F., which it often does during the short winter of two or three months. The bedroom windows can be left open at night unless for some special reason it is forbidden by the doctor.

With regard to the rules for the daily routine of an invaiid at Naples, it is impossible to give details here. Consequently, the best course for him to adopt on his arrival is to consult his physician. One can say in a general way, however, that a delicate person should not go out before 10 a.m. or 11 a.m. during the coldest months, and that he should be always in the house before sunset, since at that hour, owing to the cessation of the solar radiation, the lower stratum of air becomes chilled for some little time. He should spend the warmer hours of the day in the public gardens or driving in the Campagna.

Inasmuch as the winter climate of Naples may be taken as an example of a dry, temperate climate where, to quote Dr. Huggard, "the heat demand or demand for tissue changes is moderate in amount and regular," it is most suitable for the organisms which lack the power of response that would be required in a colder climate such as that of the United Kingdom. Therefore the persons who are most likely to benefit by wintering in Naples are: the generally delicate; the aged and the worn-out; those slowly recovering from acute diseases; those suffering from anæmia, chlorosis, scrofula, and rickets; those suffering from catarrhal affections of the mucous membranes, more particularly of the throat, bronchial tubes, and intestines; those suffering from torpid phthisis, emphysema, and bronchial asthma; from chronic Bright's disease, diabetes, rheumatism, and gout in the asthenic form. Naples is also a suitable climate for neurasthenic patients and nervous, overworked, and excitable persons who cannot sleep well and require change of air and surroundings.

Balneological and Climatological Section.

December 8, 1909.

Dr. Leonard Williams, President of the Section, in the Chair.

The Chiltern Hills and Dales in certain of their Natural and Medical Aspects.

By S. D. Clippingdale, M.D.

When a member of the House of Commons, wearied by his legislative labours, wishes, while Parliament is in Session, to retire, he does so by putting into operation the ancient practice known as "Applying for the Chiltern Hundreds." If his application is granted—and it commonly is[1]—he becomes Steward of the three hundreds of Desborough, Burnham, and Stoke, and for the very moderate salary attached to that office—viz., £1 per annum—he is expected to drive from the peaceful recesses of the Chiltern Hills certain dangerous robbers believed to infest them. If, instead of looking after the thieves, who are better dealt with by the local police, he went to reside in the district, he would find little use for his punitive powers and much to restore the "æquanimitas" he has lost by his devotion in the Legislature of his country.

The Chiltern Hills

(cealc, Ang.-Sax. = chalk) are a range of chalk downs lying to the W., N.W., and N. of London. In shape they form an irregular quadrant, the mean radius of which is about thirty miles from the centre of the Metropolis. The S.W. extremity of this quadrant is at Streatley, in

[1] It was refused to a member in 1842.

Berkshire; the N.E. extremity at Luton, in Bedfordshire. Between these extremities the range of hills passes through the counties of Oxford, Buckingham, and Hertford. It is only with that portion contained within the County of Buckingham that this paper deals. The length of the entire range is some seventy miles. The breadth varies from the narrowest limits to a span of about twenty miles. The slope of the hills varies; towards the south-east they slope gradually until they merge in the valley of the Thames; towards the north-west they end in a rather bold escarpment overlooking the Vale of Aylesbury.

In addition to the main group, there are certain detached ridges and isolated hills. For instance, there is the Bledlow Ridge to the west, and to the north-east the range known as the Ivinghoe Hills, consisting of Beacon Hill, Gallows Hill, and Steps Hill. To the north are the isolated hills of Quainton and Brill; to the south-west are the Fingest and Ibstone hills; while to the extreme west is Dashwood Hill at Stokenchurch. The great healthiness of the district is, no doubt, due in part to the fact that there is no higher ground between the Chiltern Hills and the east coast; so that air coming from the North Sea passes north of London, and so reaches the Chilterns cold and pure, although it drops its salt on the way.

Gaps.

This range of hills is broken by six passes, or gaps, which, taken in order from south to north, are as follows: (1) The gap through which the Thames passes between Goring and Streatley; (2) the gap giving passage to the new Oxford Road through Beaconsfield and Wycombe; (3) the gap separating Bledlow Ridge from the main group and giving passage to the road from Wycombe to Princes Risborough; (4) the gap running transversely from east to west, giving road accommodation between Missenden and Ellesborough; (5) the gap formed by the Misbourne Stream, and which accommodates both the road and the railway to Wendover; and (6) the gap through which the North-Western Railway passes between Ivinghoe and Tring.

Rivers.

The valleys between these hills are, for the most part, devoid of water, but there are three small streams which arise in the north and west and converge towards the south and east. These three streams are the Chess, the Misbourne, and the Wye.

(1) The *Chess* arises a little above Chesham by two heads—one, a spring in the grounds of "The Bury," a local estate, the other a spring at Higham Mead. After a course of eight miles, which takes it through the parishes of Flaunden and Chenies, it joins the river Colne at Rickmansworth. Soon after leaving Chesham it receives the effluent from the sewage farm of that town. The *fall* of this river is about 1 in 326; its *velocity*, measured at Chesham in dry weather, was found to be about 1 mile in 3½ hours.

(2) The *Misbourne* arises at Great Missenden, and after a course of fourteen miles, during which it passes through Amersham and the Chalfonts, it, like the Chess, enters the Colne, but nearer Uxbridge. The *fall* of this river is about 1 in 221, and its *velocity*, measured at Amersham in dry weather, was found to be about 1 mile in 3¼ hours. The Misbourne occasionally deserts its normal channel and finds a vicarious passage through the chalk. The change of channel is said to take place about once in six years. The periodic disappearance from sight is thought by some to have created the name "Missingbourne."

(3) The *Wye*, or *Wycombe Stream*, arises at West Wycombe, and, receiving a tributary from Hughenden, flows through the town of High or Chipping Wycombe, then onwards to Loudwater, where it makes a bend to the southward, and enters the Thames at Hedsor. Its *length* is about nine miles; its *fall* about 1 in 209; and its *velocity*, measured at High Wycombe in dry weather, about 1 mile in 1½ hours. In its upper part it receives the effluent from the Wycombe Sewage Farm, apparently without detriment, as edible trout are still caught in the stream; nor has the Thames Conservancy complained of the condition of the river when it enters the Thames.

As to the influence upon health of these three streams, it will be noticed in the first place that each is a remarkably sluggish stream. On the banks of each there are mills or factories, which create a certain amount of pollution, necessitating a corresponding amount of purification, and this purification is a fertile source of contention between the various local sanitary authorities and the various mill and factory owners and other riparian dwellers. It is to be presumed, therefore, that where these rivers pass near or through towns they will eventually require covering over, as has been found necessary with similar streams near London.[1]

[1] *See* "West London Rivers Extant and Extinct," by the author, *Journ. of Balneol. and Climatol.*, Lond., 1909, xiii, p. 93.

Geology.

If a geological map of England is examined it will be seen that the whole of the south-east of England is marked chalk. This chalk forms two ridges enclosing between them the valley of the Thames. The southern of these two ridges constitutes the North Downs, the northern the Chiltern Hills. The chalk forming the Chiltern Hills is not a homogeneous mass, but is divisible into layers. Roscoe[1] describes these layers thus: "A lower division of marl, a middle division of massive chalk, with a few flints and with a hard nodular base known as the Melbourne Rock, and an upper division in which the flints abound, and which has a band known as the chalk rock at its base." The thickness of the chalk is about 800 ft. The chalk comes into view everywhere —in road-cuttings and quarries, and also in the ancient crosses cut through the turf at Ellesborough and at Bledlow. The chalk being soft, the hills made of it have been easily moulded by the weather; hence their beautiful undulating form. A good supply of drinking water has, of course, always been obtainable without sinking very deep wells, and is still so obtained, although the mains of the various water companies are gradually superseding private wells. The summits of the hills are, of course, devoid of water; and here, to effect storage, "dew ponds"[2] have been formed. These dew ponds are excavations in the chalk and lined with clay. They are from 30 ft. to 40 ft. in diameter and from 5 ft. to 6 ft. deep. The water they contain is collected from rain, dew, and mist. It never evaporates, as, on account of the elevation of these ponds, the condensation is always greater than the evaporation. The water of these ponds is intended for cattle, but, unfortunately, is occasionally taken by the villagers, with adverse consequences, as will be shown later.

Although chalk is the chief material of which the hills themselves are formed, other geological formations are to be found in the valleys in the immediate neighbourhood. Thus the town of Aylesbury stands on a bed of Portland rock; Purbeck strata are found at Brill and at Quainton; greensand at Aston Clinton; Oxford clay and fluviatile gravel in some of the valleys; and Woolwich and Reading beds on the south-east, where the hills slope to the river Thames.

The "Glacial period" has left its evidence only in the valleys. It is thought that the great ice-wave never reached the summits of the hills.

[1] Roscoe, "Methuen's Little Guide to Buckinghamshire."
[2] "Dew Ponds," *see* article in the "Victoria County History."

Orography.

The elevation of the principal hills is shown in Table I. It will be sufficient here to state that the greatest height is obtained by Coombe Hill, near Wendover, which obtains an elevation of 852 ft. above sea-level. Next in order comes Boddington Hill, with an altitude of 840 ft.; then Haddington Hill (828 ft.), Ivinghoe Beacon (811 ft.), Bledlow Ridge (759 ft.), and Quainton Hill (610 ft.). The highest villages are Stokenchurch (724 ft.), Brill (649 ft.), and Penn (554 ft.). Other orographical details will be found under other headings.

Scenery.

What, perhaps, may be described as the "charm of the Chilterns" exists in the scenery, and in describing this no better words can be used than those used by Mr. Green in his sketch of John Hampden: "The quiet undulations of the chalky country, billowy, heaving and sinking as of some primæval sea suddenly hushed into motionlessness; soft slopes of grey grass or brown-red corn falling gently to dry bottoms; woodlands flung here and there in masses over the hills. A country of fine and lucid air, of far, shadowy distances, of hollows tenderly veiled by mist; peaceful everywhere, with a flowing unaccentuated grace, as though Hampden's own temper had grown out of it."

Fauna and Flora.

These subjects can only be briefly referred to, the former on account of its paucity, the latter on account of its prolixity.

Fauna.—There is no difference between the lower animals of this and those of any other region. The birds, on account of the density of the woods, flourish well, especially the nightingale, which is found in large numbers on the south-west slopes of the hills. Nor would it be decorous to omit reference to the "Anserinæ" which are bred to a large extent in this neighbourhood, and which as "Caneton d'Aylesbury" form so welcome an item in the menu of the dinners which a scientific society finds it so necessary to hold. The appreciation of these birds is an ancient matter, for the manor of Aylesbury was formerly held by the tenure, *inter alia*, of supplying two of them annually for the royal table.

Flora.—Mr. Druce,[1] in his excellent treatise, after lamenting that the flora of Buckinghamshire has never been properly studied, goes on to enumerate a modest three or four hundred specimens with which he is personally acquainted. Here it must suffice to mention (1) the *Gentian*, which thrives well in the district and is to be found in the garden of a distinguished physician at Wendover; (2) the *Poppy*, which, as will be shown later, is grown for nefarious purposes; and (3) the *Beech*, a tree which grows well on a chalky soil, is a characteristic feature in this district, and forms material for the local staple industry—namely, chair-making.

Afforestation and Deforestation.—The beech trees have been cut down on various occasions and for various purposes. The old monks cut them down in order to dislodge the thieves;[2] James I cut them down to make ships for the Royal Navy; and for the last two centuries they have been cut down to supply the well-known Wycombe chairs and tables. The denudation caused by the latter industry has naturally called for re-afforestation, so that during the last ten years (1895 to 1905) the wooded area in the district has been increased from 29,421 acres to 34,548 acres.[3]

Meteorology.

In a district so acutely diversified by hill and dale, meteorological phenomena are naturally marked. This is especially so in regard to—

Rainfall, which, as in other hilly districts, is above the average, the reason being, of course, that the rain-clouds, being driven up the hill-sides, condense and discharge their contents. The average rainfall for certain places in this district is given in Table III.

Thunderstorms are said to be common, especially what may perhaps be termed "dry thunderstorms," in which electrical discharge is unaccompanied by rainfall. This district, therefore, might be a suitable one for the study of "magnetic storms" and the theories connected therewith of Sir Oliver Lodge and Professor Gregory.

[1] Druce, G. Claridge, M.A., F.L.S., in Roscoe's "Guide," and in the "Victoria County History."

[2] The thieves remained, however, notwithstanding the demolition of their haunts necessitating the "Stewardship of the Chiltern Hundreds," an office endowed with the power of life and death. So late as the middle of the seventeenth century it used to be said, "If you beat a bush you will dislodge a thief" ("Drayton's Polybion").

[3] "Victoria County History."

Humidity, Sunshine, and Prevailing Winds. — Inquiry at the Meteorological Office reveals the perhaps unfortunate circumstance that there are no local records of the above important conditions. With regard to *sunshine*, however, certain data recorded at that office go to prove that the amount of sunshine in the Chiltern district must be some 350 to 400 hours in excess of that for London; in other words, about one hour more each day.

Fog.—It is to be regretted that this offensive attribute of the Metropolis, hitherto unknown in the Chiltern district, has of late years made its appearance there, its advent being, no doubt, due to increased population and to the new railways. As in London, the month selected by fog is November, and, as in London also, the fog is often thicker on the hills than in the valleys, the reason being that fog, although it arises in the valleys, in sweeping up the hills comes into contact with a layer of cold air, is kept down, and so made more dense.

Frost and the other rigours of winter, as might be expected, are keenly felt in this district.

Ethnology.

An "Ancient British Outcrop." — With regard to the native inhabitants of this district, an interesting point has to be noted. Local observers have long been acquainted with the existence, in the more remote regions, of a class of persons always associated with the oldest native families, who have certain marked peculiarities—namely, a rather stunted growth, a swarthy complexion, and a reserved manner. Roscoe, for instance, says: "There is a marked difference between the inhabitants of these hills and those of the open country, the former being darker and shorter, and more Celtic in appearance. This difference, however, is gradually disappearing with intermarriage." Locally, these persons have been regarded as of Romany origin, without any attempt to explain how a nomadic tribe, of Eastern origin, could reach and remain secluded among these hills.

For the purpose of this paper, inquiry was made at the Royal Anthropological Institute, and Mr. H. S. Kempsford, the assistant secretary, has been so good as to furnish what is, doubtless, the accurate explanation. Mr. Kempsford, in his letter to the author, states that the native inhabitants of the Chiltern Hills are to be regarded as "an ancient British outcrop"—that is to say, they are the descendants of the aborigines who were never dislodged from their recesses by

invaders, either Roman, Saxon, or Danish. Dr. John Beddoe, also, has been good enough to supply the following information. His letter is as follows: "The Chantrey, Bradford-on-Avon, April 30, 1909.—I have done no work in the Chilterns myself, nor do I know whether anyone else has; but it is probable that the Chilterns retain more of the Celtic, Iberian, or other primitive stock, than, for example, the Vale of Aylesbury, or the Vale of the White Horse. The latter we know, from the *Saxon Chronicle*, were conquered, and no doubt colonized, from the Western side. I found more dark eyes and hair in Dunstable than in Aylesbury."

Dr. Beddoe, also, in the eighth volume of the "Memoirs of the Anthropological Society," gives the following limited, though interesting, observations made by him:—

Of 25 prisoners in Aylesbury Gaol	the average height	was	5 ft. 5·92 in.
Of 25 lunatics in the local asylum	,,	,,	5 ft. 6·15 in.
Of 13 farmers at Nash, North Bucks	,,	,,	5 ft. 8·34 in.

Whether due to their successful resistance to invasion or to their remoteness from centres of civilization, the inhabitants of this part of Buckinghamshire have always shown a determined resistance to restraint. This has shown itself at all times—in matters religious, political, and social. The student of political history will bear in mind that it was here the patriot Hampden precipitated the Civil War by his refusal to support the Royal Navy, because he did not reside at the seaside[1] (an action which would not be acclaimed as wholly patriotic at the present day), and the student of ecclesiastical history will remember that when Knox delivered his famous diatribe against Queen Mary and her creed, he did so, not at Westminster Abbey or at Paul's Cross, but in the Parish Church of Amersham, where he doubtless found a willing audience. In fact, with the exception of the "Peasants' Revolt" of the fourteenth century, every "social earthquake" which has disturbed our country seems to have found a ready echo in this region.

At the present day this love of freedom and impatience under restraint appears to manifest itself in the person of that dangerous character, the man with a conscience, and, according to the reports of the local officers of health, direful results will assuredly follow the outbreak of small-pox or some other lethal distemper.

Opium Eating.—Another undesirable peculiarity of these people is their addiction to the opium habit. Inquiry by letter on this subject

[1] Prior to Hampden's time the Navy was supported only by the coast towns.

obtained a general answer of "Non cognoscitur." In touring the district, however, abundant evidence was found. A druggist in one of the towns, for instance, stated that his firm, which had been established over a hundred years, formerly kept a box in which was stored crude opium for supply to the lace-makers. Another druggist stated, among other things, that an old woman when out on Saturday night, making her purchasers for the following week, would call regularly for her pint of laudanum. A local medical man stated that fifteen years ago it was common to observe the listless gait and the contracted pupil of the opium eater, and that patches of white poppy were still grown for making poppy tea for infants. Other observers stated that the sucking of poppy seeds and the sipping of liquid opium were still prevalent in some of the more remote regions. The custom, however, received a check by the passing of the Pharmacy Acts, and, it is hoped, will soon disappear altogether.

Economics.

To induce people to reside on the hills, higher wages were formerly offered. Miss Jamieson, in her excellent article on the subject,[1] says that in 1765 the relative scale of remuneration on the hills and in the valleys was as follows :—

Chief bailiff ...	...	£6	10	0	on the Chilterns	...	£6	0	0	in the Vale.
Ordinary servants ...		4	10	0	,, ,,	...	4	0	0	,, ,,
Boys from 16 to 20 ...		3	0	0	,, ,,	...	2	10	0	,, ,,
Boys from 12 to 16 ...		2	0	0	,, ,,	...	1	13	4	,, ,,

Curiously enough, although an inducement, by higher remuneration, was made to a man to go upon the hills, no inducement was offered for him to remain there; for if he married on the hills no increase was made in his salary, but if he remained in the valleys a rise always followed his marriage.

The repeal of the Corn Laws in 1846 did not affect the farmers on the hills, but those in the plains suffered severely from foreign competition, which could easily reach them.

Health.

The details of the morbility and mortality in this district can, perhaps, be conveniently arranged under two headings, *Past* and *Present*.

[1] Jamieson, "Victoria County History."

(I).—*Past.*[1]

In dealing with the past medical history, a topographical arrangement appears most suitable, and is adopted here.

Amersham had its pest-house,[2] but a pest-house was not always a plague-house. A pest-house was a building into which was put a person suffering from a disease believed to be infectious, but which might not have been so. In fact, a pest-house was a place into which any sick person might be put who frightened his neighbours. The pest-house was always on the outskirts of the town or village, and was often only used in time of pestilence. It was the successor of the lazaretto, or leper-house, and the predecessor of the modern isolation hospital. Amersham suffered from no plague, except a plague of religious zeal, in which children were compelled to set fire to the faggots placed round their parents to destroy them. The year of the plague (1665) was a healthy year for Amersham, there being only eleven deaths in a population estimated at 1,000.[3] Like other places, Amersham had its sufferers from the "King's Evil" (scrofula), and in the churchwardens' accounts is a copy of the certificate furnished to each patient sent up to London to be touched by the Sovereign.[4]

[1] For the purpose of this paper the following clergymen have kindly supplied information from their respective Parish Registers: Rev. F. J. Britnell (Wycombe), Rev. J. P. Britton (Lee), Rev. C. E. Boultbee (Chesham), Rev. G. B. Brown (Monks Risborough), Rev. A. S. Commeline (Beaconsfield), Rev. J. K. Cook (Weston Turville), Rev. J. S. Davis (Fingest and Ibstone), Rev. L. Fenn (Little Missenden), Rev. T. Hawley (Ivinghoe), Rev. Hon. A. K. Hobart Hampden (Great and Little Kimble), Rev. C. Macdermott (Great Missenden), Rev. B. Kerby (Penn), Rev. J. Mander (Princes Risborough), Rev. A. E. Murray (Wycombe), Rev. L. Packer (Saunderton), Rev. R. Palmer (Bradenham), Rev. J. Riley (Brill), Rev. C. E. Roberts (Halton), Rev. R. Shann (Chenies), Rev. C. D. Shaw (High Wycombe), Rev. W. T. Stubbs (Tring), Rev. S. W. Vardon (Ellesborough), Rev. A. H. Whitfield (Hughenden), Rev. C. S. A. Whittington (Chalfont St. Peter), Rev. G. E. Wilson (Great Missenden), Rev. F. J. Winterton (Stoke Mandeville), Rev. W. W. Wooler (West Wycombe).

[2] What may, perhaps, be styled the "pedigree" of these places is roughly as follows: (1) *The Lazaretto, Leper Hospital*, or *Lobe House*, from A.D. 936, when the first leper-house was established in York, to the extinction of leprosy at the end of the fourteenth century. (2) *The Pest-house*, which was sometimes a disused Lazaretto, from the end of the fourteenth to the middle of the eighteenth century. (3) *The Isolation Hospital*, from the establishment in 1746 of the first smallpox hospital until the present time. (N.B.—A new and valuable work on this subject is Miss Clay's "Mediæval Hospitals of England," Methuen and Co.)

[3] Estimated by "Rubin's Law," *see* Table IV.

[4] This certificate was as follows: "We, ye minister and churchwardens of ye parish of Amersham, in ye county of Bucks, do hereby certifie yt . . . . of ye parish aforesaid, is afflicted, as we are credibly informed, with ye disease commonly called ye King's evill, and to the best of our knowledge hath not heretofore been touched by his Majestie for ye said disease. In testimony whereof we have hereunto set our hands and seals." (N.B.—It will be observed that a prior touching invalidated.)

Aylesbury.[1]—As may be readily imagined, a town of the size of Aylesbury is not deficient in medical history. In the reign of Henry I two hospitals were founded here—one for lepers and other sick people, the other for the aged and infirm. These institutions, being of a religious nature, were abolished by Henry VIII, and were succeeded by the usual pest-house. The inmates of this pest-house did not, apparently, receive "all the comforts of home," for on one occasion, being left without firewood, they broke out and seized the nearest fences and sheep-racks, for which the corporation had to compensate the owner. The pest-house, having served the purposes of a hospital for some three centuries, was succeeded by the Bucks County Asylum, built in 1833. Aylesbury suffered severely from epidemics of smallpox. The first epidemic occurred in 1671, and was very severe and very fatal. The churchwardens' accounts show considerable expense in dealing with it; among other interesting items, payment to a man "to stop the players coming from Leighton." In 1747 the number of deaths from smallpox was seventy-seven. In 1748 there was a severe epidemic, attended with 236 deaths. In 1766 there was another epidemic, costing the corporation £40. About this time Lady Mary Wortley-Montagu introduced her system of inoculation, and smallpox disappeared from the annals of Aylesbury for some time. The gaol at Aylesbury seems to have had an unenviable notoriety for the begetting and spreading of infectious disease, such as "gaol fever," "spotted fever," "putrid fever," and "plague." John Howard, the prison philanthropist, visited Aylesbury Gaol in November, 1773, and again in November, 1774, and he found that in the period of twelve months between his two visits, although there had only been twenty prisoners, six or seven had died of gaol fever. In 1784 a deplorable incident occurred. A man was imprisoned for a £10 debt; he contracted gaol fever and died; he was visited and nursed by his wife, who also became infected, returned to her village and died, as did several of her neighbours to whom she had conveyed the infection. All children, however, escaped. The bubonic plague appears to have affected Aylesbury with singular irregularity. In the epidemic of 1603 the sickness was so great that the sheriff had to "move the County" to an adjoining town. In the epidemic of 1625 the place was so free that the vicar was able to make the following entry in the register: "About the 16 Dies in the year 1624 began the Great Plague,

[1] For the purpose of this paper much valuable information concerning Aylesbury has been kindly collected by Mr. Edwin Hollis, curator of the County Museum.

which continued till the end of Dec., 1625, and all which time there died not one of the Plague in the Town of Aylesbury. Deo Optimo Maximo Gratiæ Sempiternas, &c." In the epidemic of 1665 Aylesbury suffered severely. Deaths from plague are not specified in the register, but the mortality is shown to have increased by about 25 per cent. Thomas Ellwood, the Quaker, was imprisoned at this time in Aylesbury Gaol. Plague having entered the gaol, Ellwood petitioned for his release and was transferred to High Wycombe. His co-religionist, Isaac Pennington, was less fortunate. From religious scruples he neglected to address, by his proper titles, his custodian, the Earl of Bridgewater, who replied that unless Pennington did so he might lie in the gaol and rot.[1] The last severe epidemic to visit Aylesbury before quite recent times was an attack of Asiatic cholera in 1831-2, which carried off 60 persons.[2]

Beaconsfield, which lies on a chalk plateau 340 ft. above sea-level, and with pine forests close at hand, presents, as might be supposed, a very good bill of health. The perennial salubrity of Beaconsfield is testified to by Waller, the poet, who represented the place in Parliament. In a letter to Pitt in 1795, the year of the great famine, Waller says: "I live in a place as poor as most, but we have seen no traces of those dreadful exterminating epidemicks which, in consequence of scanty and unwholesome food, have in former times wasted whole nations."[3] It has to be stated, however, that at a comparatively recent date—namely, in 1830—Beaconsfield suffered severely from malignant scarlet fever, at that time prevalent throughout the country.[4] The pest-house of Beaconsfield was situated about a mile north of the town.

Brill is a place without medical history of sinister import. At the foot of Brill Hill is a disused spa which sought, in its day, to rival Bath in popularity, but failed on account of its hopeless inaccessibility.[5]

Chalfont St. Giles is of medical interest, from the fact that Milton went there to escape the plague of London in 1665. His sense of security, however, would not have been increased if he had been aware that, soon after his arrival, two deaths, apparently from the plague, occurred in the village. Milton arrived in July, and the following entries occur in the parish register soon after: "August 26—A stranger

[1] "Penns and Penningtons of the Seventeenth Century," by Miss Maria Webb.

[2] [3] [4] Creighton's "History of Epidemics in Britain," Lond., 1894, ii.

[5] *See* a paper by the author, "A Byegone Buckinghamshire Health Resort," *Journ. Balneol. and Climatol.*, Lond., 1907, xi, p. 39.

was buried out of ye Vatch ffamily suppost to die of ye plague. September 3—John ye son of Obadiah Heywood was buried. 'Tis supposed he died of ye sicknesse." Milton, however, was a smoker, and he would have heard that the tobacconists of London escaped plague. He smoked daily while at Chalfont, and his doing so may have acted as a prophylactic—mental or material. If Milton had died while at Chalfont, "Paradise Lost" would have been unfinished and "Paradise Regained" unwritten, for the former was completed and the latter commenced at Chalfont.[1]

Chesham.[2]—Except that there was the usual pest-house, Chesham presents nothing of medical interest until the close of the eighteenth century. Then there were two severe epidemics—one of scarlet fever in 1788; the other, of diphtheria, in 1793-4. These epidemics were carefully studied by a local medical practitioner, Mr. Henry Rumsey, who published his reports in the medical journals of the time. The two epidemics have been carefully analyzed by, and are fully dealt with by, Creighton in his work on British Epidemics.[3] In 1871 Chesham was visited by a severe epidemic of enteric fever, to which the then vicar, the Rev. A. F. Aylward, fell a victim, as recorded on his monument in the church. Chesham Church contains not one but two so-called "lepers' windows," which may or may not prove that at some bygone time Chesham had a large number of inhabitants who suffered from some form of skin disease supposed to be infectious, and who, consequently, were excluded from the interior of the church. There is a chalybeate spring at Chesham, and to attract visitors a pump-room was erected over it in 1820. The enterprise, however, failed.[4]

Great Hampden's limited rôle of sickness is notable for the sad fatality which occurred in the family of the rector, the Rev. Robert Lenthall, in the year 1647. The rector's eldest daughter, a girl aged 14,

[1] Milton is supposed to refer to his stay at Chalfont in the following lines:—

> "As one who long in populous city pent,
> Where houses thick and sewers annoy the air,
> Forth issuing on a summer morn, to breathe
> Among the pleasant villages and farms."

[2] The Rev. C. E. Boultbee, vicar of Chesham, has kindly supplied much valued information for this paper.

[3] Creighton's "History of Epidemics in Britain," 1894, ii, p. 715.

[4] Chesham Spa. The following analysis of the water is given by Creighton: In one gallon—4·056 gr. muriate of lime, 1·168 gr. silica, 1·600 gr. carbonate of lime, 0·320 solid contents; 7·144, &c.

had been on a visit to Wycombe, and on returning to her father's house showed signs of some malady, of which she died the following day. Shortly after, the rector's wife was seized with the same malady, being taken ill one day and dying the next. Then the rector's eldest son, "a hopeful young man of one and twenty," his distressed father calls him, succumbed in the same sudden way, and he was followed by a fourth victim, a nephew, aged 14. In each case the patient was taken ill one day and died the next. In each case the rector, perhaps with commendable wisdom, buried the body before nightfall on the day of the death. The whole story, described by the bereaved gentleman himself in his parish register, makes pathetic reading, and is in strong contrast with the story of the celebrated nonconformist divine, Richard Baxter, who, to escape the Great Plague of 1665, went to reside with his friend Richard Hampden at Hampden House, leaving his wife and family, as he confesses in his diary, in the midst of the plague at Acton.[1]

Great Missenden's recorded medical history does not begin until the year 1824, following a severe outbreak of smallpox. At that period medical opinion was in the balance as to the relative value of inoculation and of vaccination. At Great Missenden vaccination had been effected, apparently with impure lymph, with very serious consequences, and inoculation was returned to. Eight hundred of the villagers were inoculated, 300 by the doctor and 500 by the clergyman.[2]

Stoke Mandeville formerly stood near the now deserted church, and the remains of the ancient village may still be traced. Some pestilence, probably the plague of 1665, is said to have been responsible for its extermination, but there is no absolute evidence of this, as the parish registers for the period are wanting.

Wendover is interesting, medically, on account of its being the accredited birthplace of Richard of Wendover, physician to Pope Gregory IX. Wendover, in mediæval times, possessed a hospital, about which, however, very little is known. It was a religious institution,

[1] "Relique Baxterianæ," quoted by Creighton, i, p. 665.

[2] The whole story is thus set out in a letter from the vicar, the Rev. R. Marks, and is recorded by Creighton, ii, p. 592: "Great Missenden, May 6, 1824.—The summer I came here the smallpox was introduced and, as the weather was very hot and the confluent sort was what appeared, the people began to die almost as fast as they took the plague. Great prejudice prevailed against vaccination in consequence of the parish having some years ago been vaccinated by a gentleman who knew nothing of the matter and contaminated the people with decompo-ed virus, when it was good for nothing but to make ulcers and produced very wretched arms, and left them all liable to smallpox, which they were all inoculated for the same year; and now I have the happiness of seeing the plague and destruction of a most horrid smallpox completely stopped."

and, as such, had to share the fate of others at the Dissolution of 1539. A pest-house took its place. In sanitary matters the Wendovians have not always been above reproach. Dr. Leonard West,[1] the historian of the borough, records a state of things in 1658 which, but for the drastic action of the Town Council, might have become a very serious nuisance. With regard to the plague there is no local record, as the parochial registers for the time are missing. Dr. West, however, from other evidence, has satisfied himself that the plague did visit Wendover.[2] At Halton, adjoining Wendover, the vicar writes that in the churchyard several small tobacco pipes, such as the gravediggers used when burning persons who had died of plague, have been picked up.

Wycombe.—Wycombe, like Aylesbury, had its two hospitals—one for lepers and other sick persons, the other for the old and destitute; in other words, an isolation hospital and a workhouse. There was also a third institution called a hospital (St. John's), which was really a guest-house for strangers. All these institutions, being of religious foundation, disappeared at the Reformation, and were succeeded by the pest-house. There was also a "Hospital Chamberlain," whose duty seems to have been to administer to the medical wants of the necessitous poor at their own homes. His accounts are very carefully kept, and are in the possession of the courteous Clerk to the Corporation, Mr. A. G. Clark. From the Great Plague Wycombe suffered severely, as may be seen from the following letter, which has not hitherto found its way into any local or county history: "Wicombe Nov. 7.—I need not tell you how heavy the sickness has fallen upon this little corporation, out of which there have died 80 persons since it first seized us, but (God be praised) we are at present in hopes of a final abatement; and, under the care of a very loyal and discreet magistrate (Mr. Edward Bedder, our new Major), who deserves much commendation both for his care of all under his charge and for his remarquable zeal and good affection to his Majestie's Service."[3] The following entries in the parish registers prove the severity of the Plague:—"1665. Number buried this year, 149; the pest, 96." "1666. Number buried this year, 144; whereof the plague, 101." It is unfortunately impossible to ascertain what proportion this mortality bore to the population, as the baptismal register, from which alone a

[1] "The History of Wendover," by Leonard H. West, LL.D., Aylesbury, 1909.

[2] There is a tradition that a dog from Wendover carried the plague to Ellesborough, killing the vicar, the Rev. Thomas Avery (*see* Lipscombe's "History of Buckinghamshire").

[3] From the *Intelligencer*, November 15, 1665.

census could be estimated, is missing. Among the persons imprisoned for their faith in Wycombe Gaol in 1665 was Dr. William Parker, M.D.Cantab., M.R.C.P.Lond., a Quaker. He would have been imprisoned at Aylesbury, but his wife pleaded successfully for his removal to Wycombe, where the plague was less severe. Another point of medical biographical interest in Wycombe is that it was the domicile of Dr. Martin Lluellyn, who is said to have accompanied Charles I. to the scaffold, receiving from that monarch the gloves he wore on that occasion. Dr. Lluellyn resided in the house now occupied by Dr. Wheeler. No evidence has been recovered as to the extent to which High Wycombe has suffered from epidemics of either smallpox or scarlet fever; but seventy-five persons died there from the cholera epidemic of 1849.

With reference to other places within the scope of this paper there is no evidence, either in the parish registers or elsewhere, of any serious sickness. Before leaving the subject of past sickness in the district, it must be stated that no part of the country suffered more than Buckinghamshire from the "Black Death" of 1348 and 1349. There are no local records, but the episcopal records show the deaths of no fewer than seventy-seven of the local clergy.

(II) Present.[1]

In describing the maladies found with varying frequency in this district at the present day, a nosological arrangement can conveniently be followed:—

Anæmia, as might be expected, is not unknown in the valleys. It is often associated with goitre (q. v.).

Bronchitis is frequently met with, as in other hilly regions, and broncho-pneumonia is a common cause of death; yet the mortality from diseases of the respiratory system is only 1·7 per 1,000, as compared with 3·7 per 1,000 for the whole of England.

[1] The following medical men have kindly responded to application for information for the purpose of this paper: Dr. W. H. Hogarth (Medical Officer of Health for the County), who, at much trouble, has furnished comparative statistics; Dr. J. C. Baker (Aylesbury), Dr. W. R. Bannerman (M.O.H., Wycombe), Mr. James Berry (London), Dr. W. Bradbrook (Bletchley), Mr. Frederic Eve (London), Dr. E. H. Greaves (Amersham), Dr. T. F. Long (M.O.H., Chesham), Dr. T. G. Parrot (M.O.H., Aylesbury), Dr. C. S. Stolterfoth (Stokenchurch), Dr. A. H. Turner (M.O.H., Beaconsfield), Dr. E. O. Turner (Great Missenden), Dr. F. S. Watson (Princes Risborough), Dr. H. T. Wheeler (High Wycombe), Dr. C. G. Woollerton (Wendover).

Calculus.—Buckinghamshire was formerly third among the counties of England as to frequency of stone in the bladder,[1] the reason, of course, being the large proportion of chalk in the water. At the present time it is almost unknown, and at the county asylum at Aylesbury it is rare for a patient to be found suffering from either renal or hepatic calculus. This rarity is attributed—and no doubt rightly attributed—to the treatment by the water companies of the water they draw from the chalk, although only one of those companies — the "Chiltern Hills" company — adopts any "softening" process.

Cancer.—In every district, however salubrious, there is generally to be found some disturbing bane—some fly in the ointment of the apothecary. In the Chiltern district this bane presents itself in the form of cancer, which is everywhere common, especially in the valleys. It appears to be communal, runs in certain families, and is probably fostered by intermarriage. The most common form is carcinoma, and no organ seems immune from it. Dr. Hogarth, the Medical Officer of Health for the county, states that the mortality from cancer in the district has increased within the last twenty-three years. The same authority also gives the following comparative mortality: In the Chiltern district, 1·0 per 1,000; in rural England generally, 0·7 per 1,000.

Goitre.—As in other hilly districts, goitre is common. Mr. James Berry, senior surgeon to the County Infirmary at Aylesbury and an acknowledged authority on diseases of the thyroid gland,[2] has made the important discovery that the softening of hard water by Clark's process, while it diminishes the tendency to calculus, increases the tendency to goitre.

Helminthiasis exists in those districts where the villagers carelessly drink the unfiltered and unboiled water of the "dew-ponds" (above mentioned, p. 32) and the little streams. Dr. Stolterfoth says the condition is very common in the Stokenchurch district, the worms principally met with there being the Acarides and the Oxyuria vermicularis.

Paludism in all its forms is extremely rare. There are no marshes or fens; consequently there is neither marsh nor fen fever.

Rheumatism varies both in variety and in severity in various localities. A consensus of opinion of the medical men practising in the district appears to be that rheumatism is rather above the average

[1] Cadge, "Causes and Distribution of Calculus," *Brit. Med. Journ.*, 1874.

[2] James Berry, F.R.C.S., "Diseases of the Thyroid Gland," London, 1901.

in the valleys, rather below the average on the hills. No place seems to be wholly free from it—the arthritic variety prevailing in one place, the muscular variety in another.

Tuberculosis.—Although consumption is fairly common in Buckinghamshire generally, as in other parts of the country, it is rare in the Chiltern district, except as an imported disease. Dr. Hogarth's report as to the relative mortality is as follows: For the Chilterns, 0·6 per 1,000, for rural England 1·09 per 1,000. Dr. Bannerman, of High Wycombe, and Dr. Watson, of Princes Risborough, both state that if a chair-maker in one of the Wycombe workshops becomes infected, he does well as soon as he is put upon a farm on the uplands; and Dr. Turner, of Beaconsfield, has made the important observation that if a member of a local family, free from tuberculosis, marries into an immigrant tubercular family, and himself contracts the disease, he gets it in an acute form, and his case may make a rapid course—an example of the well-known law that a disease attacks new soil with special virulence.

Zymotic Diseases.—Although the drainage is often primitive, and sanitary precautions ignored, the mortality from zymotic disease compares very favourably with that for rural England generally, Dr. Hogarth's figures being 0·3 per 1,000 for the Chilterns, 0·92 per 1,000 for rural England.

The *General Death-rate* of the Chiltern district, as compared with that for rural England, is, Dr. Hogarth states, 12·5 and 14·7 respectively.

The *Senile Death-rate* cannot be exactly ascertained, but it is commonly admitted that longevity in this district is marked. In fact, it would almost appear that, provided a pension be secured, old age is merely a matter of the wish and the will.

Rothschild Villages.

In a paper intended to exhibit the healthiness of a district it would be wrong to attribute all to nature when something is due to man. In the northern part of the district reside certain members of the Rothschild family, whose benevolent influence does much to stamp out intemperance and lessen disease. In a typical "Rothschild village" the public-house is unknown, and the medical man goes about with an assurance from his "over-lord" that the non-medical wants of the sick poor will not be neglected—a state of things reminiscent of the feudal

system, with all its advantages but with none of its disadvantages. Mrs. Frederic Eve, who resided for some years in the district, states that the influence of the Rothschild family does not end with death, as the villagers are encouraged to keep their graveyards in order and to venerate the graves of their relatives.

In conclusion, it only remains to indicate the suitability of the Chiltern Hill district as a health resort for long or short periods. It may be a matter of surprise that, in a place so eminently suited for it, no sanatorium for consumptives exists. Some years ago the benevolent owner of "Chequers Court" proposed to erect one on Coombe Hill. His proposal, however, was not welcomed by others, who, naturally perhaps, were unwilling to introduce into the district a disease alien to it, and the project was abandoned. With the exception of the holiday homes of Miss Pennefather, at Aston Clinton, and of Miss Solomon, at Wendover, in each of which excellent work is done, there is nothing in the nature of a public convalescent institution throughout the whole district.

As a place for the week-end holiday the Chiltern district is well known, especially among London consultants, many of whom have houses there. It is a place in which almost every kind of hobby can be enjoyed at short notice; in fact, it is doubtful whether there is any region near London in which a little leisure can be spent with greater pleasure.

TABLE I.—ALTITUDE OF THE PRINCIPAL HILLS IN FEET ABOVE SEA-LEVEL.

Coombe Hill	852	Hunt's Hill	588
Boddington Hill	840	King's Hill	575
Haddington Hill	828	Baccombe Hill	568
Beacon Hill (Ivinghoe) ...	811	Beacon Hill (Penn)... ...	554
Bledlow Ridge	739	Frith Hill	544
Muswell Hill (Brill) ...	649	Bush Hill	518
Quainton Hill	610	Dawes Hill	518

TABLE II.—ELEVATION OF THE PRINCIPAL TOWNS AND VILLAGES.

Amersham	400	Hampden, Great	685
Aylesbury	275	Hampden, Little	668
Beaconsfield	344	Missenden, Great	417
Bradenham	332	Missenden, Little	357
Brill	649	Penn	554
Chalfont St. Giles	238	Princes Risborough ...	370
Chalfont St. Peter	200	Stoke Mandeville	323
Chenies	400	Stokenchurch	724
Chesham	526	Wendover	431
Chesham Bois	546	Wycombe, High	242
Gerrard's Cross	280	Wycombe, West	283

TABLE III.—RAINFALL (IN INCHES) FOR 1907 AND 1908, FROM MILL'S "BRITISH RAINFALL."

	1907	1908
Amersham	28·02	26·91
Beaconsfield	25·92	25·49
Chalfont St. Giles	28·39	27·31
Chesham	Not recorded	27·11
Great Missenden	29·71	28·71
High Wycombe	29·15	28·38
Wendover	27·89	25·54

TABLE IV.—POPULATION OF TOWNS IN PRE-CENSUS PERIOD, ESTIMATED BY RUBIN'S LAW. (*a*)

	16th Century		17th Century		18th Century
	Middle	End	Middle (*b*)	End	Middle
Amersham ...	750	990	870	1590	1500
Aylesbury ...	1290	1560	1950	2100	2400
Beaconsfield ...	(*c*)	(*c*)	780	1170	728
Chesham ...	1110	1710	1080	1590	1650
High Wycombe ...	(*c*)	(*c*)	(*c*)	1860	2760
Wendover ...	(*c*)	(*c*)	1050	1020	1110

(*a*) Mr. Marcus Rubin read a paper before the British Association in 1900, showing that a pre-census population could be roughly estimated by multiplying the number of baptisms by 30.

(*b*) In the middle of the seventeenth century baptism was largely in abeyance.

(*c*) The lacunæ indicate the absence of reliable data.

TABLE V.—POPULATION OF TOWNS AND VILLAGES IN CENSUS PERIOD, FROM REGISTRAR-GENERAL'S RETURN.

	1801	1811	1821	1831	1841	1851	1861	1871	1881	1891	1901
Amersham ...	2314	2688	3104	3313	3645	3662	3650	3259	3001	3129	3209
Aylesbury ...	3126	3497	4400	5021	5429	6081	6168	6962	7793	8680	9099
Beaconsfield ...	1048	1461	1736	1763	1732	1684	1662	1524	1635	1773	1570
Bradenham ...	170	181	220	263	226	138	185	169	183	152	154
Chalfont St. Giles	762	824	1104	1297	1228	1169	1217	1243	1264	1286	1362
Chalfont St. Peter	1174	1153	1351	1416	1483	1482	1344	1459	1456	1509	1753
Chenies	423	510	595	649	625	565	468	495	388	378	324
Chesham	3962	4441	5032	5388	5593	6098	5985	6488	6502	8018	9005
Chesham Bois ...	910	130	160	157	218	185	218	258	351	532	767
Ellesborough ...	480	469	581	665	708	782	724	703	608	661	577
Hampden, Great...	228	235	281	286	290	308	266	262	233	246	207
Hampden, Little...	79	69	88	103	83	73	68	61	46	76	48
Hughenden ...	887	989	1247	1457	1481	1541	1653	1792	1603	1765	1725
Kimble, Great ...	316	319	360	436	469	501	408	409	422	395	343
Kimble, Little ...	142	143	165	176	177	184	182	283	161	170	149
Missenden, Great	1411	1576	1735	1827	2225	2097	2250	2278	2170	2385	2166
Missenden, Little	625	678	937	937	1011	1142	1089	1148	1113	1136	1112
Penn	927	950	586	647	744	704	811	1028	1063	1029	1056
Risborough, Monks	768	899	834	1018	1083	1064	985	938	847	810	714
Risborough, Princes	1544	1644	1958	2122	2206	2317	2392	2549	2418	2318	2189
Stoke Mandeville	248	341	402	461	293	535	477	528	497	480	411
Wendover... ...	1397	1481	1602	2008	1877	1937	1932	2033	1902	2036	2009
Wycombe, High ...	4248	4756	5399	6299	6480	7179	8373	10491	13154	16409	19282

Atmospheric Electricity.

By GEORGE MAHOMED (Bournemouth).

FOR a century and a half scientific men have been aware that the atmosphere maintained a definite though varying electric condition. One of the earliest experiments was remarkable for its ingenuity and simplicity. An arrow was attached to a metallic string, the other end of which was joined to an electroscope. When the arrow was discharged by a bow towards the sky the pith balls in the electroscope diverged, and diverged more the higher the arrow ascended. From this the observer inferred that the atmosphere was positively electrified, and that the potential was higher the further from earth. A later experimenter insulated a metal sphere on a rod from the roof of his house. Having earthed it for a moment, again exposing it to the atmosphere, and then conveying the sphere by an insulated carrier to his laboratory, he calculated the amount of charge it had received. One gentleman, M. Richmann, of St. Petersburg, lost his life by receiving a charge from the ball while making observations during a thunderstorm. Franklin's experiments with kites are familiar to you all. The subject did not, however, attract methodical observation until some experiments were made by Lord Kelvin about 1872. For these he used a copper rod attached to an electroscope and bearing a fuse; a little later he invented the water-dropper apparatus, similar to that used at Kew at the present day. This consists of a copper reservoir supported on insulating legs. From the reservoir a small pipe allows a thin column of fluid to break into drops at a point outside of the building. At the moment when the water breaks into drops it disturbs the electric condition of the surrounding air, and these alterations are recorded by a quadrant electrometer. The movements of the electrometer are photographed by an automatic apparatus, and thus the variations of the atmospheric potential are observed continuously throughout the day. Observations with this instrument have been made at Kew since 1880. Dr. Chree, the director there, has summarized the results in various papers presented to the Royal Society. Similar observations have been made at the Bureau Central Météorologique in Paris, at the Eiffel Tower, in various other stations in France and Italy, at Lisbon, Brussels, Copenhagen, Jersey, and other places. The results briefly summarized are as follow (fig. 1) :—

The fluctuations in days and months are very considerable. They show a diurnal variation: the maximum of potential occurs in the summer months about 8 a.m., and between 8 and 10 in the evenings; in the winter about 10 a.m., and between 6 and 8 in the afternoons. *The readings are always higher in the winter than in the summer.* The morning minimum is about 4 a.m. throughout the year; the afternoon

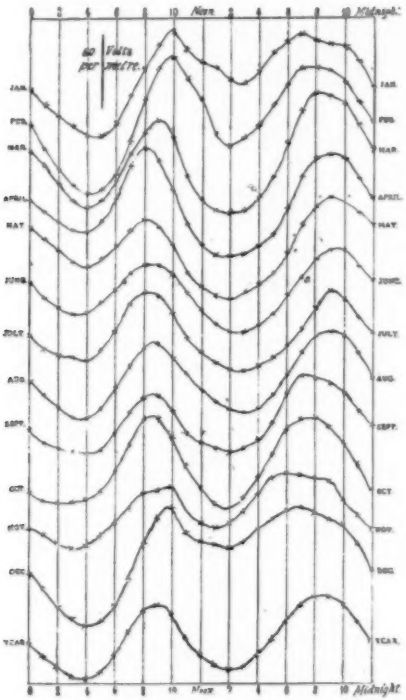


FIG. 1.

Kew data. Diurnal variations (volts per metre). The scale is indicated by the line showing the length equivalent to 50 volts.

minimum usually about 2 p.m. Meteorological conditions—such as barometer, temperature, sunshine and wind—have been compared with the potential recorded, but the results are not definite. "In winter the weather conditions which accompany a high barometer favour the existence of high values and big diurnal changes in the potential." But in the summer the barometric readings and the mean, and range, of

daily variations of potential seem to have no relation. The only resemblance between them is in the slight diurnal range of barometric pressure, which does seem to agree with that of the electric potential, excepting that the maxima of the latter are an hour earlier than the barometric; that is, even when the glass is said to be steady there is a slight hourly variation, which when worked out to 0·0001 of an inch shows a mean hourly variation similar to the electric, though a little slower in attaining its maxima and minima. There is, however, no correspondence between the grosser movements. A high potential occurs indifferently with a low or high barometer.

I will now endeavour to explain a little more clearly what is meant by the term "potential." The earth is generally negatively electrified, the atmosphere positively. It is probable that the negative electrification of the earth is not uniform, but that currents of greater or less intensity exist; but for purposes of calculation it is regarded as uniform and considered as zero. The strata of atmosphere vary considerably in the amount of positive electricity they hold. Wherever in nature objects that are positively and negatively electrified meet, there is a desire to yield up the excess and establish an equipoise. Thus there is always a strain on the part of the earth to give to the atmosphere negative electricity, and on the part of the atmosphere to give to the earth positive, so that an equipoise may be established. And this equipoise always is established, so that when you rear an insulated copper rod into the air you get no reading on the electroscope. To get this you must in some way disturb the equipoise, and the intensity with which this is sought to be replaced is the "potential." This disturbance may be effected, as I have said, by allowing a stream of water to break into spray; by allowing sand to run from a nozzle, or by kindling a fuse, which carries away negative electricity as the smoke ascends in the air; the amount of positive strain is then recorded by the divergence of the gold leaf. When you consider that the mass of the earth is negatively electrified, and the upper parts of the atmosphere positively, and bear in mind the strong inclination to establish equipoise, some points will become self-evident. For instance, a towering, pointed rock (fig. 2) will tend to get rid of negative and attract thereby positive electricity. The potential in the neighbourhood will be high. Water is a good conductor; a cloud formed on the ground or side of a mountain would probably be charged with negative electricity. Air currents which condensed into a cloud in the higher strata would form a positive cloud. If such a cloud passed over one still connected with the earth, it should

induce a charge in the latter of opposite sign; that is, by influence, raise its negative charge to a very high potential. The influence of clouds have much to do with the sudden alterations of potentials at any station; so must the terrain. A flat stone plain must part with electricity much more slowly than, say, the sedges by the side of a river. A deep valley must give different readings to a jagged spine on an elevated plateau. The effect of a forest of fir-trees must also be considerable. It seems to me that a series of observations made in the same spot by observers who had selected them for certain definite electric characteristics would be valuable. But the difficulties in the way of the observer are great. The instruments must be standardized, so that the indications may be uniform; but unfortunately instruments do not remain true to their standard. A moist atmosphere penetrates into the most carefully guarded instrument and upsets the insulation, so that the readings are not the same as the instrument would have recorded yesterday. This may be overcome by the use of desiccating substances to dry the air inside the case of an electroscope. Fortunately experience in making observations will give by-and-by a kind of extra sense, so that one instinctively gauges the truth, or otherwise, of a result. If the leaf goes steadily and quickly to a certain height and remains there, it is probably a true reading. If it does not start at once, then jumps up to a certain angle and falls again, and after that goes to a higher point, it is pretty certain that there is a leak in the insulation. The potential rises rapidly for every metre above ground up to a certain height, and then the increase per metre becomes less and less. Thus Le Cadet found the following variations in balloon ascents in August and September, 1893:—

August	8	Height (metres) ...	615	740	790	1,005
		Potential	75	45	35	29
August	9	Height (metres) ...	824	830	1,060	1,255
		Potential	37	43	43	41
September	11	Height (metres) ...	1,140	1,378	1,630	
		Potential	42	38	32	

Potential on the ground was 150. The diurnal variation at mountain stations is also low.

Considering that the sun is positively electrified and the earth negatively, one would expect the potential to be highest near the surface of the latter. Why the strain should vary twice a day is not easily explicable, but it is probable that the heating of successive portions of the earth's surface gives rise to a better ionization of adjacent portions of the atmosphere, and there may be a slurring due to the atmospheric envelope moving less quickly than the earth.

What is meant by ionization? As far as I understand, an electric molecule is made up of positive ions and negative ions—called also electrons. It has been stated lately that no discharge of electricity takes place in the absence of electrons. They are the minutest particles of negative electricity; they are far more minute than the atoms of positive electricity, and they move with a velocity comparable to that of light.

It is interesting to reflect that, at the time when most of us were taught electricity, we derived the impression that the superior kind of electricity was positive, and that negative was a kind of less useful but necessary by-product; that they stood to each other and the world in something of the relation of a consultant to a general practitioner.



Fig. 2.

But time has changed all that. The really active, business particle is the negative. The active part of the X-ray discharge is the negative; the positive atoms slink out the back way by the canals. The emanations from radium are negative. *How do these different atoms exist in the atmosphere?* This is a free country, and in a free country everyone may make hypotheses. I am going to venture on one now. It is dangerous—some one will tell me by-and-by that I have not a scintilla of fact or evidence to base it on. I admit it. But what is an hypothesis? A kind of "pill to purge melancholy." You make some observations, or some one else does, that do not square with your preconceived notions; you are unhappy, your mind can find no place to poise upon. Make an hypothesis that will fit in with your fragmentary

knowledge, and your mind will alight on it like the dove on its second excursion from the Ark.

Figure 3 explains the hypothesis. On the left is a perfect electric molecule—a married positive atom, its affinities all satisfied by little electrons. In the middle you see a company of positive bachelor atoms. Their affinities are not satisfied; their fore and aft bonds are occupied in holding on to one another (the extreme ends of the chain have caught up stray electrons). The lateral bonds satisfy one another as well as they can. It is a bachelor existence only endurable in communities. On the right you see the effect of a bombardment by electrons liberated by one of the operations of nature. They have broken the bachelor chain in two. They have seized on the half-satisfied bonds adjacent, and, pouring through the gap they have made, are attacking the further side of the chain. These are, as I conceive, the various ways in which the atoms of electricity exist in the atmosphere: (1) In a state of rest or equilibrium; (2) in a state of partial or temporary equipoise; (3) in a state of active vibration, seeking rest.

The causes which contribute to the third state—that which engenders a current of electricity—are, among others: (1) electric discharges (you will remember that the discharge from the cathodal end of an X-ray tube is the common source of electrons); (2) the emanations from radium, uranium, and probably barium, selenium, and calcium; (3) the discharge of gases into the atmosphere; (4) the heating and cooling of the earth's surface; (5) magnetic currents; (6) volcanic action, and the discharge of dust into the atmosphere.

In connexion with magnetic currents it is interesting to note that there is a daily current from west to east—most marked in the tropics—and other currents going from the tropics to the poles, and that these latter, through their property of deflecting the electrons into their course, give rise to the appearance of auroræ; while, with respect to the discharge of gases as another method of ionization, it may be noted that sun-spots are supposed to be due to the irruption of masses of hydrogen, calcium, &c. The periodicity of sun-spots and disturbances of the magnet are undoubted, and it is probable that they also influence the atmospheric potential. It seems that years of minimum sun-spots are also years of minimum potential, but the observations have not been made over a long enough period to make this certain. We must remember that sun-spots occur only over a certain zone in the sun's surface, that the sun is not a flat disk but a rotating globe, and that our atmosphere is only likely to be affected when the spot and the two bodies axially correspond.

I will now mention a few results I obtained during the summer with the portable apparatus I designed. The diagram I now show you is a hypothetical section through Bournemouth from the west cliff at the bowling green, by way of Hume Towers, Talbot Woods, to Red Hill. It will surprise those of you who know the locality by its sudden, steep gradations; this is due to the fact that I have, as it were, shut the country up like a concertina, so as to get the section on to an average-shaped slide. In other words, the official in the surveyor's office who kindly made this

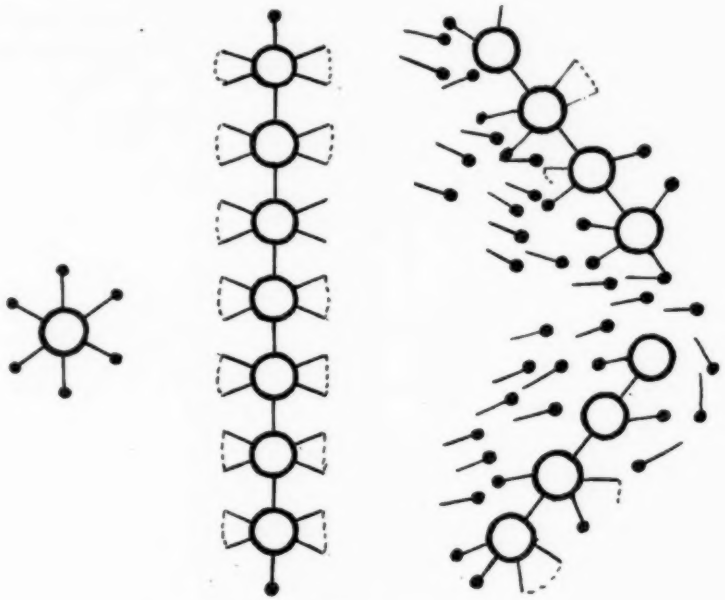


FIG. 3.

plan for me has used two scales—a fairly reduced scale for altitude, and a very much reduced scale for length. The greatest height shown is about 130 ft., but the distance from end to end represents 4 or 5 miles. Fig. 4 presents observations, made within a few minutes of each other, showing the difference in potential due to height alone. One observation was made on my lawn, and is less than its height would lead one to expect; the loss is due to the shut-in nature of the site and the presence of a tall Lombardy poplar, which brings the atmosphere in its neighbourhood down to the potential of the earth. Fig. 5

presents four sets of observations made at two spots of equal altitude. One site, represented by the +, was on the west cliff; the other was a spot between the end of Branksome Wood Road and the Talbot Woods, a heather-clad piece of common which I have designated by the ☾ crescent. From a half to three-quarters of an hour was spent between making the two observations on each occasion. Between these two open, heathery sites there is interposed a part of the town, with its detached villas and gardens, an electric-tram line with overhead wires, and the valley and brook. The first observation shows a considerable difference, which I at first thought might be due to the influences just recited; but on repeating it a few days later I found an exactly equal

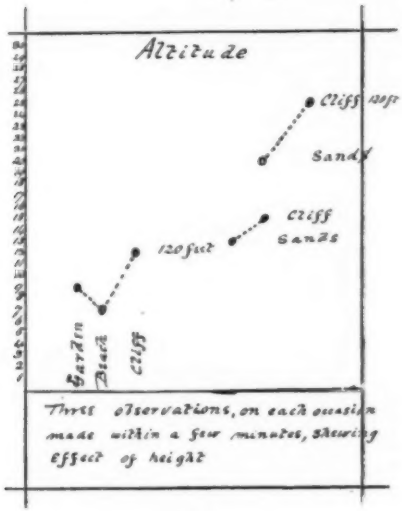


FIG. 4.

potential registered. I then remembered the weather on the first occasion. It had rained in the early morning, and was cool and cloudy in the forenoon. After lunch it continued to clear, and by the time I got to the cliff it was a fine, sunny afternoon, and the clouds had rolled away. I repeated the observations about a month later. There was a difference between the two opposite to that found on the first occasion. One circumstance I noticed at the time: while it was breezy on my making the first observation ☾, the breeze had become half a gale before I made the second, on the cliff, so that I had to hold on to

the table to prevent it being blown over. There were squalls and rain all night, and torrential rain the next morning. On looking at a self-recording barometer next day, a distinct V-shaped notch was found, corresponding to the period between the two observations. I made another observation a week later, when I again got practically the same potential at each site. The second and fourth observations therefore convince me that the two sites may be considered identical.

It is interesting to note that the high reading was with a north-west wind. Lord Kelvin, who made observations with a portable apparatus in the Isle of Arran about 1880, considered that high potentials were always found with, or preceding, a north wind.

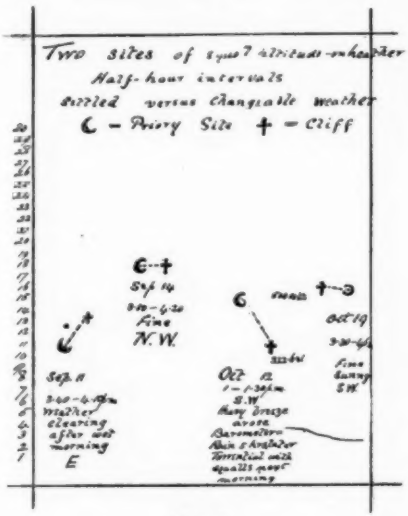


FIG. 5.

The last pair of observations I have to show you (fig. 6) were made at the river near Throop, and in a field near the Hollies at Moordown, respectively. There is a difference of about 100 ft. in the height above sea level. The observations made near the river are symbolized by an anchor, those on the hill by a conventional tree. The first two show a very high potential, exactly similar in quantity, though one would expect the one that was made at 100 ft. higher would show an increase in potential. The second pair shows the potential at the river to be actually higher than that found on the hill. The obvious explanation is

that the passage of a body of water over a part of the earth's surface causes the ionization of the atmosphere in its neighbourhood, either by convection or friction. Some years ago I visited a small sanatorium, or Kurhaus, at Assmannshausen, on the Rhine. The doctor told me that near the Rhine the climate was less relaxing in the summer than in places remote from it. I suggested that the passage of a mass of water might generate electricity. He agreed, but said no one had made any observations of that sort. I knew nothing of "atmospheric electricity" at the time, but the idea remained in my mind, and finding other congenial fragments in that odd repository, they began to develop,

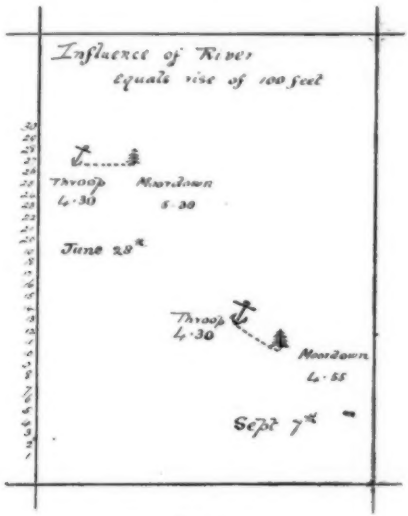


FIG. 6.

subconsciously, a desire to enquire into the subject. I began to read what I could find, and at length wrote to Dr. Chree, of the National Physical Laboratory. This gentleman gave me much information in letters and interviews, and I have developed a keen desire to make more extended observations. The Corporation have shown their interest in these enquiries by giving me a site for a small shed in an open spot, and I hope to continue them during the winter.

In conclusion, I have to thank Col. Sir A. Pechell for his help in photographing the slides I have shown you, and the meeting for giving me this chance of delivering myself.

Balneological and Climatological Section.

January 27, 1910.

Dr. Leonard Williams, President of the Section, in the Chair.

Saline Waters and the Use and Abuse of Common Salt.

By R. Ackerley, M.B.

However inadequately I may put before you the subject of my paper, "The Use and Abuse of Common Salt," and its relation to the action and use of saline waters, I am so convinced that it is well worthy of our attention that I do not apologise for introducing it to this Section of the Royal Society of Medicine.

I do not write as a physiologist, but as one who for many years in the work of a general practitioner, and now for three years in spa practice, has been trying to find out in what way our daily customs and habits affect for good or ill the condition of our bodies.

In the ætiology of disease it is being more and more recognized that the continuous or frequent action of small factors is at least as important as the occasional action of greater ones. The daily action of air and water on rocks, and of, say, earth-worms on the soil, has had at least as much influence on the surface of the earth as earthquakes and floods.

Among the daily habits to which my attention has been directed for many years—not specially in connexion with its action in the causation of certain forms of œdema—was the almost universal one of adding to our food a large quantity of common salt, though, like most of us, I had taken it as fairly well established that the habit was a good one.

A minute or two may, however, be devoted with advantage to a consideration of how the use of salt may have become so general, and how far it is universal, before we go on to consider the more important question of the supposed need of the human body for more of it than is

contained in food, and the good or evil results that follow therefrom. The hypothesis that the desire for salt is due to the fact that our remotest ancestors passed their time in salt water [25] need not be more than mentioned. One can quite believe that early in his history man began to use salt because he liked the taste of it. He may have got used to the saline taste of animal blood, and then, finding that either sea salt or salt deposits in land if rubbed into meat gave it a taste he liked, may easily have acquired a habit of adding salt to food. It is not improbable, too, that very early in his history he found by accident that salt had the property—much more valuable in those early times than now—of preserving food. He would use it for that purpose, and then from its preservative power on the dead body, especially its preventing the malodour of putrefaction, would quite easily conclude that it was good for, and would in some way preserve, the living body. So he would teach his children, as children have been taught up to the present day, that salt was good for them. But it is open to one to suggest that had the salt been merely a preservative of food the salt habit would not have become so general. As in the case of other things, when the taste for it was acquired, very little serious thought would be given to the question whether or not it was good for him. The man who likes wine, tobacco, or warm clothing and stuffy rooms, always persuades himself that his particular fancy is good for himself and others. In this connexion may I quote Ch. Féré [7], who, agreeing that salt is considered a necessity by many people, goes on to say: "It is a stimulant which is not any more indispensable than are alcohol and tobacco, which, though they have come into general use more recently than salt, are almost as fully established in our daily routine. Salt added to food is not necessary, and in certain conditions it is harmful."

Although the use of added salt is very general, it is far from being universal. In ancient times Sallust reports that the Numidians turned up their noses at it; and Plutarch was surprised that the Egyptian priests did not salt their food. In our own time, Widal [34] states the northern inhabitants of Russia and Siberia and the Kirghizes of Turkestan have an actual dislike to it, though they live in the neighbourhood of salt deposits. The Bedouins (according to Wrede) will have none of it, and C. Dittman describes the repulsion the Kamschatkans and Tunguses displayed at the addition of salt to food. Prescott [24], writing of the Tlascalans, a vigorous and highly civilized people of South America, says: "For more than half a century they had neither cotton, nor cacao, nor salt. Indeed their taste had been so far affected by long abstinence from

these articles that it required the lapse of several generations after the Conquest to reconcile them to the use of salt at their meals." And in a description of the North American Indians by a Mr. Catlin, I came across a paragraph which suggested that not only did they not use salt until corrupted by Europeans, but that they attributed bad effects to the habit some of them acquired. Mr. Catlin [5] says: "During the ravages of cholera I was in these regions, and I learned, from what I saw and heard, that it carried death among the tribes on the borders in many cases as far as they had adopted civilized modes of life, with its dissipations, using *vegetable food* and *salt;* but whenever it came to tribes living exclusively on meat, and that without the use of any salt, its progress was suddenly stopped." As against this, I know that abundant evidence shows its general use from the earliest historic times: that one word alone—"Salary" (Salarium, the allowance of salt to the Roman soldier)—shows how important it was considered; so important that the word used for the allowance of one kind of food (or adjunct to food) gradually became the term applied to the whole (fixed) reward for a man's labour. But, quite apart from anything else, I think its preservative power alone would make it almost a necessary of life until recent times.

It is also generally argued that a reason for adding salt to food is to be found in the desire that some animals display for it. Statements are made that it is well known that animals travel immense distances to get the salt at "salt licks," and that domestic animals—herbivorous animals are meant—take salt greedily, and are better for it. Two eminent members of this Section, Dr. Bain and Dr. Edgecombe, in their excellent work on the Harrogate waters, think it is sufficient to state, without quoting any evidence, "It is well known that in cattle impaired nutrition follows on diminished intake of salt" [2]. I am aware of Boussingault's experiments, but they show, so far as I am aware, only that animals gained in weight and in texture of skin—no real proof of *bien-être.*

I will deal with the question of animals at once. It is quite obvious that additional salt cannot be obtained by a very large—possibly the largest—number of even herbivorous animals. It is quite possible that I have been unfortunate in my search, but I have failed to obtain any *first-hand* evidence that animals—deer, buffaloes, &c.—travel immense distances for the sake of salt. That they travel towards "salt licks," and, when there, that they lick the salt, I am quite willing to believe; but I should require evidence more than I can get that the food or water in the neighbourhood was not the real attraction.

As regards domestic animals—cattle and horses—I have been in the

habit of inquiring from breeders and farmers what salt they give their animals. Most men put a lump of rock salt in the stalls or in the fields, but I am told that some cows and horses never touch it. Some farmers do not give salt at all, and do not believe that their animals suffer in consequence. One stud groom, with sixty or seventy horses in stables, gave a tablespoonful of salt in a bran-mash once a fortnight, and thought that that was all they required. But the most suggestive statement was made by one of my patients who bred horses. He did not give them salt, as a rule; but if he found a horse thin and out of condition as to its skin, he gave it salt, and found that weight was put on and the skin improved. But when asked whether the horse was more fit for work or racing, he replied, "No, I don't think it is." When I lived on the borders of Dartmoor I used to see large numbers of cattle turned out on the moor to feed, but I never heard of them being supplied with salt; and, though I kept horses for fifteen years, I never gave them salt, and had no reason to regret not doing so. I think there is some evidence that horses and herbivorous animals living on dried grass and grain do like salt, but is it not possible that the mere liking accounts for everything and that there is no physiological need for the salt? At any rate, it is abundantly clear that both men and animals do perfectly well without the addition of salt to their food, and that there is no need for it in the sense there is need for ordinary foodstuffs or for water.

To return to the human animal. It has been suggested that men who live largely on vegetables which are rich in potassium salts require sodium salts to assist in the elimination of the excess of potash, which would otherwise prove harmful. There seems to be no real evidence of this; and Forel [34, p. 14] stated at a meeting of the Vaudois Medical Society in 1905 that near Locano there was a settlement of vegetarians who would not touch salt. Apparently they did not suffer in any way from their abstention. He mentions specially two people who had remained in perfect health although they had not had a grain of added salt for three years. The evidence appears to me to show that both men and animals take salt not because they need it but because they have acquired a taste for it. Emphasis must be laid on the last words. The taste seems to be acquired. The infant fed on human milk is getting only 1·5 to 2 parts per 1,000 of NaCl in its food. Presumably it is getting all that is necessary for its needs, though it is storing up a fair quantity owing to its rapid growth. Probably at no time of its existence is its metabolism so great and its health so good as at this period. But apart from that its taste for its food does not need any

stimulation by NaCl, and I fancy no one has suggested its addition either as a plain solution in water or through the blood of its mother. After the child is weaned, for some time, if it is having a milk diet, no salt is added to its food. But as soon as beef-tea, meat, fish or bread are given, additional salt is also given. Except to bread—and I confess I can recall no case in my own practice where bread was seriously objected to—it is quite common to find the greatest objection shown to the new food, and sometimes it is to the added salt. I have known children take beef-tea, or gravy and potato, or pounded chicken or fish, if given plain, but object to it strongly if salt is added. And later on, when they begin to understand orders, children frequently try to disregard the command to "take salt" or "take more salt," because they dislike it. One patient I saw last summer (an enormous salt-eater) told me she could remember being whipped because she would not take salt. But the parent or nurse persists, the natural disinclination of the palate and digestive organs is overcome, and the child in the majority of cases acquires a liking for salt—as it would do for alcohol and tobacco, if the use of them were insisted upon in the same way. The desire for salt in later life cannot therefore be quoted as indicating a physiological need for the salt, any more than the craving for gin in the man who has been used to it from childhood is an indication of a physiological need for gin.

In considering, further, any justification for the addition of NaCl to our food, it is reasonable to ask why, considering the number of other salts in our bodies—some of them, if not all of them, on any hypothesis as necessary for our existence—why it is that one, and only *one*, is added to our food as a regular thing, and added in enormous quantity. Until one begins to think about it, the quantity is not realized. Taking the estimate that NaCl forms $\frac{1}{200}$ of the whole weight of the body in a man of 10 st., this would come to 11 oz. Von Noorden [21] puts it as only 7 oz. in a man of 70 kilos (approximately 10 st.). It is quite common for a man to take $\frac{1}{2}$ oz. to 1 oz. of salt daily—i.e., $\frac{1}{22}$ to $\frac{1}{11}$ of the whole NaCl contents of the body; the average seems to be 20 grm. or $\frac{3}{4}$ oz.

So far as I can make out from researches into the statements of physiologists, all the salt that the body requires daily is at most 2 grm. to 3 grm. (30 gr. to 45 gr.), and considerably more than this is contained in our food without artificial addition. But they nearly all go on to say that, apparently, an extra quantity appears to do no harm, though they admit that the healthy body is so intolerant of it that it proceeds to eliminate it as soon as possible. The best general statement on this

point I have come across is contained in Hutchison's [14] "Food and Principles of Dietetics," where he writes:—

"Sodium is chiefly taken in the form of sodium chloride, or common salt. Of this most people consume about 20 grm. daily, which is probably at least ten times as much as is really necessary to meet the needs of the body. There are not wanting people who maintain that this excessive consumption of salt is not only needless, but even harmful [37]. This, however, appears to be an extreme view. It may be admitted—for the experience of those who refuse to add any salt to their food amply proves it—that the amount of sodium chloride contained in a natural form in ordinary foods is quite sufficient for our needs; but there is no proof that an extra addition of salt in the form of condiment is in any way injurious to health. On the other hand, it is equally far from being proved that such addition conduces in any way to the well-being of the body. It has been asserted, for instance, that the addition of salt to the food aids digestion (Ogata), but more recent and exact experiments have shown that—in health at least, and in moderate doses—salt has very little real influence on digestion at all, while in large quantities it actually delays the process [23].

"If, moreover, sodium chloride is entirely removed from the food, the secretion of hydrochloric acid is lessened, or even arrested altogether, and upon this basis it has been urged that one should limit the use of salt in cases of hyperacidity of the stomach. Where, on the other hand, appetite is poor and digestive power feeble, the moderate use of salt in the food may act as a digestive stimulant in the same way as any other condiment. There is also reason to believe that it may slightly aid the absorption of food [8].

"On the general processes of nutrition in the body salt seems to be equally devoid of any pronounced effects. On the one hand it has been maintained that it acts as a cell stimulant [9], while on the other it has been denied, on seemingly equally good grounds, that it has any distinct influence on metabolism at all. The latest and most conclusive experiments tend to show that any action salt may have is in the direction of lessening nitrogenous waste, *provided a sufficiency of water is supplied at the same time.* From all the evidence we may safely conclude that the artificial addition of salt to the food has either no appreciable influence on health at all, or, if it has any, it is an influence which must be described as favourable rather than otherwise."

It is with the concluding paragraph of this excellent summary that I am going to ask you to disagree. It has been already stated that, on the whole, physiologists find no need for more than 2 grm. to 3 grm. of salt daily, and that more than this is contained in all normal dietaries. I must refer members again to Widal [34] for a discussion on this point. Now, although the average quantity taken by the Western nations seems to be between 15 grm. and 20 grm.—i.e., between $\frac{1}{2}$ oz. and $\frac{3}{4}$ oz., that is from seven to ten times as much as there is any necessity for—this quantity is frequently exceeded. To give some instance of this, which

one only hears of on careful inquiry: Two years ago the wife of a man who had extensive chronic eczema, hearing that I had inquired about salt, brought me a salt-cellar which had been sent to her from home, and, handing it to me, said: "My husband takes that salt-cellar full of salt at breakfast, lunch, and dinner,"—it would contain over ½ oz., even if moderately filled. On another occasion, at one of the hotels, I saw an elderly man with chronic diarrhœa. He had a private sitting room in which he had his meals, and, in answer to my inquiry as to salt, his wife (one knows how helpful wives are in showing up their husbands' bad habits) went to a sideboard, and, taking out a large old-fashioned salt-cellar, quite empty, that must have contained about ¾ oz., or 23 grm. when full, remarked, "That was full at lunch (the previous meal). I had one spoonful: he took the rest," and that was usual at his meals. One meets constantly with statements like this: "I can't do without salt; I have always taken large quantities, I take it in teaspoonfuls." This was said by a man with chronic morning diarrhœa. Another could not take coffee without adding salt. Another "felt lonely without salt." Another adds it to marmalade. In another case, which I shall allude to later, it was deliberately taken in teaspoonfuls between two courses. Another took almost as much salt as egg. And yet another stated (he was a man with very troublesome psoriasis, which is steadily disappearing on a lessened ration of salt), "I eat three times as much salt as any member of my family, and we all like it." And to dip bread or bread-and-butter into salt is a fairly common habit.

In all these cases there has been a quantity taken far in excess of the average amount of 15 grm. to 20 grm. daily, and cases such as these are common. But it would be quite a mistake to suppose that the bulk of the additional salt is added at the table; most of it is added in the preparation or cooking of food. Taking the quantities in parts in 1,000—i.e., grammes to the kilogrammes, according to H. Strauss [30] the amount of NaCl contained in common foods is as follows:—

Milk	1·5	to	1·8
Butter, unsalted			0·2
Butter, salted	10·0	,,	14·0
Cheese	15·0	,,	25·0
Eggs	1·4	,,	1·6
Buttered eggs and omelets	24·0	,,	27·0
Meat, uncooked	0·85	,,	1·13
Meat, as served on the table[1]	19·0	,,	28·0

[1] It is right to say that this apparently refers to the German way of serving meat, and would not apply to plain roast or boiled meat, but would to entrées and so on.

Wheat			1·3
White bread[1]	4·8	to	7·0
Brown bread			7·5
Potatoes			0·57
Potatoes, mashed	5·0	,,	9·0
Asparagus, cooked[2]	27·0	,,	35·0
Caviare	60·0	,,	70·0

I have no analysis of such things as bacon and ham, corned beef, kippered herrings, bloaters, dried haddocks, sausages, and so on. The quantity of salt in these varies considerably, but in any case, and especially in the case of fried ham and bacon, it is enormous. Soups, too, contain anything from 15 to 20 parts per 1,000.

To show how much salt may be taken unwittingly, I may mention the case of a man, aged 54, with angina pectoris. He was fond of good cooking and had been in the habit of taking a fair amount of salt. At my request, while living at a hotel at Llandrindod Wells, and taking otherwise the ordinary food as served, he avoided all soups, bacon, ham and other salted meats and fish, and added no salt on his plate. He was able to keep for me all the water he passed. In three days he passed over 40 grm. of NaCl—i.e., taking the days separately, 13·75 grm., 13·77 grm., and 12·53 grm. It may be mentioned that his urea output was correspondingly large—viz., 45 grm., 42·50 grm., and 45 grm., suggesting, I think, a fairly large intake of food.

But one may now leave the intake of salt and devote a little time to the consideration of how it leaves the body. In normal subjects, taking only a moderate quantity of salt and a sufficiency of water, practically all the salt leaves the body through the kidneys; something less than $\frac{1}{20}$ grm.—i.e., less than a grain—per diem is shown in many estimations as passing by the bowel. A certain quantity passes by the skin during profuse perspiration. Sir Lauder Brunton says [3]: "The simple experiment, which we find in every boys' book of chemical tricks, of telling into which basin of water a hand has been put by the turbidity which occurs on testing it with nitrate of silver, shows how constantly we are losing salt from the skin; and if we put our tongue to our hand after we have been perspiring freely, the taste will convince us that the quantity of salt we lose by the skin is not inconsiderable." I have been unable to arrive at any conclusion regarding the amount of NaCl contained in sweat (the analyses vary enormously), but from

[1] My own baker adds 3 lb. of salt to eight dozen 4-lb. loaves—i.e., $\frac{1}{2}$ oz. to one loaf =7·8 grm. to the kilo.

[2] May not this be at the bottom of the trouble some people find in asparagus?

superficial observations on myself, and the statements of patients, I think that the quantity depends very largely on the amount of NaCl that is ingested and how far it is being eliminated by the kidneys. When people take a fair quantity of NaCl and not much water, the skin has a definitely salt taste to the tongue. When the quantity of salt is considerably reduced, and especially if more water is taken, the salt taste of the skin disappears in a way which is noticed by patients without their attention being called to it. But it is agreed that nearly all the NaCl is excreted by the kidneys, and the healthy kidney appears to be able to excrete a very large quantity. Widal and Javal [34, p. 17], quoting Lemierre and Digne, mention cases where as much as 80 grm. ($2\frac{3}{4}$ oz.) were eliminated in a day, and it must be conceded that where the kidneys are sound there is, *as a rule*, no apparent difficulty in getting rid of the average amount of 15 grm. to 20 grm. Quite apart from any ill-effects, one may mention that Allaran and Bernard, Casper and Richter and others, have proved by catheterization of the ureters that the urine coming from a diseased kidney contains less salt than that coming from a healthy one [32]. The greater part, if not all, of the salt probably passes through the glomeruli via Bowman's capsule. The amount passed during the twenty-four hours depends, on the whole (in the healthy body), on the amount ingested. In most cases, unless the amount is excessive, the greater proportion is recovered within twenty-four hours. Soon after a meal containing a fair quantity, the amount contained in a limited specimen may be large; personally, in over 1,000 patients and three or four times as many observations the highest percentage has been 1·69, or 16·9 grm. to the litre.

It may be suggested, in passing, that the very rapidity with which the body gets rid of this material suggests that it does not want it. But in some people, however much they may be taking by the mouth, the percentage of salt excreted does not reach anything like that figure (I am not speaking now of cases where there is any evidence of renal disease), and with these the elimination is delayed, and instead of the proportion varying from 5 to 15 per 1,000, as is very common, every specimen appears to contain something about 8 per 1,000. This at least suggests that there is some difficulty in these cases in the kidneys excreting more than a rather low percentage.

So rapidly does the body get rid of the NaCl in the day time that it is rare to find in people taking only a small quantity of salt anything above 6 grm. to the litre in the first water passed in the morning, and it is frequently less than that, 2·5 to 3·5 being not uncommon. But with

those who are taking a large quantity of salt with little water, the proportion that is passed on rising in the morning, and very often during the night also, may run to 12 per mille or more.

It is not necessary now to discuss the retention of chlorides in febrile diseases, such as pneumonia. And I do not propose to discuss in detail the rôle that chlorides play in Bright's disease, beyond saying that Widal and Javal and others have, I think, proved that in very many cases of dropsy in Bright's disease there is NaCl retention, and that if the salt is withheld or greatly reduced, so that the kidneys are able to eliminate the quantity previously retained, and then deal with the daily surplus, the dropsy disappears. For the whole discussion of this, if any Members of the Section have not already read it, I may refer to the little book I have already quoted (Widal and Javal, "La Cure de Déchloruration"). But what comes out very clearly from their researches, indeed is obvious if one thinks at all about it, is that, lesion of kidney or no lesion, if NaCl is retained—unless it can be deposited in some way in the tissues, or unless the absolute percentage of chlorides in the fluids of the body is increased—water must be retained with it.

A short résumé regarding the way retained chlorides may behave is given by Dixon Mann [18] :—

"Gruener records some experimental observations made on himself in relation to the chloride metabolism, and draws the following conclusions: The relative amount of chlorides in the blood is generally constant, but it may be transiently increased by the administration of excess of sodium chloride. The absolute amount is subject to far greater variations, and when the amount is very excessive it leads to a physiological chlorhydræmic plethora, causing considerable increase in body weight; an equivalent reduction occurs when the excess of chloride is excreted. Should the excessive administration of salt be continued, it gives rise to œdema or favours its occurrence. On the other hand, the relative amount of chlorides in the tissues is not constant; it is subject to considerable variations, especially in pathological conditions; this histo-retention has been observed in interstitial nephritis and in various infectious fevers. The chlorides are then retained in osmotically inactive combination with the tissues, the entire organism being brought to a higher chloride level, the body weight remaining unaltered. H. Strauss believes that at the commencement of the retention the tissues probably play an important part, but that subsequently they contain less than the juices."

Now, hitherto, it has been generally supposed that, *unless* there is evidence of renal disease, some excess of added salt is not harmful, and the evidence that would be required would be albuminuria or dropsy, or some obvious failure of the kidneys to do their work. Even Widal and

Javal seem to suggest that the healthy man, to satisfy his sense of taste, may allow himself to take an excess of salt with impunity. I venture to suggest to you that even the average amount added to his food by an apparently healthy man is harmful, and that it is more than likely that the work thrown on to the kidneys of having day after day and year after year to eliminate a wholly unnecessary quantity of salt is the actual cause of their later inefficiency. It is admitted that the work done by the kidneys in eliminating the salts is enormous. Thus, a priori, must not the augmentation of work, by doubling or trebling the NaCl to be excreted, tend to destroy prematurely the power to excrete? It is generally assumed, even by those who admit that the organism does not require the added salt, that it is easily excreted by the healthy kidney, and, being easily excreted, cannot be harmful. Now, unless one is at once met by the statement that retention of chlorides in itself demonstrates a damaged kidney—and then it would in many cases be the only evidence of such damage—observation shows that, in the apparently healthy, chlorides are frequently retained. Marie, quoted by Higgins [11], mentions a case where 92 grm. were accumulated in twelve days and 15 litres of water were necessary to dissolve it out of the tissues.

I have had many cases, without any signs of renal inadequacy, where the same large proportion of NaCl continues to be excreted from some days after the intake has been largely reduced. In a recent experiment on myself I took, living on a diet of milk and apples only, 40 grm. of added NaCl—the salt being dissolved in the milk—the total NaCl being estimated at 50 grm. The first was taken at 9 a.m. on the 11th of the month, and the last at 8 p.m. on the 12th. By twelve noon on the 13th only 39·4 grm. had been passed by the kidneys; that is to say, that sixteen hours after the last salt was taken, 10 grm. were still in the body (I should say that during that time there had been only one action of the bowels, and that not in any way relaxed). Other evidence of temporary retention is the amount of NaCl which some people pass during the night, and before food is taken in the morning. In observations on myself and members of my family, and in cases where none or only a small quantity of added salt is taken, one finds that from 80 to 90 per cent. of it passes before retiring to bed. But in many cases in patients I find that the urine of the early morning, and also any passed during the night (and large salt-eaters are frequently disturbed in the night), contains a large proportion of chlorides—say 10 to 12 grm. per litre. Although in one sense this is temporary, still, if a condition of

chloridæmia exists for some twelve hours in the twenty-four, if it is even relative and the kidneys *are* just able to eliminate all that is taken in the twenty-four hours, is it not possible it is harmful?

But does all the salt pass through the kidneys, even where there is no disease of those organs? There is abundant evidence that it does not. Cases of chronic diarrhœa are not rare. In many so-called bilious conditions considerable quantities of chloride may be thrown off through the stomach: the lungs and nasopharynx are known media for its excretion, and the skin is called upon to do its share.

Temporary limited accumulations are generally accompanied by increase of fluid in the body. Widal and Javal [34] record experiments showing how the body weight went up and down according as more or less salt was given, less or greater loss of water by the kidneys accounting for the variation in weight. I find that my own weight can be made to vary from 4 lb. to 6 lb. by varying the amount of common salt taken. On a diet containing less than 6 grm. of salt per diem the weight falls; on a diet containing 10 grm. or more it goes up. The evidence all goes to show [34, pp. 25—29] that even when the kidneys are not affected at all there is increased hydration of the body when added salt is taken even in less quantities than the average man takes it.

Now, with your permission, I want to look at this question from another point of view—the possible harmfulness of our present habit of salt eating. It is agreed that sodium chloride is the only salt generally added to food, and that the quantity added is in enormous excess of any bodily need known to the physiologist. The greater number of animals, and a fair number of men, get on perfectly well without such addition. The greater part of the salt ingested is excreted by the kidneys, but sometimes, apart from manifest renal diseases, they fail to excrete it rapidly, so that there is some retention, and with this retention there is associated retention of water. Other organs—the skin, the bowel, and the mucous membranes—also excrete salt when the intake is large. Now, considering these things, one might reasonably expect that harm was likely to follow:—

(1) From throwing upon the excretory organs wholly unnecessary work;

(2) From impregnating the fluids and tissues of the body with a proportion of one salt only far in excess of its needs;

(3) From the retention in the body of compensating fluid; and

(4) From the necessity thrown on other organs of getting rid of this large amount of what is truly—not being required—a foreign body.

Before discussing these points, may I say a few words on the effect of added NaCl on the child? The body is, to begin with, exceedingly intolerant of salt. Give an infant, which has been breast-fed, milk containing anything but a small proportion of NaCl, and vomiting or diarrhœa is likely to result. In the days when I believed in the addition of NaCl I have made a baby of my own sick, more than once, with a small quantity of added salt; and, on the other hand, have frequently been able to arrest chronic looseness of the bowels in children by stopping altogether or diminishing the quantity of salt given. But if the salt is added very gradually, the child does not, as a rule, reject it, but absorbs it and passes it through the kidney; and above the age of mere infancy the young kidney can often deal with it better than the older one. Children I have tested got rid of any salt taken at a meal with extraordinary rapidity. I recall one case which demonstrates this: A boy, aged 7, with so-called functional albuminuria. His first specimen in the morning was—specific gravity, 1014, no albumin; NaCl 3·5 per 1,000. After breakfast of bacon (very salt bacon) the specific gravity of a specimen was 1024; it contained about $\frac{1}{10}$ albumin and NaCl 13·6. The night specimen, after a milk and bread-and-butter supper, and after a fair amount of exercise, was 1,030; slight, not measurable, albumin, NaCl 8·3. (These were the only observations I was able to make, but they are suggestive as to the effect of NaCl and food, as against mere position and movement in causing the albuminuria.) But though the young kidney can excrete salt in largish quantities, I venture to suggest for your consideration whether the skin and catarrhal affections of young children may not be partly due to excess of this condiment. We, all of us, I think, are careful when we get cases of eczema in the young to knock off salt foods, &c., and to give largely of diluents; and as regards the mucous membranes, may I quote Dr. Sydney Ringer [26]: "Chloride of sodium is a large constituent of mucus, and salt, when taken into the system, probably promotes the production of those excretions of which it forms a large part. It is, indeed, a food to the mucous membranes." One's comment is, what happens if this "food" is given in great excess?

So far as children go, there is one observation I made when I held the post of Medical Officer to an Isolation Hospital at Tolworth. Unfortunately, my case-book has disappeared, so I cannot offer detailed evidence. But my practice was to keep children on a milk diet for much longer than is, I believe, usual. The milk might be thickened with some farinaceous food, but no *bread* and no beef-tea, &c., was given for a fortnight, as a rule. My percentage of albuminuria was exceedingly low,

and, though it is true the hospital was a small one, in the nine years I was Medical Officer a fair number of cases passed under my observation. I had no idea then that NaCl might be an irritant to the kidney, but, looking back, it is open to surmise whether the practically saltless diet for a longer period than is usual might not have warded off albuminuria.

But I do not suggest for one moment that the unnecessary NaCl has very marked influence on the kidneys in early life; if it had, it would very soon be stopped. My point, rather, is that except occasionally, the young kidney is capable of doing the work thrown on it without manifest injury; but as in the moral world, so in the physical—

> "The Sin that practice burns into the blood,
> And not the one dark hour which brings remorse,
> Will brand us after of whose fold we are."

It is not the single large dose of poison, but the repeated exceedingly small dose, never suggesting its harmfulness by any immediate symptoms, that is most to be feared. So the first question I ask is whether the burden thrown on the kidney by having to excrete an altogether unnecessary quantity of salt for forty to fifty or more years may not lead to the very condition that later shows itself, among other ways, by an inability to deal with salt excretion, and that is aggravated by the exhibition of salt? Direct proof of injury could only be obtained by experiments on men or animals, carried on for many years, and such proof it is practically impossible to obtain. But if one looks out for possible and likely causes of fibrotic changes in the kidney, one that is not improbable is the strain thrown on the kidney of excreting an altogether unnecessary quantity of NaCl, especially as in the majority of men the excretion is rendered more difficult through an inadequate intake of water. This would, it seems to me, not only give the organs extra work, but supply a source of irritation likely to lead to fibrotic or cirrhotic changes. The improvement that ensues in patients suffering from Bright's disease (quite apart from dropsical conditions) when a diet containing very little NaCl is given is very marked, and, arguing from analogy, is at least suggestive of the rôle NaCl may play in the ætiology of the cirrhotic kidney.

I now pass on to the harm that may result from the fluids and tissues being forced to come into relation with a larger quantity of this sodium salt than is necessary, and from the associated retention of fluid in the body. I will take the gouty condition. It is not necessary to define this, but I wish to include under the term not only acute and chronic articular gout, but those manifestations of defective nutrition

and elimination which we have got into the habit of calling "gouty." The sodium salts, especially the chloride, have been under suspicion for a long time.

Sir W. Roberts [27] has laid great stress on the large rôle played by the soda salts. Speaking of experiments *in vitro*, he summarizes:—

"We have seen that the dominant factors in uratic precipitation, as studied in the laboratory, are the proportion of urates and the proportion of sodium salts contained in the medium. The highest tendency to precipitation is reached when there is a concurrence of these two factors in maximum intensity."

Hence at once we see how NaCl may be both a direct and indirect cause—direct as supplying an excess of the sodium base, indirect in its capacity of an appetizer, leading people to take an altogether excessive quantity of nitrogenous food.

The most recent experiments I have seen regarding the solubility of urates have been those of Dr. Gordon Little [15], where he shows that while 1,030 parts of distilled water will dissolve 1 part of sodium biurate, of a solution of 0·5 per cent.—i.e., 5 per mille—of NaCl it takes as much as 44,000 parts.

Biurate taken	Parts of distilled water required to dissolve one part	Parts of 0·5 sodium-chloride solution at body temperature required to dissolve one part
Calcium biurate	4,760	3,420
Sodium ,, ...	**1,030**	**44,400**
Magnesium ,, ...	2,440	1,960
Potassium ,, ...	550	555

Dr. Little's comment on this is:—

"The enormous effects of the sodium chloride on the solubility of the sodium biurate is, in the language of physical chemistry, probably to be referred to the mass action of the common sodium ion of the sodium chloride and sodium biurate, tending to throw out of solution the less soluble of the two salts—viz., the sodium biurate. At least, such an action has certainly been shown to occur in countless cases in pure solution of salts possessing a common ion in their constitution. Hopkins and Hope [12] quote Nernst's generalization to the effect that any two salts susceptible of dissociation, which contain an electric ion in common, naturally diminish each other's solubility."

Now seeing that excess of sodium salts (the chloride) is being taken by the vast majority of us, even if there is no considerable retention, and even if water is retained sufficiently to keep the ratio of salt somewhere about the normal, there must be a considerable time in the twenty-four hours when the ratio is excessive, and that would probably be in tissues and parts of the body where the circulation is least active. And it is

precisely in those parts, if the figures quoted by Sir W. Roberts are correct, that we do get the highest proportions of NaCl [27, p. 102].

TABLE SHOWING THE PERCENTAGE OF SODIUM SALTS IN THE SEVERAL FLUIDS, TISSUES, AND ORGANS OF THE BODY.

	Per cent.		Per cent.
Blood serum	0·70	Blood corpuscles	0·20
Lymph	0·70	Brain	0·20
Synovia	0·80	Muscle	0·08
Cartilage	0·90	Spleen	0·04
Fibrous tissue	0·70	Liver	0·02

It is true that very much of the chemistry of the living body is at present unknown to us, but I submit, with all humility, that there is a strong presumption that the continual, or almost continual, excess of a highly diffusible sodium salt must necessarily modify the vital chemical processes, and that it is, in itself, a sufficient cause for the precipitation in the fluids of tissues of the body of salts less soluble than itself—especially salts with a sodium base. But, with even relative salt retention, there is generally retention of water. Now, whether water is retained or not, as far as one can ascertain, the blood itself gets rid of the excess of NaCl fairly rapidly. If water is not retained, there must be a hypertonus of the fluids of the body; if water is retained, there must be increased peripheral resistance. In either case—I speak very diffidently here, because I am not a physiologist—it appears to me that there must ensue an interference with the power of the tissue, and of each individual cell, to get rid of its waste products, and also an interference with the peripheral circulation. So that at one and the same time we have a condition which will lead to an accumulation of waste products or toxins, and also to a raising of the blood-pressure. And this is precisely what we have in the gouty and allied conditions.

Regarding the rôle that excess of NaCl may play in increasing the blood-pressure, it is instructive to read a paper by Dr. Leonard Rogers [28] on the use of hypertonic NaCl solutions in cholera. In cholera, as one knows, the loss of water and NaCl by the bowel is excessive; the amount of the salt lost amounts to from 5 grm. to 10 grm. per litre (as a rule, in health, the NaCl loss by the bowel is negligible). With this there is an enormous fall in blood-pressure, down even to 35 mm. on a Riva-Rocci manometer. The intravenous injection of 3 to 4 pints of a hypertonic solution—i.e., 1·35 per cent. (or 13·5 grm. to the litre)—caused a rise in blood-pressure in all cases. As examples I may quote: from 35 to 100, 55 to 110, 55 to 105, 60 to 100; and they claim that they had such excellent results that, in a series of cases, the mortality

was reduced from 63 per cent. to 33 per cent., though moribund cases were included. It is interesting to read that in several cases where life was saved, temporarily, there was uræmia a few days later. But in daily life will not excess of NaCl also lead to a higher blood-pressure?

And though it is difficult, and may be impossible, to prove that the habit of taking salt from infancy is a large factor in causing gout, I feel sure, from experience in a large number of cases, that if, like Sir W Roberts, we direct patients to restrict, as far as practicable, the use of NaCl with their meals, if they obey us, the advice will have excellent results for them, and we shall gain in kudos, though our purses may not be so full.

Time does not permit me to go over points connected with dyspeptic conditions and some forms of obesity connected with common salt, but I may say I have had some cases of so-called hyperchlorhydria in which the symptoms have disappeared entirely on reducing the NaCl. The most serious case I have ever seen was in a youngish woman who used to eat lumps and teaspoonfuls of salt with, and apart from, her food. In relation to the digestion of starch there is evidence that it is retarded by the use of NaCl [22]. As regards obesity, in many cases the continued restriction of salt has more effect than any other dietetic change. The rôle NaCl plays in œdema is set forth at length by Widal and Javal, and I may refer to a very interesting case published by the late Dr. J. H. Bryant [4], and more recently to a reference to œdema in children by Dr. Melland [19].

(May I say here that, as regards the restriction of salt, it must begin in the kitchen? I found, when I began in my own dietary to reduce the quantity of salt taken, that on my plate I was taking only about 3 grm. per diem; and by leaving that off I reduced the daily quantity only from 15 grm. to 12 grm. By attending to the amount added in the kitchen the quantity has been reduced to something about 7 grm. or 7½ grm. per diem.)

As regards the question of retention of fluid, it may be pointed out how this must assist the retention of what we have at present to class as "toxins." Whether these are purely chemical or the result of bacterial action, the presence of unnecessary fluid must, I think, necessarily render their elimination more difficult. As regards bacterial action, Vincent [31] finds that the presence of NaCl favours the growth of bacteria, especially when the tissues are artificially raised in temperature, as in summer (obviously in summer, with copious perspiration,

the percentage of salts in the blood is increased).[1] What bacteria he does not say.

We now pass to consider quite briefly the vicarious excretion of NaCl by other organs. I call it vicarious because in the infant at the breast, and in people who take a small allowance of salt, there is no indication that the bowel, the skin, or the mucous membranes take part in the excretion of NaCl, except to a very slight degree.

I have already mentioned diarrhœa in children. I have now had several adults with diarrhœa, especially morning diarrhœa, who have been relieved of their annoyance, though it had been of years' duration, soon after reducing their NaCl intake.

In chronic bronchitis of the gouty the expectoration is frequently very salt to the taste: I confess I have not tested it for chlorides. In this form of bronchial catarrh, and in several cases of nasal catarrh, especially in the morning catarrh accompanied by distressing sneezing, I have known very considerable improvement follow the reduction of added salt. In a case of a glycosuric patient, who was continually hawking up a quantity of thin excretion (he required a vessel in my consulting-room for expectoration), I found that this contained over 4 per 1,000 of NaCl.

In some chronic conditions of the skin, whether it is that the skin suffers from attempting to eliminate a larger quantity of salt than it is fitted for or not, the improvement that follows the reduction of salt in food is very striking. Gaucher and Desmoulière [10], referring to skin diseases, say: "There is a high elimination [of NaCl] as the case improves, and, on the contrary, the lowest amounts are found at the onset of the symptoms, or else in chronic diseases when there is a recrudescence of the condition." One very striking case may be mentioned. In 1907 I saw a man, aged 59, with a slightly gouty history but in excellent condition except for an extensive dry, scaly eczema of the whole of the abdomen, upper parts of the thigh and scrotum, with intense irritation at night. It was of some years' duration. The bowels were loose, as a rule three actions a day, the first a loose one before breakfast. He had suffered much at the hands of many physicians, and had been at several spas, but had obtained no relief. He was the man I mentioned who used to take a salt-cellar full of salt at a meal. The salt was given up; the skin began to improve almost at once, and

[1] It is surely open to speculation, too, whether the retention of chlorides in pneumonia is beneficial, as is assumed. Is it not possible that if chlorides were withheld resolution of the damaged lung would be accelerated?

he was immensely better before his three or four weeks at Llandrindod were over. Two months later he wrote to tell me he was free from his trouble for the first time for years. Now the sequel is interesting. The next year he turned up with a patch of eczema round the umbilicus and some on the scrotum. It had been coming on for a few weeks. He had remained free from trouble for several months, and then, as he was exceedingly fond of salt, he thought possibly giving it up had nothing to do with his improvement, so he took salt, though in less quantities, again, with the result that the eczema returned. In his case there was no sign of renal inefficiency—on two occasions I found 11·5 and 14·6 per 1,000 in an early morning specimen of water. I must not delay you by giving additional cases, but I may mention that, besides cases of eczema, I have known considerable improvement in cases of psoriasis, recurrent herpes, urticaria, and especially in that troublesome condition—pruritus ani.

I want here to draw attention to the dietetic rules laid down by the authorities on skin diseases. As regards pruritus, Sir Malcolm Morris [20] says: "It will be well also if the patient can be induced to exclude shell-fish, pickles, and all highly seasoned, salted or preserved food from his dietary"; and as to eczema, the late Dr. Radcliffe Crocker [6] says "highly seasoned and made dishes should be avoided." "Salt meats are only contra-indicated because as a rule they are difficult of digestion and less nutritious, weight for weight, than fresh meat." Still, the salted and highly seasoned, which generally means highly salted, food is forbidden. And in connexion with this one may refer to the usual dietary ordained for the gouty, especially the things they must not take. If one takes a list of articles of diet to be avoided by the gouty [17]—"Rich meat soups (ox-tail, turtle, mock turtle, kidney, mulligatawny, hare, giblet); salmon, mackerel, eels, lobster, crab, mussel, *salted* fish, *preserved* fish, *tinned* fish; duck, goose, pigeon, high game; meats cooked a second time, hare, venison, pork, lean *ham*, liver, kidney, *salted corned* or *cured* meats, *pickled* meats, *preserved* or *potted*[1] meats, sausages, all articles of food pickled in vinegar, all highly seasoned dishes and rich sauces"—what strikes one as regards all these is that in nearly all NaCl is required to a very large extent to render them palatable to the man whose palate is used to much salt, or else they contain much salt. Though I have got more than used to food prepared without NaCl, I confess a jugged hare without salt appeared to me, to say the least, uninteresting. Once more, I suggest, may not the added salt be the trouble?

[1] The italics are mine.

Finally, as showing what may go on when there is a failure of all excreting organs to deal with excess of chlorides, may I quote Widal? [33] :—

"The visceral chloridæmia may be manifested by dyspnœa, vomiting, diarrhœa, headaches, Cheyne Stokes respiration, attacks of eclampsia, according as the incubus falls on the lungs, the digestive apparatus, or the nerve centres. The powerful and immediate effect of dechloridation proves that these symptoms belong to the state of chloridæmia."

I will pass on now to a brief consideration of saline waters, especially in relation to the question NaCl secretion. Our President [35], with an appreciation of the harmfulness of retention of chlorides in conditions of renal disease and gout, has very ably put before the late Balneological and Climatological Society, his reasons for thinking that, in these conditions, saline waters must necessarily be harmful, and more recently in the special Spa Number of the *Practitioner*, writing of some renal spas he states [13] :—

"It has long been recognized that many of the purgative spas, such as Homburg and Kissingen, exercised a diuretic as well as a purgative action, owing in all probability to the large amount of water ingested during the ordinary course of treatment. But the great majority of these waters contain a relatively large proportion of common salt, a constituent which has been shown by Widal and others so to militate against functional renal efficiency that in predisposed persons—in persons, that is, who, like the gouty, are troubled with a measure of renal inadequacy—it is possible by exhibiting the salt to produce dropsy, and to cause its disappearance by 'withholding' the salt. Having regard to these facts, it is evident that a natural mineral water which contains chloride of sodium cannot be regarded as a satisfactory vehicle for assisting in ridding the system of toxic principles which normally find exit by the kidneys."

Now this assumption that a saline spa, whether the waters contain a less or greater proportion of NaCl than does blood serum, is likely to be injurious to the gouty is held by a fair number of men, and so far as I can judge, at any rate with regard to the weaker salines, without any evidence.

As regards Llandrindod, with which I am most familiar, the four springs which we use most vary in NaCl from 4·7 to 1·1 per 1,000, the intermediary ones being 3·9 and 2·3 per 1,000. That is to say that *all* of them contain less NaCl than blood serum. But the underlying assumption, in the suggestion that these saline waters are harmful in gouty conditions, is that these mixed natural waters behave exactly like the same quantity of salt *taken with food*. They behave, so far as I can

ascertain from watching them very closely for nearly three years with eyes wide open to see any doubtful or harmful results, very differently indeed. In the first place, even though there is a large excretion of NaCl after a meal containing it, the salt taken with food is retained longer in the body and excreted more slowly than the same quantity of even the single salt dissolved in water and taken on an empty stomach. In reference to this, one may point out that even water taken with meals is retained in a way it is not if given apart from meals. Single experiments, and experiments extending over a week at a time on my own person, go to show that 5 grm. of NaCl taken on an empty stomach in anything from 1 to 3 half-pints of cold water will produce a rapid and watery salt-containing action; 5 grm. and even 10 grm. taken with food have no marked effect on the bowels. The mixed salts of the stronger saline water act in the same way in similar quantities if taken cold.

I quite agree with the contention that the purgative action of strong salines is not all that is wanted, and alone may be harmful. As Dr. W. G. Smith writes, referring to purgative waters: "Unless the toxic substances in the blood are removed, the relative amount of toxins left behind will be greater, and so the last state of that patient will be worse than the first." But possibly someone from Harrogate will tell us whether it is the practice of men of knowledge and experience there to deplete the body of fluids by the use of their stronger salines, and do nothing else. Until they tell me it is, I will not believe it.

But I should like to suggest that in the face of the evidence that gouty and even damaged kidneys are able to excrete a fair amount of salt in the day, though possibly not in so large a percentage as the young kidney, the important thing is that the ratio of NaCl should be kept low. At Llandrindod even the strongest saline water contains less NaCl than blood serum does, so that if that water alone were given it would be a diluent so far as the salinity of the fluids of the body are concerned; and I have not met in my spa practice any patient unable to pass at least 5 grm. per litre. Even in a case of chronic nephritis with chloride retention quoted by Dr. Bain [1], the relative quantity of NaCl passed by the patient on three consecutive days was:—

6·79, 4·97, and 6·57 per 1,000, viz.:	Urine in c.c. ...	1303	1448	1336
	NaCl in grm. ...	9·26	7·20	8·79

But at Llandrindod Wells it is not our practice to give only the strongest of our saline waters. We give regularly and largely of weaker waters—at Llandrindod very largely of a saline (described as a sulphur water because it contains H_2S in solution) of which the NaCl contents

are only 1·1 per litre, and in many cases that is the only water given. Now this water is an exceedingly efficient diuretic. I find that in myself and many of my patients it acts much more rapidly than distilled or plain water. It carries with it a very small quantity of urea—0·8 per cent. is about the average—but its NaCl contents are augmented to an amount varying from 2·5 grm. to 6·5 grm. per litre; very rarely indeed do I find the ratio greater than 6 grm. So that it is acting as a very efficient flush, so far as NaCl is concerned.[1] I wish to lay emphasis on the fact of its rapidity of action; for I am inclined to think that the small quantity of salts it contains may assist its rapid passage through the stomach and absorption from the bowel.

But quite apart from thus ignoring the existence and very large use of the weaker salines, Dr. L. Williams and others write as though NaCl were the only constituent of those mineral waters. But at Llandrindod, even in the water containing most NaCl, it amounts to only 75 per cent., and salts of calcium, potassium, and magnesium make up the other 25 per cent., and in the sulphur water I have already mentioned it amounts to only just 60 per cent. (80 in 132). Now is it not more than likely that these other salts have some effect on their passage through the body, in the elimination of waste products and retained salts? I have already quoted Dr. Gordon Little [15] on the solubility of urates. Let me quote him again. After pointing out that salts with the same base diminish each other's solubility, he says, referring to Hopkins and Hope [12]:—

"But the converse of this proposition has been found to hold true in certain cases, and salts possessing no electric ion in common may mutually increase each other's solubility in a fluid. The possibility exists, therefore, that the ingestion of a mixed dietary may produce such a temporary increase in the proportion of salts other than those of sodium (especially potassium salts) as to increase the solubility of any retained sodium biurate, and so accelerate its excretion. They consider this an important principle in lithiasis."

Instead of mixed dietary, read mixed saline water, and there is at once a suggestion why these mineral waters are so efficacious in gouty conditions.[2] (*See* Table, p. 73.)

[1] Whilst writing this I repeated an experiment I had tried many times before. I had breakfasted at 9. At 1.5 I emptied the bladder and took 10 oz. (280 c.c.) of the water, containing 1·1 per 1,000 NaCl. At 1.25 I passed 195 c.c. of urine with a specific gravity of 1006, containing 3·9 per 1,000 of NaCl and 0·6 per cent. of urea. The amount of NaCl recovered was 0·7 gr. (only 0·3 gr. had been taken in the water).

[2] The magnesium and potassium salts will assist in the elimination of salts with a soda base, and vice versa. Calcium and magnesium urate, as we saw, were more soluble in a NaCl medium than in distilled water.

The experience of thousands of people who, frequenting the saline spas, and having no treatment except that of imbibing a largish quantity of the waters, derive such benefit that gouty symptoms are got rid of, and remain in abeyance for a considerable time, is to my mind evidence that, even without medical supervision, the risk to the gouty kidney at saline spas is not very great. But I admit that cases occur where people, taking water on the advice of lay friends, or advised as to quantity by their home doctors, do get into trouble. They absorb the water instead of eliminating it; weight goes up, blood-pressure goes up, and, occasionally, there is albuminuria. Now in the few cases I have seen, the people who suffer in this way have been small water-drinkers and large salt-eaters; and I am inclined to think that it is the NaCl previously retained in the body that is the cause of the trouble, and had these people begun to take largely of water at home, or at such a spa as Evian, the same trouble might have ensued. But among my own patients there has been no case of this kind.

It is impossible to give detailed evidence in this paper; but the dangers Dr. Williams fears for gouty patients at saline spas do not exist, if the least care is taken to inquire and examine into the renal condition, and especially as to the chloride output. If large quantities, or even average amounts, of culinary salt are being taken, and if in the urine passed after meals the chloride percentage is small, or (whatever the quantity) if the early morning urine is rich in chlorides, I am careful about the stronger saline—washing the body well with one of the hypotonic waters. (May I say here, in passing, that I believe the waters should be given cold?) But at the same time I give careful injunctions as to the reduction of the salt taken with food, and with these precautions I find, in gouty cases, rapid and lasting improvement. High arterial tension is lowered, œdema (whether apparent, or latent and manifested only by weight) is reduced; so-called bilious attacks and diarrhœa are lessened, and chronic catarrhs of mucous membranes are relieved, a sallow skin becomes clear; in a word, all the symptoms that are associated with gout or chronic salt-poisoning are benefited, instead of being aggravated.

I fear I have tried your patience by this somewhat lengthy paper, but, in conclusion, may I say that it appears to me that the use of culinary salt consists mainly in its preservative power; it is of use temporarily, and occasionally, as a stimulant to persons who, though possessing sound organs, are defective in weight, and have a subnormal blood-pressure. But, on the other hand, the abuse of salt is great. I

suggest that the evidence of NaCl poisoning is overwhelming, if we are on the look-out for it. In the life of Darwin, by his son, it is related that when the great naturalist first visited the Snowdon district in North Wales, he failed to notice the evidence of glacial action. On a subsequent visit, when he knew better what to look for, he found signs of it in every direction. What I ask you to consider is whether there is not a strong presumption, and a fair amount of evidence, that in some of the chronic diseases of middle and late life the excess of sodium chloride, which has been taken from infancy, is a more or less direct or contributory cause. I suggest that it stands to diseases caused by defective elimination of waste products as foul air stands to respiratory diseases, and we shall not realize its importance until a sufficient number of us have given up adding it to our food. On the other hand, I submit that the condemnation of saline spas in gouty and renal conditions, on the ground that NaCl is one of their largest constituents, is not justified, either theoretically or by the large clinical experience of practitioners at those spas.

Method of Estimating NaCl in Urine.

Volhard's is the one on which I rely. Mohr's method is less accurate, and may be even 30 per cent. out, but it is very rapid and is therefore useful in daily practice.

Note.—The following figures show how in my own case the quantity of NaCl ingested has been reduced without any discomfort, and with marked benefit to general health. The weeks were taken quite casually and were not selected, except as marking stages as the NaCl was reduced:—

Ordinary diet	Daily average		Grm. NaCl to 1 litre
March 10 to 16, 1908 (7 days) ...	Urine 2100 c.c.	NaCl 15·5 gr. ...	7·4
September 1 to 7, 1908 (7 days) ...	Urine 1900 c.c.	NaCl 10·95 gr. ...	5·4
January 1 to 7, 1909 (7 days) ...	Urine 1528 c.c.	NaCl 7·68 gr. ...	5·0
Milk diet (3 lit. milk and ½ lb. apples)			
December 19 to 22, 1909 ...	Urine 2526 c.c.	NaCl 6·34 gr. ...	—
December 20 to 22	Urine 2678 c.c.	NaCl 5·95 gr. ...	2·2

It is worth notice that as the NaCl is reduced less water is taken instinctively, and yet even so the proportion of NaCl falls. The low proportion of NaCl on a milk diet is very marked.

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DISCUSSION.

Dr. Solly said that his own experience and that of his colleagues in Harrogate supported Dr. Ackerley's contention that an excess of salt absorbed was harmful, especially to the gouty or to those whose eliminatory organs were at all defective. In administering the "strong sulphur water," which was very strongly charged with sodium chloride, it was always considered necessary to insure free purgation, even to the extent of frequently administering a "liver pill" overnight, and Harrogate practitioners were prepared to find trouble arising if the taking of the waters was not rapidly followed by free evacuation; the dehydrating action upon the blood and tissues being counteracted by the taking of a glass of one of the milder waters (whose salinity is about equal to "normal saline solution") during the forenoon, afternoon, or both.

Dr. MOUILLOT said there could be no doubt that the diuretic action of weak saline waters was very marked. One was apt at first to think that this was due only to the quantity of water which was consumed; but very little experience showed the contrary. Even men who would habitually take a far greater quantity of liquid, in the form of whiskies-and-sodas, would complain of the diuretic action of the weaker saline waters. So he was sure Dr. Ackerley was right in calling attention to the diuretic action of the waters. With regard to the "Harrogate strong sulphur," which was given in aperient doses, his belief was that it was not absorbed because of its great proportion of salt; that where it acted well, its action was that of a stimulating flush. But there was no doubt about the weaker waters being absorbed, and that they had a strong diuretic action.

Dr. G. E. HASLIP desired to ask the author what was the effect of sodium chloride as a purgative. He had recently had under his care two obstinate cases of constipation. One was a soldier who had to give up a fairly good position in India. He had taken many drugs without effect, but had been perfectly well since he began to take a teaspoonful of the common salt in water, on an empty stomach. He had had the same experience recently in two other cases. He also had been struck with the contention in the paper that salt taken in water was harmless, but when taken with food had a bad effect.

Dr. R. ACKERLEY, in reply, said that it was curious that sodium chloride had not been used more often as an aperient. In the old "Squire's Companion" for 1894 it was given as a purgative in doses of 120 gr. to 240 gr. He found that on himself 75 gr. taken in a tumbler of water, preferably cold, would act in fifteen minutes effectually, and without pain. Examination of the fæces was a difficult matter in ordinary practice, but it would be worth while to find out exactly what quantity of chloride of sodium was passed through the bowel when used as an aperient. Dr. Williams attacked saline waters universally for gouty people. [The PRESIDENT: Other things being equal.] His point was that they were not to be condemmed in gouty conditions. He (Dr. Ackerley) had found reasons for condemning salt taken with food. Surely there was an enormous difference between the temporary use of any drug taken in a particular way with water and that drug taken with food. Supposing all present were arsenic-eaters, should there be no distinction between giving arsenic with every meal for forty years and the taking of arsenic at a particular time in a particular way for a special purpose? Saline waters did not behave as a mere solution of sodium chloride. Even water behaved differently when taken on an empty stomach from what it did when taken with food. As had already been said, the blood serum did not seem to alter in the matter of its saline content; and that fact furnished an argument against the use of salt. The blood itself would not tolerate more than a certain quantity of the substance, and got rid of it as soon as possible.

Balneological and Climatological Section.

March 9, 1910.

Dr. Leonard Williams, President of the Section, in the Chair.

Arthritis Deformans.

An Address Introductory to a Discussion on the Subject.

By R. Llewelyn Jones, M.B. (Bath).

While deeply sensible of the honour, yet I must confess that it was with many misgivings that I acquiesced in our President's suggestion and consented to open a discussion before this Society upon so obscure and intricate a subject as arthritis deformans. However, having had the temerity to essay the task, I found myself forthwith confronted with the by-no-means trivial difficulty of deciding as to which aspect of this complex subject would most readily and most profitably lend itself to discussion. Upon reflection, it occurred to me that I could not do better than select as the theme of my remarks the classification and differential diagnosis of the various joint affections grouped generally under the term "arthritis deformans." Doubtless all of you who are familiar with the extensive literature will admit much remains to be done before we shall be in a position to effect such a differentiation as shall lie beyond the reach of cavil. Our tardy progress towards this much-to-be-desired achievement may legitimately be referred in part to the bewildering state of our nomenclature.

This question of the terminology of chronic joint diseases is, indeed, one that clamours for speedy and careful consideration, if only with the

object of insuring a greater measure of definitude in regard to the application of the various nosological terms. As matters stand, numerous and so varied are the designations employed, and these withal are so vaguely defined, that not infrequently one finds the same term applied to denote wholly diverse joint affections and vice-versa identical arthritic conditions labelled by different appellations. Such indiscriminate application on our part of the various terms in use inevitably begets its own Nemesis in the shape of those mutual misunderstandings which so grievously hamper intelligent discussion, and for the same reason render perusal of the literature so laborious and but too often futile. Being therefore wishful to—as far as possible—obviate any confusion on this question of nomenclature, I would state that I use the term "arthritis deformans" in a generic sense, as covering all those various arthritic conditions to the discussion of which we are this evening committed.

Latterly the opinion has been steadily gaining ground that not one but several diseases have been included under one or other of the various terms used to designate collectively this class of joint disease. This view is, however, by no means as novel as certain writers on the subject are apparently inclined to consider, for they would appear to have overlooked the fact that even some years prior to the appearance of Charcot's classical monograph a German physician, von Volkmann, had clearly differentiated the anatomical lesions of rheumatoid arthritis and osteo-arthritis. Moreover, this contention of von Volkmann cannot be dismissed as mere speculative inference, for, as his writings demonstrate, he based his contention as to their fundamental distinctions upon the bedrock of ascertained pathology as revealed to him in his anatomical researches. Thus, he first established the fact that in rheumatoid arthritis the initial changes began in the synovial membrane, the cartilages being affected secondarily. In addition, he noted the primary thickening of the periarticular and synovial tissues, and the subsequent atrophy of the same; likewise the occurrence of fibrous and bony ankylosis. Furthermore, in his work he contrasted the features of this group with those of osteo-arthritis, of which he gave a classical description. It will be seen, therefore, from the foregoing details that the modern view as to the distinctness of rheumatoid arthritis and osteo-arthritis is, after all, but a recrudescence of that originally brought forward by von Volkmann. With the advent, however, of Charcot's monograph on the subject, the remarkable researches of von Volkmann underwent an almost total eclipse. The great French physician, as we know,

though he tacitly acknowledged the clinical distinctions of the two main varieties of arthritis deformans, nevertheless claimed that pathologically they were but different expressions or grades of the same underlying morbid process. So commanding was the personality of Charcot that this illustrious physician's conception as to the essential pathological unity of all forms of arthritis deformans dominated not only the French but most other schools of thought. Fortunately, however, the German school still retained their power of independent observation, and in the fullness of time von Volkmann's pioneer researches, as well as his deductions therefrom, were amply confirmed and reinforced by a series of anatomical investigations by different workers, among whom may be mentioned Hueter, Samdran, Waldmann, Hoffa, and Wollenberg.

Elsewhere [1] I have referred in detail to the researches of these observers on the morbid anatomy, but time does not admit of my alluding in detail to their post-mortem studies this evening, and I must content myself with stating that the results of these and other anatomical observations have largely determined me in regard to the classification I am about to submit to you of the various types of arthritis deformans. Under this title at least three distinct varieties of joint disease may be distinguished:—

(1) Rheumatoid arthritis or atrophic arthritis;
(2) Osteo-arthritis or hypertrophic arthritis;
(3) Infective arthritis.

With regard to the first-named division, rheumatoid arthritis, it may be mentioned that the synonymous term of atrophic arthritis was first introduced by Charcot, who noted in regard to one of his clinical types of arthritis deformans that secondary atrophic changes developed during the later stages of its life-history. This atrophy, he tells us, involved not only the skin, subcutaneous and muscular tissues, but even the bones: hence his suggestion that the name "atrophic arthritis" should be adopted as embodying the salient pathological characteristic of this variety. Recently, for the same reasons, the use of the term has been revived by Goldthwait, who would, however, restrict its application to the more chronic forms of rheumatoid arthritis. My clinical experience, however, accords with that of most English observers in that there is also an acute form of the malady to which the term under discussion is also applicable. Lastly, in addition to the foregoing acute and chronic types, I would also place under the same

[1] "Arthritis Deformans," Bristol, 1909.

heading of rheumatoid arthritis that group of cases known as Still's disease. I do this provisionally, inasmuch as I have not on Still's basis of differentiation found it possible to effect a satisfactory discrimination between examples of this variety and cases of rheumatoid arthritis occurring in children. Briefly stated, Still claimed that this disease could be clinically differentiated from rheumatoid arthritis by the absence of glandular and splenic enlargement in the latter, and by the presence in rheumatoid arthritis of bony thickening and lipping. It is not, however, obvious how a satisfactory differentiation can be achieved upon these lines. Thus rheumatoid arthritis, at any rate in its early stages, is not accompanied by bony outgrowths either in children or adults. Neither can any cleavage be upheld on the ground that lymphatic and splenic enlargement are absent in rheumatoid arthritis. For my own experience accords with that of others in that enlargement, at any rate, of the glands is frequent, while examples of splenic swelling are accumulating. To my mind too much stress has been laid on splenic and lymphatic enlargement as a differentiating factor, and, pending positive bacteriological findings, I am inclined to regard Still's disease as a form of rheumatoid arthritis in which the prominence of glandular swellings is to be accounted for as a peculiarity incidental to the early age of its victims, and that their presence cannot be construed as adequate evidence of its being a specific joint affection.

Passing now to consider the morbid anatomy of the above rheumatoid type, we are, I think, justified by recent anatomical researches in drawing the following conclusions:—

(1) The morbid process begins in the synovial and periarticular tissues, which undergo thickening and proliferation, but the cartilage, at any rate in the earlier stages, remains intact.

(2) The changes that ensue in the cartilaginous and bony structures are to be regarded as secondary, and are predominantly atrophic in character.

(3) In some cases proliferative changes take place in the bone and cartilages, but these occasional new bony outgrowths are inconsiderable in size.

(4) Lastly, in the terminal stages of the disease secondary atrophic changes ensue. Thus the cutaneous and capsular tissues undergo progressive shrinkage, the joint cavity becomes obliterated, with fibrous or bony ankylosis. This atrophic process involves also shafts of the bones, the cortex becoming attenuated and the cancellous tissue replaced by scanty, thin trabeculæ.

Before proceeding to consider briefly its clinical aspect, one may allude to the one salient characteristic of the ætiology of rheumatoid arthritis—i.e., the frequency with which the disease is associated with, or arises in sequence to, affections of avowedly infective or toxic character, hence the strongest presumption of the malady itself being also of like origin. From the clinical standpoint rheumatoid arthritis may be defined as a progressive polyarthritis, which runs a more or less acute or chronic course, diversified by remissions and exacerbations. The disease occurs chiefly in persons under forty years of age, and is far more common among women than men; but in children, boys, if anything, are more frequently attacked than girls. It begins in the small joints, spreading centripetally, with a tendency to symmetrical distribution. The more exposed joints become the seat of smooth, spindle-shaped swellings, due rather to periarticular thickening than to effusion, but no nodular outgrowths can be detected at the level of articulation. Eventually the enlarged joints undergo contraction or shrinkage, all the articular structures—skin, bone, and cartilage—participating in this secondary atrophic change. The temporo-maxillary and cervical articulations are almost invariably implicated. Coincidently severe wasting of interossei and extensor groups of muscles ensues, with marked exaggeration of the deep tendon reflexes. Enlargement of the lymphatic glands is not infrequent, while pigmentation, localized sweatings, and vasomotor derangements are often striking collateral features. Cardiac troubles are rare. The temperature, when raised, is of low grade, when compared with other arthritic affections, especially so when we bear in mind the widespread involvement of joints. The pulse-rate is frequently quickened, and this feature, when accompanied by pyrexia of low grade and persistent swelling, especially of swollen joints, is very suggestive of rheumatoid arthritis. Considered briefly, the foregoing constitute the cardinal pathological and clinical features of rheumatoid arthritis, and we now pass on to consider the second group, osteo-arthritis or hypertrophic arthritis.

The second group, osteo-arthritis or hypertrophic arthritis, now claims attention. From an ætiological standpoint it contrasts with rheumatoid arthritis in that infective conditions do not appear to be so intimately related to its genesis, while, on the other hand, the influence of trauma, so inconspicuous in rheumatoid arthritis, is very much in evidence in osteo-arthritis. Additional factors are to be found—senility, arterio-sclerosis, and in those cases which arise so frequently at or about the menopause. It is possible that its onset at this period may be

determined by some modification in the internal secretions either of the thyroid or ovary, or both. As to its morbid anatomy, in this variety of arthritis deformans the essential anatomical lesions are those produced in the cartilaginous and bony structures, which undergo enlargement or hypertrophy. Most authorities now agree with Virchow that the synovial tissues are only involved secondarily. Histologically, the ground substance of the cartilage undergoes a process of segmentation, while its cellular elements proliferate. Ultimately the cartilages by attrition become thinned, and their central portions, where pressure is greatest, being at length completely worn away, the denuded ends of the bones appear, and these subsequently become sclerosed and their surfaces eburnated. On the other hand, the peripheral portions of the cartilage, not being subject to such pressure, instead of being thinned become thickened, the proliferating elements forming irregular crests or nodules, which, primarily cartilaginous, ultimately become ossified. The osteophytic outgrowths thus developed are the source of the deformity of the affected joint, and are responsible also for the limitation of its mobility, but, although complete locking may occur, bony ankylosis does not ensue.

The secondary changes in the synovial membrane have been specially studied by Hoffa, who distinguished two stages in their production: (1) The first is characterized by villous overgrowth, proliferation of fixed connective-tissue cells, with extensive pigmentary deposit. (2) In the second phase, degenerative processes predominate, the synovial membrane undergoing numerous changes and ultimately complete transformation into structureless hyaline tissue, a metamorphosis said by Hoffa to be pathognomonic of the affection. The excessive deposit of pigment noted by Hoffa is of great interest, and may possibly possess some ætiological significance. Bowlby, as we know, has shown that intra-articular hæmorrhage can initiate osteo-arthritic changes in the subject of hæmophilia. Now it is quite possible in many instances of osteo-arthritis a long-forgotten trauma may have supplied the initial impulse to the morbid process. Godlee has recently adduced some striking instances in which a joint injured in early life has in later years become the seat of osteo-arthritis. Such injuries, it is conceivable, might readily determine the occurrence of a greater or less degree of intra-articular hæmorrhage, which by its presence might initiate what might be termed a reactive osteo-arthritis. It is easy to understand how, once started, the morbid process tends to perpetuate itself. Thus during the natural exercise of a joint so affected the roughened cartilaginous surfaces and bony asperities must be a source of continual irritation to the synovial

membrane, which in its turn responds by new formation of connective tissue and hypertrophy of its tufts. Again, these elongated synovial processes, either by becoming free in the cavity or from time to time becoming incarcerated between the joint surfaces, contribute to its irritability, and a vicious circle is thus established. This hypothesis as to the *modus operandi* of trauma may account for the presence of the abnormal degree of pigment noted by Hoffa.

It will be convenient here to institute a comparison between the anatomical features of rheumatoid arthritis as opposed to osteo-arthritis.

(1) In rheumatoid arthritis the initial changes are found in the cartilaginous tissues, and bony tissues are only affected secondarily.

(2) In osteo-arthritis, on the other hand, this sequence of pathological events is reversed, the primary alteration taking place in the bones and cartilages, the synovial membrane being only implicated secondarily.

(3) The character also of the changes in the bone and cartilage differs in the two affections. Those in rheumatoid arthritis are retrograde and passive, while those in osteo-arthritis are active and proliferative. In short, atrophy of bone and cartilage is the distinguishing feature in rheumatoid arthritis, hypertrophy in osteo-arthritis.

(4) Bony ankylosis occurs not infrequently in rheumatoid arthritis; rarely, if ever, in osteo-arthritis.

From the clinical aspect osteo-arthritis is essentially a disease of the middle and later period in life, being rarely met with under the age of 40. It affects both sexes equally. The mode of onset is usually insidious, and the course of the disease slow but progressive. Constitutional disturbance is slight, the temperature not raised, the pulse not quickened, while pigmentation and trophic disturbances are generally absent. Muscular atrophy is slight. The articular lesions are but rarely generalized, the tendency of the disease being to involve but a few joints, and these not symmetrically, while it shows a predilection for the larger articulations. In these respects it contrasts with rheumatoid arthritis, which is polyarticular, symmetrical in distribution, with a marked preference for the smaller joints. Moreover, when the smaller joints of the fingers are attacked by osteo-arthritis the terminal phalangeal joints are sites of election, as opposed to the marked affinity displayed by rheumatoid arthritis for the mid-phalangeal articulations. *Local characters* of the articular swellings will obviously be more easily studied in exposed articulations, by preference the mid-phalangeal; unlike rheumatoid arthritis when it affects the same articulations, the

skin over the joints presents no abnormal features, and at no period does it undergo atrophic change. Marked swelling, when present, is due to synovial effusion and not to periarticular thickening; hence we get no spindle-shaped appearance. On the contrary, the outline of the joints, instead of being smooth and rounded, is broken and irregular, owing to the presence of bony outgrowths studding the line of junction of the articular ends. Displacement of the bones, when it occurs, is usually lateral in direction, and due directly to the mechanical thrust of the bony excrescences, and not to muscular spasm. In rheumatoid arthritis, on the other hand, hyper-extensions and luxation are the rule, and for these muscular retractions are directly responsible. This diversity between the articular lesions of these two diseases becomes more pronounced in the later periods of their life-history. Thus in rheumatoid arthritis the one-time voluminous spindle-joints undergo progressive shrinkage—skin, bone, and cartilage tending to wither away. Eventually the articular structures dwindle into what Goldthwait has aptly termed the small-end joint of rheumatoid arthritis. On the contrary, in osteo-arthritis the more ancient the lesion, the more massive the bony outgrowths, the more marked the enlargement of the affected articulations.

In concluding this brief account of osteo-arthritis, one cannot help commenting on the paucity of its collateral phenomena as compared with rheumatoid arthritis. This, to our mind, constitutes the salient distinction between these two diseases. Osteo-arthritis is much more a disease of joints, and joints only, than rheumatoid arthritis, in which the arthritic changes form only one feature in a very varied clinical picture.

It now remains for us to consider the third division—i.e., infective arthritis—for it cannot be doubted that examples of this nature intrude themselves into any category of arthritis deformans, however scrupulously compiled. These cases of infective arthritis have to be considered chiefly in regard to rheumatoid arthritis, more especially its acute forms, inasmuch as it is in connexion with them that confusion is most likely to occur. Before proceeding further, it will, I think, be in the interests of lucidity if I preface my remarks by stating that I do not propose dealing with the differential diagnosis of those conditions reputed to be most frequently confounded with acute forms of rheumatoid arthritis. I refer, of course, to acute rheumatism, acute polyarticular gout, acute gonorrhœal rheumatism, and secondary syphilitic arthritis. Now, with regard to all these joint affections, if we except acute gout, their association with a

definite micro-organism has been more or less clearly established. Apart from these, however, there still remain a number of joint affections hitherto unaffiliated to any organism with which undoubtedly acute rheumatoid arthritis is very commonly confused. In this absence of any bacteriological findings, we have to rely upon those clinical features which apparently distinguish acute rheumatoid arthritis from these non-suppurative types of infective arthritis. Unfortunately, nothing can be gained from the age incidence of these two varieties of joint disorder, for both occur at all periods of life. But something may be gathered from a marked predilection of rheumatoid arthritis for the female sex. In approaching the study of such cases the previous history should be carefully elicited, especially as regards the recent occurrence of some of the infective maladies known to be followed occasionally by non-purulent forms of arthritis, such as dysentery, typhoid fever, and so forth. If such be forthcoming, it would constitute strong presumptive evidence that we are dealing with an infective arthritis following on other of these diseases rather than one of acute rheumatoid arthritis. Again, in the presence of local foci of infection, such as pyorrhœa alveolaris, otorrhœa, &c., it must not be too readily assumed that the joint condition is one of acute rheumatoid arthritis, for undoubtedly many of the instances of joint trouble occurring in such association are examples of infective arthritis. But when, as too frequently happens, no past or present source of infection can be detected, reliance must be placed on the following characteristics. Thus in infective arthritis the constitutional symptoms are often much more acute, the temperature is of higher grade, swelling of the lymphatic glands in association with the infected joints is almost invariable. The spleen is more commonly enlarged, while leucocytosis and a secondary anæmia are usually present. As regards the articular lesions, though a large number of joints may be involved in infective arthritis, in my experience such widespread invasion is not the rule. What happens in the majority of instances is that at the onset of the malady the disease attacks one, two, or three joints, and, having done so, shows no tendency to involve any other articulations, even though weeks or months may have elapsed during which the temperature may have been more or less continuously raised. Now, had the condition been one of acute rheumatoid arthritis, it would by that time have implicated all, or a large number of articulations. Again, the mode of distribution differs. In infective arthritis the large joints are by preference attacked, though I have known exceptions to this rule in the case of the small joints of the hand. In rheumatoid arthritis the disease almost invariably

begins in the smaller joints. Again, in infective arthritis the joint lesions show no tendency to symmetrical involvement, contrasting with rheumatoid arthritis.

With regard to the character of the articular lesions, nothing can be gleaned from their shape, for the spindle-shaped joints, so frequently claimed to be pathognomonic, of rheumatoid arthritis are exactly reproduced in cases of infective arthritis, when it attacks exposed joints such as the phalangeal. But the overlying skin in infective arthritis generally shows a definitive red blush, whereas in rheumatoid arthritis it is unnaturally white or bluish in colour. Again, widespread inflammatory œdema of high grade is not infrequently present in infective arthritis, and forms a very valuable aid in differentiation. Finally, apart from articular lesions, we find in acute rheumatoid arthritis, as opposed to infective arthritis, an abundance of collateral phenomena. Consequently, in those comparatively rare instances of rheumatoid arthritis in which the disease remains for some time confined to the small articulations of the hand, the following suggestion may prove helpful in distinguishing the case from one of infective arthritis: If the opposite hand, hitherto unaffected by joint lesions, is found to be the seat of vasomotor phenomena, neuralgic pains, or muscular cramps, the presence of such will be in favour of the condition being one of rheumatoid arthritis. While the foregoing, to my mind, constitute the chief points of distinction between these infective arthritides and the more acute forms of rheumatoid arthritis, it must be confessed that the dividing line is by no means sharply defined. In all probability a satisfactory differential diagnosis will not be available on purely clinical grounds, but on bacteriological data, which it is hoped will soon be forthcoming.

DISCUSSION.

Dr. FORTESCUE FOX was not disposed to attach great importance to the atrophic or hypertrophic character of arthritis, or to its commencement in the cartilages or synovial membranes, for purposes of classification. Arthritis was a degenerative joint affection, resulting from many causes, singly or in combination. In the early decades of life, when the tissues were most susceptible to infective diseases of all kinds, arthritis was commonly infective, acute, and generalized. It was part of an acute invasion characterized by rapid and progressive wasting and constitutional disturbance like that accompanying tuberculosis. At present it was often impossible to arrest this almost malignant arthritis of the young. A different clinical picture was presented by the arthritis of the middle period of life, in which infective changes were mixed with gout and the climacteric. Sometimes gout alone at this period of life set up a degenerative arthritis, usually hypertrophic, and with a favourable prognosis. The arthritis of middle life was remarkably amenable to treatment in the majority of cases. Lastly, the senile period of life had its own arthritis—"a disease of dissolution," in which infection and gouty elements had a certain place, but the main causative factor was traumatism. The arthritides of the basal joint of the thumb and of one hip or shoulder were common varieties of senile arthritis. He suggested that the anatomical distinctions upon which Dr. Jones had insisted were rather to be regarded as belonging to the *epoch* or the *phase* of the disease.

Dr. PRESTON KING could not follow Dr. Jones in his fine distinctions between rheumatoid arthritis and infective arthritis. He believed rheumatoid arthritis not to be a distinct disease so much as a train of symptoms and changes following from infections from various centres. The "soil" factor was of more importance than was apt to be recognized. The quick pulse, sweating, and pigmentation of the skin were not, in his experience, confined, as Dr. Jones suggested, to the later osteo-arthritic or hypertrophic forms of the disease, nor were hyper-extensions and ankyloses confined to, or even more common in, the earlier rheumatoid cases.

Dr. BUCKLEY (Buxton) said he recognized the existence of the three forms of arthritis deformans, but that there were many intermediate types. He believed in the infective theory, but considered that very different forms might arise from the same infection, the modifying factors being especially such constitutional states as goutiness in its widest sense, impoverished conditions of the blood such as might arise from anæmia, habitual constipation, &c., and from external causes such as trauma and general wear and tear. The possible combinations of these factors could produce many apparently differing forms of arthritis. Hypertrophy of the structures in the neighbourhood of the affected joints sometimes occurred in the later stages of cases beginning as atrophic arthritis, and this, in his opinion, indicated an attempt at repair of the damage

and was accompanied by the disappearance of the more acute symptoms. It was therefore possible that some cases which should apparently be classified as hypertrophic arthritis were due to the action of the same infective agent as the atrophic form, but that the powers of resistance in the individual were much greater. It had recently been suggested that Heberden's nodes were due to attempts to compensate for excessive wear and tear, or to repair damaged structures—a view which deserved careful consideration.

Dr. HALE WHITE thought that the previous speakers had not laid enough stress upon the extreme chronicity of the slight joint changes to be met with in some cases of infective arthritis, nor to the frequency with which these changes were due to gonorrhœa. He was of opinion that there was a definite disease to which the name "rheumatoid arthritis" should be applied. He detailed the symptoms of a typical case, and his description tallied with that given by the reader of the paper. He admitted that some cases of arthritis, in which the source of infection could be clearly traced, closely resembled those of rheumatoid arthritis, and he thought it would conduce to clearness of thinking if such cases were called rheumatoid arthritis with a prefix indicating the micro-organism which was the cause of the trouble; but in the present state of our knowledge there remained a considerable number of cases of rheumatoid arthritis, chiefly occurring in young women, the micro-organism causing which was at present unknown, but that a micro-organism was the cause he had no doubt. He had been fortunate in obtaining a post-mortem examination of such a case, and his description of the microscopic changes in the joints agreed with that given by Dr. Llewelyn Jones. The principal feature was that the disease began in the synovial membrane and in the tissues around the joint, the cartilage often not being affected.

Dr. T. FRED GARDNER (Bournemouth) drew attention to a monograph issued by Mr. Hugh Lane, of Bath, nearly twenty years ago, where the connexion between rheumatoid arthritis and tuberculous disease was well shown. The results of treatment, on the whole so disappointing, showed that remedies like cod-liver oil, arsenic, cream, fats, &c., had a beneficial effect on some cases. A history of arrested tuberculous lymphadenitis was of very common occurrence in rheumatoid disease. He also pointed out the striking fact that in advanced cases of rheumatoid disease changes occurred in the spinal cord indistinguishable from bulbar disease, pointing to an ascending degeneration. One such case in the speaker's experience exhibited the classical symptoms of bulbar paralysis.

Mr. GORDON WATSON believed that Dr. Jones divided arthritis deformans into two main classes—the synovial (rheumatoid) and the bony (osteo) arthritis. Broadly speaking, he would like to argue that these two main classes were quite distinct in their pathology—the former infective, the latter traumatic. Of the latter class he had seen many examples, following definitely on injury to a healthy single joint in young people. During the past week he had operated in a case exhibiting the classical changes associated with osteo-arthritis. The knee had been injured, and the internal semilunar cartilage torn. Recurrent

attacks of "locking" and subsequent synovitis had resulted in well-marked osteo-arthritis, with fibrillation of the articular cartilage of the femur and osteophytic outgrowths from the margins of the tibia. These changes, it seemed clear, had resulted from the chronic irritation which the internal derangement produced. He would like to see a sub-committee of the Balneological Section appointed for the purpose of defining, on a pathological basis, the many varieties of arthritis deformans.

Dr. F. H. HUMPHRIS, while admitting that there are some cases in which treatment seems unsatisfactory—he would not say useless—thought some attention should be drawn to two therapeutic agents which at any rate afford relief from the pain and stiffness. He had had six cases of rheumatoid arthritis referred to him during the last three months, and in none of them did he fail to get some measure of relief. They were treated with high candle-power electric light placed in a suitable reflector, followed by an administration of static electricity.

Mr. MIDELTON said he was quite sure Dr. Llewelyn Jones had excellent reasons for mapping out the types of arthritis he had submitted for criticism. He (Mr. Midelton) believed thoroughly in continuous counter-irritation to the spine and joints, and, if necessary, to other parts of the body, in the treatment of arthritis deformans, as suggested by Dr. P. W. Latham and others. It mattered not how long the disease had existed, or its variety; he approached it with confidence provided there was no serious complication, such as tuberculosis or cancer. He had twenty-seven cases under his care, of one sort and another, being treated by means of continuous counter-irritation. He would welcome any member of the Section at Bournemouth, and would show them as many cases as possible.

Dr. SOLLY said that he believed that all cases described as rheumatoid or osteo-arthritis, atrophic or hypertrophic, were due to infection. The differences were due to variations of degree rather than kind, and while in some cases it was comparatively easy to recognize the particular form of infection, as, for instance, in gonorrhœal arthritis, in others the actual organism could not be ascertained; but all the evidence gradually accumulating seemed to favour the view that all arthritis was primarily infective. A mixed infection was probably always present, and the combined action of various organisms, mixed in different proportions, would explain many of the variations in the classes of symptoms. Many forms of arthritis cleared up rapidly after the removal of some apparently trivial but constant source of septic infection, such as pyorrhœa alveolaris, or chronic nasal or tonsillar catarrh. Chronic colitis and chronic leucorrhœa seemed to be responsible in cases which had come under his observation; and, without going into the question of "appendix dyspepsia," it was quite certain that there were a great many people going about the world affected with chronic catarrh or even definite ulceration of some of their mucous membranes sufficient to provide a constant source of insidious septic infection, without being actually aware of the fact. It would be in everyone's range of

experience that persons have become suddenly ill and died from hæmorrhage from a duodenal ulcer of which there had been no previous indications recognizable. That was only one illustration, and, while arthritis of the "rheumatoid" type could be *proved* to be due to septic infection in a considerable number of cases, he considered it absolutely impossible for anyone at present to disprove the existence of some infection in any case. It should also be remembered that the early stages of the disease, as in the case of infantile paralysis, are often overlooked, and it is the track of the storm and not the storm itself which attracts the most attention.

Dr. ACKERLEY, though he acknowledged that there was some convenience in using a comprehensive term, thought that it would be wiser if, when a case of arthritis was seen, it was called simply an arthritis, and an endeavour were made to discover the cause or causes in that particular case. The arthritis was only one manifestation of some disease. But what was much needed was a more thorough study of the soil. Recognizing the ætiological factor of gonorrhœa, pyorrhœa alveolaris, and other forms of sepsis, what was it that determined an arthritis in one man and not in another? He doubted whether there was a real distinction between acute and chronic cases. Was it not possible that the chronic forms simply depended on a smaller daily dose of the particular toxin, or greater personal resistance to its effects?

The discussion was adjourned until the next meeting.

Balneological and Climatological Section.

April 20, 1910.

Dr. LEONARD WILLIAMS, President of the Section, in the Chair.

Discussion on Arthritis Deformans.[1]

Dr. A. E. GARROD, in resuming the discussion, said: Of the speakers who have up to now taken part in this discussion, the majority have expressed themselves as unable to accept the view, so ably advocated by Dr. Llewelyn Jones in his opening paper, that under the name of "arthritis deformans" we have been in the habit of including several distinct morbid states—distinct alike in their pathological features, in their clinical course, and in the agencies at work in their causation. Some have favoured the opinion that the conditions referred to are merely varieties of a single malady, modified in its features and course by the age of the patients, and by those subtle constitutional differences of tissue and metabolic process which formed the bases of that doctrine of diatheses, which filled so large a place in medical thought in the past, and which has been too little taken into account of recent years. We have been reminded, and the reminder is doubtless called for, that we must take into consideration the soil as well as the seed, and I am far from wishing to minimize the importance of the soil factor; but we must also remember that profoundly as the nature of the soil may modify the crop, it is the seed which determines its nature. Whatever the quality of the soil, if we sow wheat we shall not reap oats nor barley. It can hardly be questioned that the chapter of chronic diseases which affect the joints is an opprobrium of medicine and the despair of medical students, who find it hard to obtain from what they read in their text-books any clear light upon what they see with their eyes. Undoubtedly the accurate discrimination of these diseases presents great difficulties, and in many individual cases is beyond our reach, in the present state of our knowledge. If, however, daunted by these difficulties, we are content to fold our hands and abandon the attempt, we shall certainly make no progress in bringing order out of chaos. Let us recall for our encouragement the history

[1] Adjourned from March 9, 1910.

of our knowledge of articular diseases: the fact that rheumatic fever itself was not clearly differentiated before the seventeenth century, and that in comparatively recent years the view was advocated, by distinguished physicians and surgeons, that gonorrhœal arthritis was merely acute rheumatism modified in its course by the presence of gonorrhœa. There is indeed no chapter of medical science in which ancient traditions and popular opinions exercise so baleful and obstructive an influence as in this. In no chapter is nomenclature so confused and so misleading. It would, indeed, be a great advance if the names now in use could be wholly swept away, and if medical writers in different lands could agree upon a new international nomenclature of these maladies. As it is, authors writing in English, French, and German can make comparatively little use of each other's writings, and even the non-committal name "arthritis deformans" is used in different senses. In Germany it signifies the condition of which Dr. Jones has spoken as "osteo-arthritis." I believe that, in its main features, the classification adopted by Dr. Jones is as good a one as can be brought forward at the present time. It does not err, as some of the more recent classifications have done, on the side of too great elaboration, whilst it brings out the most important lines of cleavage. For my own part, I believe, with several previous speakers, that there is a distinct morbid item, a specific malady, presumably infective, of which we may speak as "rheumatoid arthritis," the leading features of which were admirably summed up by Dr. Hale White in his remarks at the previous meeting. I am convinced that a variety of other arthritic lesions, of bacterial origin, which run a more or less chronic course, may simulate rheumatoid arthritis more or less closely, and in individual cases may only be distinguished from it with difficulty, or may fail to be differentiated. These constitute the group of which Dr. Jones has spoken as "infective arthritis," a name which is open to the objection that rheumatoid arthritis itself is, in all probability, an infective disease. Lastly, I am convinced that the malady called "osteo-arthritis" by Dr. Jones, the "arthritis deformans" of German writers, is wholly distinct from those already referred to—distinct alike in its pathology and clinical features—and that the first essential to any satisfactory progress is the clear recognition of this fact. In order to avoid attempting too much in the time at my disposal, I propose to direct my remaining remarks to this single point.

I do not suppose that any of those present will question the existence of these two sharply-defined classes of cases, whatever interpretation may be placed upon the differences between them. In the one set the swelling of the affected joints is largely peri-articular, the swellings are fusiform, and no bony enlargement can be made out. In the other the changes in the bones are the conspicuous features; lipping of, and osteophytic outgrowths upon, their articular endings are clearly felt through the skin, and the surrounding soft parts are little affected. As a consequence the swellings commence more abruptly, and do not assume the spindle-shape. The Röntgen rays confirm the impressions gained by inspection and palpation. In the osteo-arthritic cases the bony outgrowths are clearly shown in the radiograms, whereas in rheumatoid cases

the articular surfaces may appear normal, although blurred in outline; but the bones are usually unduly translucent, as in other forms of infective arthritis, and the extent of the peri-articular swelling is indicated by the faint shadows which the soft parts throw. In both conditions deformities result as the disease progresses. Those seen in osteo-arthritic cases are mainly the result of bony changes, the effects of the osteophytic enlargements and of displacements to which they give rise. Limitation of movement also is mainly due to a similar cause, and to gross changes in the articular ends of the bones. In rheumatoid arthritis, on the other hand, the deformities, which take the form of deflexions and subluxations, are chiefly due to contracture of muscles which have undergone the conspicuous atrophy which is so prominent a feature in such cases. The distribution of the lesions is also different, for although in both conditions symmetry of incidence is the rule, and certains joints which are little liable to be attacked by other diseases, such as the temporo-maxillary, may suffer in either, each has its own seat of election. The differences of distribution are best seen in the hands. Osteo-arthritis specially attacks the end-joints of the fingers, producing the well-known Heberden's nodes; less commonly the proximal inter-phalangeal joints are also attacked, but seldom or never apart from the end-joints. The carpo-metacarpal joint of the thumb is another seat of election of the osteo-arthritic process, and may be the only joint which suffers. In rheumatoid arthritis, on the other hand, the proximal inter-phalangeal and metacarpo-phalangeal joints chiefly suffer, the end-joints comparatively rarely. These differences of distribution, taken in conjunction with differences of character of the lesions, give quite different appearances to the osteo-arthritic hand and that of rheumatoid arthritis. The clinical differences referred to go hand in hand with differences in morbid anatomy. The osteo-arthritic changes, fibrillation of cartilages, eburnation of bones, osteophyte formation and hypertrophy of synovial fringes, form a well-recognized clinical picture, and are met with in the one class of cases. On the other hand, in the rare instances in which opportunities are afforded of examining the joints of sufferers from rheumatoid arthritis in its active stages, the above lesions are wholly wanting. The cartilages are found intact, or are merely pitted at their edges by pressure of swollen fringes; there are no condophyles nor osteophytes, as the radiograms show during life. Even in the later stages, in cases in which the active disease is at an end, although pronounced deformities and crippling bear witness to its passage, although the cartilages may be wholly destroyed, and bony ankylosis may have occurred, the changes met with bear no close resemblance to those of osteo-arthritis. Nor must we limit our survey to the articular lesions. In rheumatoid arthritis, and especially in its more acute forms, there is conspicuous constitutional disturbance. Febrile disturbance of greater or less degree is the rule, and there is often a progressive secondary anæmia. Muscular atrophy is often extreme, smeary-brown pigmentation is common, and there may be enlargement of lymphatic glands and of the spleen, especially when the disease attacks children. In osteo-arthritis, on the other hand, constitutional and abarticular symptoms are practically absent, except

in so far as the patient's health may suffer from enforced inactivity in severe cases.

It can hardly be contended, even by those who regard the two conditions as the outcome of the same morbid process, as varieties rather than species, that the one is merely the sequel of the other. Clinical experiences, as well as dissection and examination by Röntgen rays, show that the cases which have been spoken of as osteo-arthritic have their distinctive features from the onset, and that these features do not develop in the later stages of the cases of the rheumatoid group; and, although it is quite conceivable that the changes in the characters of the affected joints might undergo profound alteration as the disease progresses, no such explanation is possible when we take into account the differences in their distribution. There remains the possibility that the structural and clinical differences may be due, not to the action of different morbid agencies, but to different soils upon which the seed falls. It has been suggested that such differences of soil may be due to the age of the patient at the time of attack. I should be the last to question the influence of personal factors, and among these of the ages of patients in modifying the features of disease. The natural history of acute rheumatism as seen in childhood, in adult and in advanced life, proclaims the potency of such influences and I believe that the differences in the clinical features of rheumatoid arthritis, as seen in children and in adults respectively, offer a further example of their working. It is unquestionably true that rheumatoid arthritis, like other infective maladies, has its chief incidence upon younger subjects, whereas osteo-arthritis is usually seen in those who are entering upon the second half of life.

A comparison may be made between these joint troubles and the renal affections which used to be classed together under the collective name of Bright's disease, but which are now clearly differentiated. Acute nephritis, a bacterial disease commonest in early life, and which may leave the kidneys permanently and gravely damaged, may be compared to rheumatoid arthritis; whereas granular kidney and osteo-arthritis are both members of a group of maladies met with in later life, and which may have their origin in the cumulative effects of minimal absorption of toxic materials, or of constantly, repeatedly insignificant infractions of the inexorable laws of health. Just as granular kidney is occasionally met with in the young, and even in children, so also cases of osteo-arthritis, with its characteristic features, sometimes occur at early ages; and the more acute forms of rheumatoid arthritis are observed in some elderly people in whom one would rather expect the osteo-arthritic lesions. Yet in both instances the distinctive characters are preserved, and the view that the age of the patient determines the features of such cases appears to me to be quite untenable.

The only arguments which have any serious weight in support of the essential identity of rheumatoid and osteo-arthritis are these—viz., that in some cases the special features are so little pronounced that it is difficult to determine to which group the cases should be referred, and that, as is undoubtedly the case, the characteristic lesions of both maladies are sometimes

present, side by side, in the same patient. The argument from difficulty of diagnosis applies with equal force to a number of morbid conditions, other than joint diseases, the essential distinctness of which is universally admitted, and among these to parenchymatous nephritis and granular kidney. The imperfections of our diagnostic methods afford a very insecure ground for the assertion of the identity of morbid processes: but the argument from simultaneous occurrence is entitled to much more weight. I have myself seen cases in which, in elderly people, some of the joints have exhibited the clinical and radiographic features of osteo-arthritis, whereas in others the changes which one associated with rheumatoid arthritis have been equally well seen. Characteristic Heberden's nodes co-existed with fusiform periarticular swelling of the joints of the proximal row. A little inquiry will, in some cases at any rate, elicit the fact that the osteo-arthritic lesions are of long standing and have developed insidiously, whereas the development of the rheumatoid lesions is much more recent and is dated from a definite onset; and I believe that the patients in question are sufferers from two independent articular diseases. Nor is it remarkable, seeing how frequent osteo-arthritic changes are in elderly subjects, that such overlapping should sometimes occur. I am very conscious that this contention is, of those brought forward, that which has least chance of carrying conviction to my hearers, and am prepared to hear it spoken of as far-fetched. It is difficult to state the grounds for one's conviction on such a point, but that conviction is based upon observation and inquiry, carried out, I believe, without any undue bias, and will, I believe, be endorsed by any who will take the trouble to make inquiries as to the course of the articular lesions in similar cases.

The question whether the diseases under consideration are distinct or not is not merely of scientific and academic interest, but is of real practical importance. Upon the answer which it receives will depend our whole point of view, and not only will our diagnosis be influenced thereby, but also the prognosis which we offer and the lines of treatment which we adopt.

Dr. P. W. LATHAM (Cambridge): In the *Lancet* of April 6, 1901 (p. 998), I advanced the theory that the changes in the joints and the muscular wasting, which are characteristic of rheumatoid arthritis, are due to irritative and destructive lesions located in the spinal cord, and especially in the cervical and lumbar enlargements. What evidence can I produce in support of this theory? According to Ross: "Pathological facts appear to show that the trophic nerves of the bones and joints are found in the mixed nerve-trunk, and that they issue along with the motor fibres from the anterior cornua of the spinal cord, where they are, like the muscular trophic fibres, connected with a group of large caudate cells;"[1] and that "it seems now generally admitted that both irritative and destroying lesions of the ganglion cells of the anterior horns of the spinal cord and their homologues in the medulla oblongata, pons, and crura, or of the

[1] Ross: "Diseases of the Nervous System," 1883, 2nd ed., i, p. 267.

efferent fibres which unite these cells with the periphery, may give rise to active atrophy of muscle."[1] Again: "Pathological observations render it probable that the ganglion cells of the anterior horns exercise a controlling influence upon the nutrition of the bones and joints, while those of the posterior horns stand in a similar relation to the nutrition of the skin and its appendages."[2] We must also bear in mind that the nutrition of the muscles and bones is dependent upon the vascular supply, and that this vascular supply is controlled by vasomotor action which can be influenced by nervous impulses either originating in the central nervous system itself, or started by afferent impulses carried for the most part by ordinary sensory nerves passing up the central nervous system from some sentient surface in any part of the body. This vasomotor action of the central nervous system may also be directly affected by the condition of the blood passing through it, or by substances introduced into the blood. Now, throughout the whole length of the spinal cord intermixed with the reflex and other mechanisms we have a number of minor vasomotor centres, the action of which, though to a certain extent independent, is ruled and co-ordinated by the dominating or head centre which, as we know, is located in the medulla or bulb. The paths taken by the vasomotor fibres have not as yet been fully made out. They leave the spinal cord by the anterior roots of the spinal nerves, but in the majority of cases the fibres do not run in a direct course to their destination in company with the ordinary motor fibres passing to the same structures as themselves. The vasomotor fibres of the lower limbs, for instance, do not pass directly with the anterior roots into the sciatic nerve, but largely, at all events, turn aside to join through the rami communicantes the abdominal sympathetic; and it is only after they have traversed a certain length of sympathetic nerve that they again return to the spinal nerves, enter into the sciatic plexus, and thus become part of the nerves of the leg; that is, an afferent impulse proceeding from the leg may, through the abdominal sympathetic, give rise to efferent vasomotor impulses acting on other parts of the body besides the spot from which the afferent impulse springs. So, also, the vasomotor fibres for the fore-limb pass in large measure from the anterior roots of the upper dorsal nerves to the thoracic sympathetic chain, and thence by the first thoracic ganglion to the brachial plexus, and so on to the fore-limb. How far this vasomotor action is in harmony or even identical with the action of the so-called trophic nerves I cannot tell. What I want to suggest is that if from any sentient part of the body an afferent impulse is conveyed to the brain, ascending by the posterior roots of the spinal cord, reflex impulses will descend along the vasomotor fibres in the cord, pass out with the motor fibres by the anterior cornua, and give rise to *some action or molecular change in their ganglion cells*, then the rami communicantes joining a sympathetic ganglion, impulses will radiate therefrom not only to the part from which the afferent

[1] Loc. cit., i, p. 241.

[2] Loc. cit., i, p. 825.

impulse sprung, but to other parts of the body in connexion with that ganglion, modifying the nutrition of the parts to which they are distributed, by increasing or diminishing the supply of blood to these parts. Further, that if there are irritative lesions either of the ganglion cells of the anterior horns, or of the efferent fibres passing on to the periphery, similar results would be produced to those I have described. If there are destructive lesions, the same results would follow as are produced by section of a nerve trunk, results with which in the case of the sciatic nerve or the brachial plexus we are familiar.

Now, as regards the effect of irritative lesions of the pelvic organs, Dr. W. M. Ord, in a paper read before the Clinical Society, February 14, 1879,[1] suggests that there is evidence to show that there exists a relation of cause and effect between uterine disorders and arthritis. The first case which attracted his attention in this respect was one of severe arthritis with severe dysmenorrhœa; the arthritis being regularly developed paroxysmally immediately before, throughout, and for a short time after each menstruation, and as regularly intermitted in the intervals. After much treatment had been ineffectually applied on the primary rheumatic hypothesis, the uterine conditions were carefully and steadily attacked for a twelvemonth. At the end of this time the dysmenorrhœa was overcome and the arthritic paroxysms soon after ceased to occur, and the patient, from being wasted, sallow, anæmic and hysterical, became an active, happy, and decidedly well-nourished member of society. He refers to thirty-three cases of rheumatoid arthritis combined with decided symptoms of uterine disorder or irritation. In a considerable proportion of the cases (fourteen) the arthritic paroxysms coincided with the menstrual periods. In seven there was dysmenorrhœa, which was conspicuously absent in the other seven. He noticed that in three cases in which the arthritis was limited to, or began and remained excessive in, one side of the body, the ovary on the same side was painful and tender. Dr. Ord concluded that, "inasmuch as the joint affection in many cases ebbs and flows with the uterine affection, and may alternate with affections of the higher centres (as is shown in a remarkable case of mania which he records), the disorder must be regarded as the product of reflex action through the nervous system." I agree with him, and believe the explanation is to be found in the reflex impulses travelling along the vasomotor nerves in the manner I have above endeavoured to describe, modifying the blood-supply and so the nutrition of the muscles and tissues to which those vasomotor nerves are distributed.

Other forms of irritation in the pelvic organs may also give rise to the disorder. The irritation produced by gonorrhœal urethritis, or by simple urethritis, can by reflex action produce in both cases an arthritis identical in its morbid anatomy with rheumatoid arthritis. Of course, in gonorrhœal arthritis, the changes in the joints are intensified by the presence in the blood of the morbid products excreted by the gonococci. Undoubtedly, in all these cases, the arthritis occurs most frequently where conditions involving irritability or disordered nutrition of the spinal cord exist, such as anæmia, nervous

[1] *Clin. Soc. Trans.*, 1879, xii, p. 90.

exhaustion, or general weakness. It is not unreasonable to assume that if there are continuous afferent and efferent impulses, or intermittent impulses frequently repeated, passing along the spinal cord, the cells associated with these impulses will become exhausted, and morbid changes or nutritional changes will be developed in the ganglionic cells of the anterior and posterior cornua of the nerve along which these impulses travel. The muscular atrophy associated with rheumatoid arthritis (but which sometimes precedes the joint affection) is regarded by some as determined by morbid impulses from the joint-nerves. Sir W. Gowers says: "The nature of the cause of ordinary arthritic atrophies long ago suggested to Paget a 'reflex influence' on the muscles as the probable mechanism, and Vulpian and Charcot have suggested a similar hypothesis, assuming a derangement of the nutrition and influence of the motor cells of the cord to be determined by the morbid impulses from the joint-nerves, and to determine the alterations in the muscles. The theory receives important support from the fact that the wasting of the muscles is prevented by previous division of the posterior spinal roots,[1] which amounts indeed to an indirect demonstration of the fact that a reflex process underlies the phenomena."[2] I suggest that in the early stages at least of rheumatoid arthritis the morbid impulses have their origin in the pelvic organs or in other parts of the body, it may be even in the nerves themselves, as I will now proceed to show.

Clinical experience has taught us that injuries to the spinal cord, or even to the nerves of a limb, will set up arthritic changes which cannot readily be distinguished from those seen in the earlier stages of rheumatoid arthritis. Dr. Weir Mitchell, Dr. Moorhouse, and Dr. Keen were the first to call attention to the effect of nerve injuries on the nutrition of the joints. The symptom occurs within a few days after the injury to the nerve. "It consists essentially in a painful swelling of the joints, which may attack any or all of the articulations of a member. It is distinct from the early swelling due to the inflammation about the wound itself, although it may be masked by it for a time; nor is it merely a part of the general œdema which is a common consequence of wounds. It is more than these—more important, more persistent. Once fully established, it keeps the joints stiff and sore for weeks or months. When the acute stage has departed, the tissues about the articulations become hard, and partial ankylosis results, so that in many cases the only final cause of loss of motion is due to this state of the joints. Of all the agencies which impede movement it is the most difficult to relieve. Were we asked to state in what essential respect these lesions differ from subacute rheumatic disease of the same parts, we should certainly be at some loss to discern a difference."[3] In a subsequent work[4] Dr. Weir Mitchell again refers to the curious arthritic changes which

[1] Raymond: *Rev. de Méd.*, 1890, p. 374.

[2] Sir W. Gowers: "Diseases of the Nervous System," 1899, 3rd ed., i, p. 561.

[3] "Gunshot Wounds and other Injuries of Nerves," by Dr. Weir Mitchell, Dr. Moorhouse, and Dr. Keen, Philad., 1864, p. 83.

[4] "On Injuries to Nerves," Lond. and Philad., 1872, p. 170.

may follow dislocations, ball-wounds, contusions, or even compressions of the nerves. "We may have one articulation—and if only one, a large one—involved, or perhaps all the joints of a finger, or every joint in the hand or of the entire limb may suffer. The swelling is never very great, the redness usually slight, and the tenderness on touch or motion exquisite. This condition of things remains with little change during weeks or months, and then slowly declines, leaving the joints stiff, enlarged, and somewhat sensitive, especially as to movement. A small proportion of such cases find ready relief, but in many of them the resultant ankylosis proves utterly unconquerable, so that it is vain to break up the adhesions under ether, or to try to restore mobility by manipulation on splints. All alike fail, and serve only to add to the essential tortures of the accompanying neuralgia and hyperæsthetic states of skin. Since writing my last paper I have met with some of the former patients who suffered with these troubles, but in no case, originally very severe, was there any great pain; indeed, in most of them, the joints had become every year more stiff and useless. It is then quite clear that injuries of the spine, diseases of this organ and of the brain, and wounds, or any form of lesions of nerves, are capable of developing in the joints inflammatory conditions, usually subacute, and which so precisely resemble rheumatic arthritis in their symptoms and results that no clinical skill can discriminate between the two." Various examples illustrating the above statements will be found in Dr. Weir Mitchell's work.

The arthropathies of hemiplegic patients described by Scott Alison,[1] Brown Sequard,[2] and Charcot[3] are further examples of arthritic mischief resulting from lesions in the central nervous system. Again, concussion of the spine from falling from a height or in skating may also induce arthritic troubles. In the *Lancet* of April 6, 1901, I have recorded an instance of this kind which is interesting both as regards the symptoms and the treatment. And in *Guy's Hospital Reports*[4] Sir William Gull reports Case 28, concussion of the spine; partial paraplegia, redness and swelling of the wrists and ankles as in acute rheumatism. He also reports Case 27, one of acute rheumatic (?) affection of the larger joints, paraplegia, slough over sacrum. He remarks: "It is a matter of great clinical interest that lesions of the cord are occasionally attended by an affection of the joints not to be readily distinguished from that which occurs in acute rheumatism." In Case 27 "fatigue from mechanical labour (mangling), acting especially on the lumbar and dorsal portions of the spine, in a delicate and anxious subject appears to have injured the nutrition of the cord. For two years, when much exerting herself, the patient felt pains between the shoulders and a sense of constriction and coldness round the chest. Paraplegia then suddenly came on followed by redness, pain and swelling of the larger joints, as in rheumatism." The clinical symptoms, then, of chronic arthritis furnish, it

[1] "Arthritis occurring in the Course of Paralysis," *Lancet*, 1846, i, p. 278.

[2] *Ibid.*, 1861, ii, p. 29.

[3] "Lectures on Diseases of the Nervous System," New Sydenham Society, 1877, p. 93.

[4] *Guy's Hosp. Reports*, 3rd ser., 1858, iv, p. 198.

seems to me undoubted evidence of pathological change in the nervous centres, arising from irritative impulses, starting either in the afferent fibres or in the brain itself, and giving rise to nutritive or organic changes in the ganglion cells of the posterior or anterior cornua, or both, of the spinal cord. Nutritional changes, of course, would not be visible even on microscopical examination; but, in some instances, skilled pathological observers have discovered organic changes in the cells of the spinal cord. Charcot[1] reports two remarkable cases of locomotor ataxia which were complicated with unilateral arthropathy. He says: "It is not very rare to find the spinal grey matter affected in locomotor ataxia; but the lesion is then generally found in the posterior cornua. Now, it was quite different in two cases of locomotor ataxia, complicated with arthropathy, in which a careful examination of the cord had been made; the anterior cornua were, in both cases, remarkably wasted and deformed, and a certain number of the great nerve-cells, those of the external group especially, had decreased in size, or even disappeared altogether without leaving any vestiges. The alteration, besides, showed itself exclusively in the anterior cornua, corresponding to the side on which the articular lesion was situated. It affected the cervical region in the first case, where the arthropathy occupied the shoulder; it was observed a little above the lumbar region in the second case, which presented an example of arthropathy of the knee. Above and below these points the grey matter of the anterior cornua appeared to be exempt from alteration."

In an article in *Brain*[2] entitled "Some Observations on Primary Degeneration of the Motor Tract," Dr. F. W. Mott, F.R.S., and Mr. A. F. Tredgold report a case of chronic rheumatoid arthritis with muscular atrophy, giving the following account of the microscopical examination of the cord:—

"*Microscopical Examination.*—Spinal cord: Sections were examined at the level of the sixth, seventh, and eighth cervical; first, tenth, twelfth dorsal; second and fifth lumbar, and first sacral segments. Grey matter: In the cervical enlargement there are very important alterations in the large cells of the anterior horns. There are fewer such cells present than in the normal cord in this region, and a large number of them are seen to be undergoing a chronic degenerative change, with the deposition of a large amount of pigment. There are some fairly healthy cells scattered amongst the atrophied ones; the process also does not pick out any special group of cells, but affects apparently to an equal extent all the different groups. In the dorsal segments examined there is not anything like the same amount of change in the anterior horn cells; here and there degenerated cells occur, but they are comparatively rare, whereas in the cervical and lumbar parts of the cord the reverse is the case, and healthy cells are in a decided minority. In the lumbo-sacral portion of the cord exactly similar changes occur as in the cervical enlargement—that is, many cells in various stages of degeneration are scattered throughout all the cell nests. The mesial-anterior group certainly appears to be as much if not more affected than

[1] "Lectures on Diseases of the Nervous System," New Sydenham Society, 1877, p. 100.

[2] *Brain*, 1902, xxv, p. 418.

any other, unless it be the postero-external. In the cervical and lumbar enlargements there is also very considerable decrease in the fibres composing the network of the anterior horns, especially in the central portion. This change, which does not occur in the dorsal region, causes a very obvious pallor of the sections stained by Weigert's method. The posterior horns, the cells and reticulum of Clarke's column, and the other portions of the grey matter, appear perfectly normal in all regions of the cord. Anterior roots : The anterior nerve roots in the cervical and lumbar regions are much atrophied." The reporters conclude with the remark : " We are unable to say whether the change in the anterior horn cells is primary or secondary to the joint change, or to give any satisfactory explanation of this difference ; we simply record the fact."

The microscopical appearances, as described in the above cases, confirm, I think, the view I have put forward as to the pathology of rheumatoid arthritis—namely, that it is due to spinal irritation or congestion, or chronic myelitis chiefly affecting the ganglion cells of the anterior horns, but extending also, when the disease is associated with " glossy skin," to the ganglion cells in the posterior horns. If this is the case, the natural inference would be that, in the earlier stages of the malady at all events, the treatment should be directed to the abatement of this spinal irritation, and that probably this can be most effectively done (the exciting cause being removed) by cupping or blistering the spine. This is no new suggestion. As far back as 1831 a paper appeared in the *American Journal of Medical Sciences*,[1] by Professor J. K. Mitchell, the father of Dr. Weir Mitchell, " On a New Practice in Acute and Chronic Rheumatism." In this paper he describes cases of chronic rheumatism arising from various causes, and cases of arthritic troubles following spinal injury, which were successfully treated by cupping and blistering. Eight to sixteen ounces of blood were abstracted from the neighbourhood of the cervical or lumbar enlargements according as the upper or lower extremities were affected, and if the cupping did not afford relief blisters were applied to the same regions. I rarely have had recourse to cupping, but I have found continuous counter-irritation a most valuable remedy. To obtain beneficial results, however, in rheumatoid arthritis, the counter-irritation must be pronounced and prolonged—slight irritation is useless, and it must be in the neighbourhood of the cervical and lumbar enlargements. Applied, for instance, to the mid-dorsal region, it is useless, as was pointed out by Dr. J. K. Mitchell eighty years ago in the paper above referred to.

The following is the plan I usually adopt: Two cantharides plasters, 4 in. or 5 in. long by $2\frac{1}{2}$ in. wide (painted over immediately before their application with linimentum cantharidis or liquor epispasticus to insure rapid action), are applied at bedtime, one on each side of the seventh cervical vertebra or the twelfth dorsal according as the joints of the upper or lower extremities are most affected. The blisters are kept on for twenty-four hours. They are then removed, the loose cuticle cut away, and a bread poultice made with boracic

[1] *Amer. Journ. Med. Sci.*, Philad., 1831, viii, p. 55.

lotion applied for the night. In the morning the blistered surface is dressed with *freshly* prepared savin ointment spread on lint, and the dressing repeated every morning for eight or ten days. Each time, before removing the lint, it should be well soaked with boracic lotion so as to prevent its adhering to the blistered surface and causing unnecessary pain to the patient. I generally administer twenty drops of laudanum the first night on applying the blisters, and, if the joints are painful, repeat the dose every night. If the joints both of the upper and lower limbs are affected, then seven or eight days after applying the two blisters, two others are applied one on each side of the seventh cervical vertebra if the first have been applied to the dorsal region, and vice versa, carrying out the same routine as before, the morning dressing of these blistered surfaces with savin ointment being repeated for eight or ten days. The pain and swelling of the joints is generally much relieved in three or four days after the application of the blisters, even when no laudanum has been given, but the improvement is more marked both as regards the swelling and the pain when the narcotic is given. If the patient is suffering much pain, instead of the single evening dose I give ℥x of laudanum together with ℥xxx of the liquor hydrargyri perchloridi every six hours. Whilst the counter-irritation is being carried out, it is most important that the patient should have complete rest in bed, the affected limbs being moved as little as possible, especially if movement causes pain. At the end of eight or ten days the healing of the blistered surfaces may be promoted by the application of some simple dressing. The patient's general health must be attended to, the diet must be such as can be easily digested—beef, mutton, fowl, &c.—with such alcoholic stimulants as are suitable. A daily action of the bowels must be ensured and a proper amount of sleep must be obtained, if necessary by the regular administration at bedtime of opium or morphia. As the patient improves, massage, warm baths, radiant heat, &c., are important auxiliaries in the treatment, but on no account must massage be employed if it causes pain in any of the joints. As regards medicine, I have found the liquor hydrargyri perchloridi useful in doses of 1 dr. twice a day with some bitter infusion. Sometimes ℥iii to ℥v of liquor arsenicalis may be combined with the perchloride solution with advantage, but it does not agree with all patients. Not infrequently it may be necessary, especially in severe and chronic cases, to repeat the blistering process again after an interval of two or three months. I need hardly say that it is of the utmost importance that before using cantharides or opium the condition of the kidneys should be carefully investigated. If they are unhealthy, this plan of treatment is absolutely unsuitable. The electric cautery may then be employed instead of the cantharides.

Such, then, is the plan of treatment which in my hands—and I am thankful to say in the hands of others, notably Dr. Gurney Champion and Dr. W. J. Midelton, both of Bournemouth, and Dr. Alexander, of Buxton—has proved successful. Patients ailing for three, five, and ten years, some "thin, wasted, bed-ridden and wishing to die," have been restored with "good movement in all the joints, and able to walk three or four miles with ease." Such is

Dr. Champion's report to me. Sometimes the diminution of pain and swelling of the joints very quickly follows this plan of treatment. In one case, four days after the application of a "blister to the cervical region, the patient's wedding ring slipped off her finger. She had been for six months previously unable to move it, owing to the swollen condition of the phalangeal joint." I have myself seen a similar result. Time does not permit me to submit to you notes of some cases successfully treated after this method. The *Lancet* of April 6, 1901, contains reports of two of my earliest cases, and of one also under Dr. Arthur Latham at St. George's Hospital. In the *Lancet* of September 28, 1907, Dr. Midelton reports several cases, one of which in particular possesses features of extreme interest: A married woman, aged 43, ailing for two years, wasted to a skeleton, and unable to move hand or foot, every joint in the body stiff and painful, and pronounced by several medical men to be incurable. Counter irritation of the spine was employed, and repeated twice at intervals of two and three months. At the end of twenty months the patient was well nourished and active, and able to resume her household duties.

Mr. WM. ARMSTRONG (Buxton) said that he would like to ask Dr. Llewelyn Jones and Dr. Garrod what was meant by the term "infective arthritis." Did they suggest that a specific micro-organism was in evidence? He himself did not believe in this theory. Four different observers had described four different micro-organisms. Surely, if there were a specific bacillus, some benefit would have been obtained by means of treatment by the various vaccines that had been prepared; while, so far as he could learn, no success had been achieved by the use of these. He was a strong adherent of the theory so ably put forward by Professor Latham, and considered that in all these cases there was an interference with the nerve nutrition of the joints due to disturbance of the junctions of the joint centres in the cord; that these centres were upset by various reflex irritations, such as gastro-intestinal toxins, utero-ovarian, nasal, pharyngeal and other disturbing factors. He had had considerable success in treating arthritis by counter-irritation. He used the electric cautery freely to each side of the joint centres, described by Professor Latham, having first produced local anæsthesia by ionization, at the same time using every endeavour to discover and do away with the various reflex sources of irritation.

Dr. SEPTIMUS SUNDERLAND said he wrote a paper in 1898 on "The Frequency of Gonorrhœal Rheumatism in Women." Therein he had asserted his belief that, contrary to the usual teaching of text-books, the so-called "gonorrhœal rheumatism" is more common in women than in men, and had given reasons for his belief. Although it is common knowledge that arthritis deformans is much more frequent in women than in men, the ratio being (according to Dr. Llewelyn Jones's book) about five to one, he was not aware that any importance had been attached to this fact in guiding us in the attempt to discover the most common situations in the body at which the germ or germs

responsible for the malady enter the system, assuming we accept the infective theory of origin as correct. Personally he believed in the infective theory of its origin, and he considered that the frequency of the disease in women compared with men may be accounted for to some extent by the fact that, just as in gonorrhœal arthritis the vagina, the cervix, and the uterus are frequently the starting places of the disease; and that, when in women, ulceration of the vagina or erosion of the cervix, or endo-cervicitis, or endometritis exists, arthritis deformans is more likely to occur than in women whose generative apparatus is in a sound condition. He had been led to this conclusion in great measure owing to a statement made to him in 1906 by Oliver Sunderland, of Bexley (which fell in with his own ideas concerning the frequency of gonorrhœal arthritis in women), who asserted that he had relieved several cases of arthritis deformans in women by the local treatment of erosions found on the cervix uteri, and was convinced that in those cases the arthritis was dependent on the lesion of the cervix infected by some unknown microbe: since that time he had treated a large number of cases, and he claimed improvement in nearly all, and cure in a few, of those in which he had found and treated cervical and uterine lesions. He had found such lesions in the large majority of the cases treated, and affirmed that most of the women he had treated for arthritis deformans had very dark hair and dark grey eyes.

He (Dr. Sunderland) said it was easy to understand that the germ or germs which might cause the disorder may frequently flourish in the vagina and uterus without being detected, just the same as the gonococcus frequently does in the gonorrhœa of women; and it is equally certain that hundreds of thousands of women suffer from a trifling leucorrhœa which they regard as trivial, but which frequently arises from vaginitis, a slight erosion of the cervix, endo-cervicitis, &c., and may be exposing them to the risk of becoming a prey to a dreadful disease, such as arthritis deformans. He put forward the view that the preponderance of cases of arthritis deformans in women is accounted for to some extent by the fact that they possess a vagina and uterus, wherein germs may lurk unsuspected and undiscovered. He pleaded for a thorough vaginal examination of all women suffering from arthritis deformans, so that any lesion which might be found should be treated. He also emphasized his impression that it is important when consulted by patients for leucorrhœa that the practitioner should, whenever possible, satisfy himself that there is no local lesion of the vagina, cervix, or uterus. He considered the frequency of mucous colitis in women might also account to some extent for the frequency of arthritis deformans in women. Finally, he believed that some of the cases diagnosed as arthritis deformans were really gonorrhœal arthritis, or cases due to the poison of other bacilli, known or unknown.

Dr. LLEWELYN JONES, in reply, said: In the first instance I have to express my appreciation of the very patient and courteous hearing extended by you to my opening remarks on this very vexed subject—the classification of the various types of arthritis deformans. In the able and illumining discussion

that ensued, the subject was considered from so very many different aspects that, despite their essential nature, it would be impossible for me to refer to them in detail during the somewhat brief time at my disposal. This being so, I purpose limiting the range of my reply to a consideration of the various objections brought forward by those who contend that no sharp line can be drawn between the various types of arthritis deformans, more particularly the rheumatoid and osteo-arthritic groups. The gist of the opposing argument would appear to depend upon the existence of certain intermediate or, as some would call them, transitional types. The nature and significance of these so-called intermediate or transitional cases will, I think, be more conveniently considered under two headings.

Firstly, with regard to those comparatively rare examples in which the lesions of both diseases are to be found overlapping in one and the same individual, some of the articulations being the seat of rheumatoid arthritis, others of osteo-arthritis. Instances of the above have been observed by Goldthwait, McCrae, and others, including myself, but the mere co-existence of these two arthritic affections does not necessarily imply identity, and in my opinion their occasional occurrence does not justify us in regarding them as anything more than instances of the overlapping of two otherwise distinct diseases. My meaning will perhaps be less obscure if I cite an analogy, and the one that most readily occurs to me, is that afforded us by the two opposed conditions, Graves's disease and myxœdema. Now, as we are all aware, in some instances of exophthalmic goitre, a condition of solid œdema sometimes supervenes in the lower extremities, a reaching-out, as it were, on the part of Graves's disease towards its antithesis myxœdema, constituting what are known as the mixed types of Basedow. But notwithstanding the existence of these intermediate types, we do not permit their occurrence to impair our clear apprehension of the fundamental distinctness of the two diseases, Graves's disease and myxœdema. The same attitude should, I think, be adopted with regard to those cases in which a patient stricken in youth by rheumatoid arthritis develops perchance in the autumn of his life a few Heberden's nodosities in his terminal phalanges, or, maybe, an osteo-arthritis of one or other of his hip-joints. Such blending of these two joint affections cannot be construed as evidence of their identity or their transition into each other, inasmuch as their onset at different periods, as well as the widely differing character of their articular lesions, sufficiently attests their distinctness.

Passing now to consider the second category—i.e., those cases in which an unusual amount of new bony proliferations develops in joints the seat of rheumatoid arthritis, and the occurrence of which is held by some to be evidence of transition of the one disease into the other. To support such an assumption it would obviously be necessary to demonstrate that new bony formations, when they present themselves in rheumatoid arthritis, are of the same nature as those typical of osteo-arthritis. The distinguishing characters of the osseous outgrowths of osteo-arthritis are, I take it, their constancy, their massive size, and the relationship they bear to the duration

of the disease, tending as they do to increase *pari passu* with its progress. As opposed to this in rheumatoid arthritis one constantly meets with instances classically complete in all their features, in whom no traces of bony outgrowths can be detected by skiagraphy. Obviously, therefore, new bony proliferations cannot be held to be constant, much less essential, features, and when occasionally present should be regarded as accidental or secondary features—in other words, epiphenomena. On the other hand, in osteo-arthritis the bony outgrowths appear early, and are so essentially an integral part of the disease that no diagnosis of the condition could be upheld in their absence. Again, the new bony outgrowths met with in rheumatoid arthritis are not only inconstant, but they differ in character, being usually inconsiderable in size, and needle-like or spicule in shape, whereas in osteo-arthritis they are massive and rounded in contour. Moreover, unlike osteo-arthritis, the bony outgrowths exhibit no tendency to increase *pari passu* with the progress of the disease, for as Reichel and Müller have shown from anatomical and surgical investigations, new bony proliferations may be absent even in cases of twenty to thirty years' duration, while, on the other hand, in cases of much briefer standing a few minor outgrowths may be present. Such erratic manifestation is in striking contrast to what obtains in osteo-arthritis, in which the more ancient the disease the more massive the outgrowths and the more enlarged the articulation. I would therefore, gentlemen, contend that the new bony proliferations occasionally present in rheumatoid arthritis are essentially different from those of osteo-arthritis by reason of their inconstancy, their inconsiderable size, and by the lack of any relationship on their part to the duration of the disease.

In conclusion, before leaving this vexed question, I would point out that we have considered this question of the possible transition of rheumatoid arthritis into one of osteo-arthritis almost entirely from the point of the respective articular lesions of the two diseases, having apparently overlooked the fact that, apart from their individual joint characters, the two conditions possess, each of them, a distinctive clinical facies. This is to my mind an unscientific attitude to pursue, as, in order to substantiate this view, that a condition of rheumatoid arthritis may pass into one of osteo-arthritis, it would be necessary that a rheumatoid patient should put on the general clinical aspect of an osteo-arthritic—in other words, that the emaciated rheumatoid should assume the appearance of the well-nourished and but too often obese osteo-arthritic. So profound a clinical metamorphosis has not hitherto come within the limits of my own experience, and, for the foregoing reasons, I would urge that we should not permit these superficial resemblances to impair our perception of their fundamental distinctness, inasmuch as such confusion must inevitably militate against the success of their treatment, essentially different as it is in both instances.

The Climate of Egypt and North Africa and its Influence on Disease.

By H. E. Leigh Canney, M.D.

Some years ago I was asked to read before the Fellows of this Society a paper on "The Climate of Egypt in Relation to Disease," and at that time I was unable to comply. Last year, however, I informed the Society that if it was still thought by the Council that a paper on this subject would interest the Fellows of the Society, I should be ready to accept the honour thus conferred upon me by the original request, and communicate a paper on certain aspects of the influence of desert climate on disease. I was influenced in this decision by the consideration that, possibly, the experience gained during some sixteen years' practice amongst invalids sent to Upper Egypt, and especially to Assouan, the furthest southern health resort of North Africa, might be of interest to the Fellows of the Society who, owing to their own pressure of work and the difficulties of time and space, might find a study *in situ* of the influence of the climate of the desert difficult of attainment, and yet of interest owing to the increasing number of invalids seeking benefit from the deserts of Africa in winter.

After observing for some years the varied success or failure attending examples of the various pathological conditions advised to winter in Africa, I have become convinced of the importance of the realization, both by doctor and patient, of certain features of desert climates presenting some difficulty and requiring suitable modifications of custom pursued elsewhere if the best results are to be obtained or even danger to be avoided.

I will first ask you to allow me to draw your attention to the general features of the climate of the desert and the varying modifications presented at the principal health resorts of Egypt and North Africa, and then I will ask you to consider the action of these features on the physiological and pathological processes of certain diseases deriving special benefit from the climate. I shall endeavour to limit the figures in the statistics given to those necessary to a broad view of the climatic features of these desert resorts. These figures are the result of a research extending over several years conducted by myself and several colleagues at different stations in Egypt by self-recording instruments under exactly comparable conditions. Those desiring a more complete

account of this subject will find the same in a work entitled "The Meteorology of Egypt and its Influence on Disease," which was published in 1897. The chief features of the climate of the desert are warmth, extreme dryness (both as regards absolute and relative humidity), extreme purity of the air, freedom from all organic life, and marked diathermancy. The winter climate is marked by great regularity, there being very few violent changes, although the range of the curve from the heat of the day to the cool of the evening is considerable. This daily oscillation is greater at such stations as Assouan and Luxor, owing to their greater dryness, than at Cairo, Mena House, or Helouan, and adds a sense of exhilaration to the climate. Desert air, on account of its great dryness, imparts a sensation of elasticity and freshness, persisting even during the warm hours of the day, which is quite remarkable, and most noticeable at the driest resorts, and towards the centre of the desert, as in Upper Egypt. As the borders of the Sahara are reached towards the north, west and south, so the dryness, buoyancy, and diathermancy diminish, the sky becoming more cloudy and less blue.

The great dominating feature of North Africa is the Sahara Desert, the direct result of the climate, stretching from the south of Algiers away to the east until it reaches the Red Sea. It is across this desert that the Nile is alone able to reach the Mediterranean Sea, carrying its annual deposit of Abyssinian mud to form Egypt or that part of the Sahara, eight or ten miles wide except the Delta, that this river is able to water. Neither the sun nor the soil is the cause of the desert, but the climate alone. The important factor modifying desert air is the presence of water capable of being taken up into the atmosphere by rapid evaporation owing to the extreme dryness. Water being an essential to life, we naturally find the health resorts of Egypt and North Africa situated within its vicinity, and therefore more or less modified by it. It is observable that the later established resorts have taken some advantage of this factor, and placed their hotels more or less remote from the influence of cultivation or evaporation, and the ultimate success or failure of any resort will depend upon the ability with which this localization of its hotels, as regards the vicinity of gardens, water, &c., taken into relation with the prevailing wind, has been considered.

In North Africa we have one group of resorts, including Tangier, Morocco, Algiers, Hammam R'Irha, Tunis, Tripoli and Alexandria, all situated on the north fringe of the great Sahara. These resorts are mainly under the influence of the Mediterranean or Atlantic, and but

slightly under that of the desert. Though in Africa, they are found to have a marine climate, mild, bracing, and tonic in winter, with a low maximum temperature and only half the daily range of the desert resorts. If we consider Algiers, its climate closely resembles that of Malaga, Sicily, and Southern Greece, all situated in the same latitude, rather than that of the desert, the description of the "Torquay" of Africa being quite appropriate. From December to March the weather is that of an English autumn with twice the sunshine and rain. The relative humidity is the same as Alexandria, though it has a rainfall of 31 in. to Alexandria's 8 in. It is much colder than the Egyptian health resorts, but warmer, less dry and less exciting than the Riviera.

Let us now consider another group of health resorts, all but one—Biskra—being situated in the Nile Valley, and so practically possessing the desert climate, though in every case we find this modified by the presence of moisture to a greater or less degree either from the presence of the river or cultivation in the vicinity. These resorts include Biskra, Mena House, Helouan, Luxor, and Assouan. By continuous records, as I have shown elsewhere, the influence of cultivation or river is exhibited in three modifications of the purely desert air: (1) The absolute and relative humidity is increased; (2) the temperature towards sunset and the hours following is rapidly lowered; (3) a far lower minimum temperature is reached than in the adjacent desert at the same hour, 20° lower being attained, although only a mile may separate the two stations. The precision with which this influence of cultivation or river can be traced enables us to divide the climates of these resorts into two classes:—

(a) "Egyptian," or desert climate, largely influenced by cultivation.

(b) "Saharan," or desert climate, very little influenced by cultivation.

The dryness of the Egyptian resorts is the great factor of importance in treatment, and therefore it is important to see how these stations stand relatively. On the one hand we have Cairo, at the apex of the cultivated Delta, and Biskra, in the midst of a large oasis, both exhibiting the "Egyptian" type, and Assouan, the driest of the existing health resorts, presenting the most striking example of the "Saharan" type. Intermediate between these we have Mena House, but slightly drier than Cairo, and Helouan and Luxor approaching the Assouan type. Apart from the Egyptian resorts, Biskra is the only resort in North Africa presenting desert conditions. It is the southern resort of Algiers, on the northern fringe of the Sahara, yet not far enough inland to escape the influence of the Mediterranean, having a rainfall closely resembling

that of Alexandria (about 6 in. to 8 in.). Although 200 miles south of Algiers, it is still in a latitude 4° north of Alexandria, and therefore some hundreds of miles further north than Cairo. It presents a climate closely resembling Mena House, but colder in the winter months. It is colder by 3° F. than Algiers, 7° F. than Helouan, and 10° F. than Luxor, and therefore far less advantageous for the treatment of the diseases commonly sent to Egypt. It is less dry than the Egyptian resorts, the relative humidity being 60.

The two factors of importance in the Egyptian desert climate are (1) the dryness, (2) the warmth.

The dryness must be considered *(a)* from the point of view of the *absolute humidity*, or total moisture, in a given quantity of air. It is this factor which is of importance in respiratory conditions, for it is obvious that a given volume of air possessing very little moisture, when inspired and raised to the body temperature, will be able to take up from the lungs a larger quantity of moisture than one less dry. The accompanying chart (A) represents this drying power of the inspired air at the different stations. The inspired air of the High Alps in winter is able to take up 15 gr. or 16 gr. per 10 cubic ft. more moisture than that of Assouan, the driest of the desert resorts, but the advantages possessed by Egypt are:—

(1) The amount of water required to be excreted by the lungs and kidneys is far less than in the Alps in winter, owing to the highly increased excretory action of the skin.

(2) The relative humidity being very low, the chance of rain, or clouds, is remote, hence the steady conditions of the Egyptian climate offer no interruption to a continuous outdoor life, and far longer hours during which sitting out is possible. Thus a patient with chronic asthma, and somewhat embarrassed heart, requiring sedatives elsewhere two or three times during the night, was able this winter to spend four or five hours daily in the desert on a donkey for four months, with the exception of one day only.

If we now consider the desert climate from the point of view of the *relative humidity*, we observe we are dealing with such low relative humidities (Chart B) that the possibility of rain or dew, mist or cloud, becomes, at the driest stations, such as Assouan, very remote. The accompanying chart (C) shows the drying capacity of the air at the different stations. The lower the relative humidity the greater the amount of moisture, and the quicker the rate at which it is vaporized from the skin. The greater this drying power of the air, as expressed

by low relative humidities, the greater the sense of exhilaration. If the drying power is reduced, owing to the presence of less dry winds or the moisture from local irrigation, tissue change is reduced and the body has difficulty in getting rid of heat as it is produced, and a sensation of depression results with marked lassitude. Hence, the tonic effect of low relative humidity must not be overlooked.

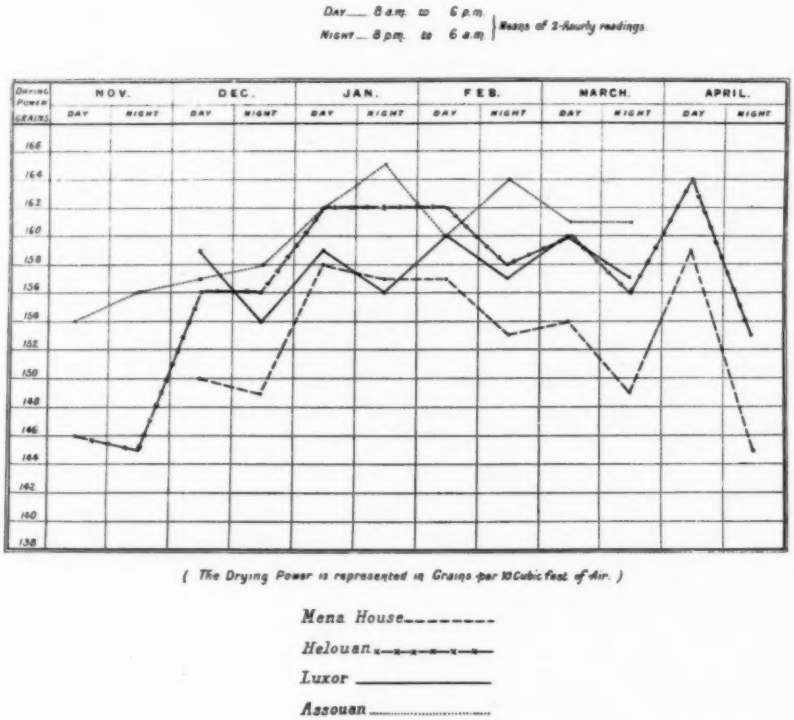


CHART A.

Drying power of the air at the temperature of the body.

The other great factor of importance in the desert climate is the warmth. The significance of this factor is seen when we consider that the vital processes are so largely concerned with the production or removal of heat. If the temperature of the air is raised, loss of heat conduction and radiation from the body is diminished, the vessels of the

skin dilate and sweat is increased, the internal organs receive less blood, tissue change is far less active, urea elimination is diminished. Mainly, however, when the increased temperature of the air passes the critical temperature (or point at which the lowest amount of tissue change takes place with effective working of the body), the body protects itself, not

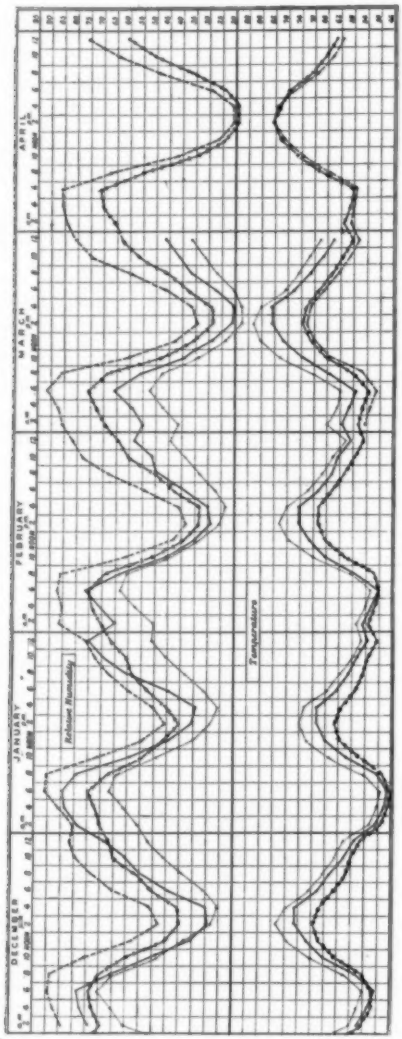
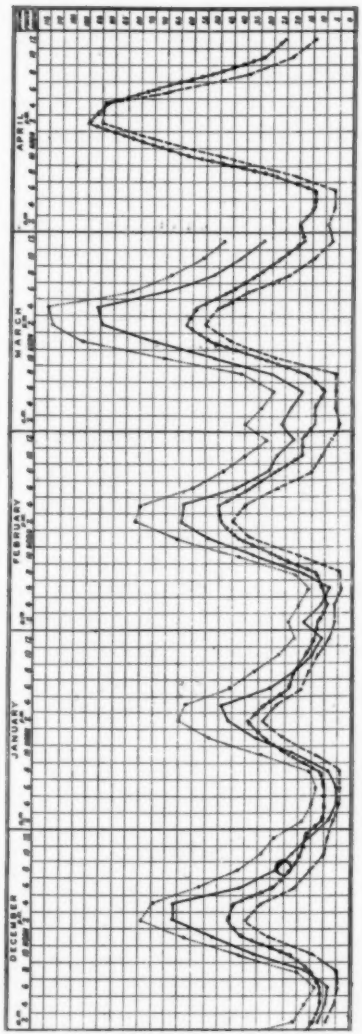


CHART B.

Temperature and relative humidity at Helouan, Mena House, Luxor, and Assouan, December, 1895, to April, 1896.

by less production of heat, but by arranging for greater loss by evaporation, radiation and conduction, and it will be seen later how these processes affect certain conditions.

It is well known that heat and cold, if alternated, have a greater influence on tissue change and, therefore, a more stimulating action on



(The drying power is represented in grains per 10 cubic feet of air.)

Mena House
Helouan
Luxor
Assouan

CHART C.

Drying power of the air at the temperature of the air.

the body, than either if continued without change. This point must not be lost sight of in considering the exhilarating effects produced by the wide range of temperature, often 20° F. to nearly 30° F., found in the regular daily curve of desert climates. One of the effects of a warm or hot climate is the increase, probably temporary only, in the vital capacity of the lungs, and Rattray attributed this to the diminished amount of blood found in the lungs, in the Tropics. On the circulation, the effect of warm, dry climate is to dilate the peripheral circulation; this diminishes the arterial tension generally, and the pulse is somewhat quickened. On the skin the effect of a hot climate is greatly to increase the amount of perspiration, but in the desert the low relative humidity allows this increased perspiration to be taken up so rapidly (in Upper Egypt ten times as quickly as in England on a dry summer's day) that unless exercise is energetic the skin remains apparently dry. It is this power of the climate, by rapid evaporation to rapidly cool the body, which requires special attention. In the urinary system the flow of water from the kidneys is greatly diminished, as well as urea and other solids to some extent, and in certain conditions, notably gouty affections, this must be compensated for by suitable intake of extra fluid.

Some of the broad effects strikingly observable in various pathological conditions may here be noted. Firstly, owing to the dryness and warmth of the air, the rate at which fluid is removed from the lungs and skin permits of a very high state of oxygenation, far beyond what is possible in colder and damper climates. A superficial observation of a number of visitors, or invalids, a week or two after their return to the more humid Cairo, will at once detect a loss of that hæmoglobin oxygen-carrying power which was so noticeable a feature in the desert climates of Upper Egypt. In the pathological states where cyanosis is a feature we at once detect great improvement in the dry and warm desert air. Very marked is the change in the asthmatic, in the man with portal obstruction and cyanotic venules, the embarrassed right side of the heart, in the cold, damp and blue hand of rheumatoid arthritis. Doubtless, every organ feels the effect of this heightened vitality; dyspepsia is no longer heard of, and in the long and indecisive struggle between toxin or micro-organism and badly-nourished cell a decisive factor is added.

The intensity of sunlight in the driest desert climates must not be overlooked as a factor, when it is remembered that animals absorb more oxygen and give out more carbonic acid under its influence.

As regards temperature, it is generally supposed the conditions are much the same at most of the health resorts of North Africa—viz., a

generally prevailing warmth. This is an error (Chart B). I have shown how much colder Biskra and Algiers are than the Egyptian resorts; and in Egypt we have the relatively cold winter months of Cairo, and the neighbouring resorts Mena House and Helouan, and the genial warmth of Luxor and Assouan in Upper Egypt. The mean minima temperatures are much the same for all stations, but the mean maxima vary widely, and it is these latter which are the best index of the length of day possible to be spent in the open with advantage. This is of great importance in cases requiring sufficient warmth to insure the action of the skin, as in Bright's disease, gouty affections, bronchitis, rheumatoid arthritis, asthma, &c., and neglect to take this point into consideration in the choice of a resort for such cases would not meet with the best success. In considering the higher temperatures, owing to the dryness of the air, at least 10° F. in Cairo and 15° F. in Upper Egypt must be added before a similar feeling of warmth is experienced to that afforded by a given temperature in Northern Europe—thus, 80° F. or 85° F. at Luxor or Assouan is pleasant and refreshing, whereas 65° F. to 70° F. would be felt cold, especially if wind were present. A maximum of 85° F. and a minimum of 52° F. gives the most desirable day in Upper Egypt, and for the most part the winter months (Chart B) are rather too cold by day and certainly by night, but during the months of November and March in Upper Egypt, and April at Helouan or Mena House, the temperatures may be considered almost ideal.

There are four principal health resorts in Egypt: two in the north near Cairo—Mena House and Helouan—and two in the south—Luxor and Assouan. They are all well arranged, and at all English physicians of experience will be found. All outdoor games are available, and much time is spent in the desert, riding donkeys, camels, or horses.

Mena House is eight miles west of Cairo, near the Great Pyramid, on the edge of the desert, and only a few feet above the cultivated land separating it from Cairo. The air is drier, purer, and fresher than Cairo, where, of course, no invalids should stay, the conditions being totally unsuitable. The season here, as at Helouan, is from November 1 to the end of April, the first and the last months being of great value, the months of December, January, and February being too cold for many cases above referred to.

Helouan is in the desert on the east side of the river, fifteen miles south of Cairo, two miles from the cultivated land, and 115 ft. above the same. Helouan is a well-appointed resort for those who wish to be near Cairo and yet have a pure desert air only moderately influenced by

the cultivation of the delta and by the river. There are important natural sulphur waters, and baths have recently been erected on an elaborate plan. The thermal waters consist of sulphur, saline, and chalybeate springs, and are of considerable value in cases of gout, lithæmia, bronchial and pharyngeal catarrh, rheumatoid arthritis, sciatica, stiff joints, engorgement of the liver and other viscera. The climate resembles Mena, but the relative humidity at night is more favourable to Helouan. It is drier somewhat than Mena, nearly as dry as Luxor, but much less dry than Assouan.

Luxor is of great antiquarian interest, and is 418 miles from Cairo by rail, in the midst of a cultivated plain three or four miles wide on either side of the river. It has long been known as a health resort and is more equable in its climate than Northern Egypt. The winter mean maximum temperature for four months is 6° F. to 7° F. higher than the resorts near Cairo, and this extra warmth is of great value to most cases. The relative humidity is 52·2 (average of two-hourly readings, December to March).

Assouan is at the foot of the First Cataract of the Nile, 551 miles by rail from Cairo, being reached in twenty-two hours. Assouan was selected as a health resort on account of its favourable position in Egypt, as regards the most important features of the Sahara climate. There is practically no cultivation on the west bank (the direction of the prevailing wind) for many miles north and south, and very little on the east bank, hence it is practically within the Nubian Desert, the driest part of the Sahara, uninfluenced by cultivation. The best time for invalids here, as at Luxor, is November 20 to the end of March, the dryness and warmth of the month of March being of great value to most cases. The average maximum temperature of the four winter months is 82·1° F., and the minimum 54·5° F. The minimum is 5° F. warmer than Helouan or Luxor by night, a factor important in cases of albuminuria, rheumatoid arthritis, bronchitis, &c. The mean temperature is 65·6° F. in December, 64·3° F. in January, 67·4° F. in February, 75·8° F. in March. The cold mornings so noticeable in the northern stations of Egypt are not felt here nor is there any chill at sunset. Assouan is the driest health resort in the world. The relative humidity is 40·9. It is 28·2 per cent. drier than Mena, 14·6 per cent. than Helouan, and 11·3 per cent. than Luxor. This dryness gives a bracing character to the climate. The prevailing wind reaches Assouan over many hundreds of miles of pure desert destitute of any form of life, even of a microbic character.

The dominating factor of the desert climate requiring the greatest attention medically is its power of rapidly cooling the body surface, mainly by evaporation. The effect produced is known as a "chill." Unfortunate is the patient whose doctor's mind is not large enough to admit any other view of disease but a microbic one, or who thinks that his patients can withstand, without precautions, a rate of cooling which I have shown is ten times more rapid than the patient or his ancestors have ever had to accommodate for, without any fear of so old-fashioned a notion as a "chill." Chill is practically the only risk the patient has to accommodate for, and it is the paramount duty of the doctor to point out clearly how this is to be done. From neglect to recognize this factor, I have seen an ordinary catarrhal enteritis or dysentery due to this cause, and curable in four days, treated as ptomaine poisoning indefinitely, to the patient's discomfiture even two years later. I have seen cases of Bright's disease, either from want of warning or total disregard of the same, recklessly exposing themselves, when hot, to wind and shade, who have paid the penalty within a few days of their mistake. I have seen cases of phthisis accepting the same risks of rapid cooling in the wind or cool shade of a dahabeah progress to total disorganization, and every case of broncho-pneumonia which I have seen in Egypt has been directly traced to this cause, and has arisen in individuals quite ignorant of the climatic requirements, and of the fact that their illness was quite unnecessary. On the other hand, year after year, I have seen patients in large numbers, equally susceptible to this factor, and some much more so, following suitable advice and precautions, not only easily arrange to meet this difficulty, but obtain excellent results from a climate otherwise of unparalleled excellence and importance. I have, however, thought it desirable to draw your attention to these cases, as I have observed a tendency to accept these evils either as something inherent in the climate or in the malady occasioning the visit to Egypt, instead of as accidents entirely preventible, and foreseen and guarded against in other similar cases. Whilst on the subject of precautions advisable in Egypt, let me remind you of the advisability, as in the case of India, of inoculation against enteric fever and small-pox, though in the case of the former protection is easily obtained by the use of boiled milk only, protected water, the refusal of salads, strawberries and ice. Very high preventive results have been obtained year after year by these means at Assouan and other health resorts of Egypt.

The best time for invalids to arrive in Egypt is early in November, staying at Mena House or Helouan in the first half of that month. The

physician must then decide whether the condition is one that will be benefited by special warmth and dryness, for, in such cases as Bright's disease, rheumatoid arthritis, asthma or bronchitis, to suggest that Algiers, Biskra, or even Cairo, and the northern resorts would offer alternatives as favourable as Upper Egypt would be to deprive the patient of much of the advantage he would otherwise have obtained.

To avoid serious mistakes, it cannot be too strongly urged that plans should be carefully considered before leaving home in consultation with some one having special experience. The great care in travelling required by all patients with pulmonary lesions, asthma, bronchitis, albuminuria, or catarrhal conditions of any organ, unless foreseen, may lead to unhappy results. These cases should travel in Egypt by railway rather than by steamer on the Nile.

The diseases influenced beneficially by desert climates are :—

(*A*) Rheumatoid arthritis, gouty and other conditions of joints. In these cases the feeble circulation, the cold, perspiring, cyanotic extremities rapidly improve. The temperature falls after some weeks, pain ceases, nutrition is good, and with outdoor exercise a new life comes to the sufferer. The hands become warm, dry, mobile to the extent permitted by previous damage. The importance of sending such patients to Egypt the first winter of the attack, before damage results in irretrievable limitation of mobility, is quite obvious and serious.

(*B*) Bright's disease and albuminuria. The great relative dryness of the air, coupled with the warmth and uniformity of the daily conditions, is of such value that no alternative climate can be found for such cases. It is important that these cases should be sent to Egypt as early as possible after onset or persistence of the symptoms.

(*C*) Pulmonary : (1) Phthisis—(*a*) non-acute cases of the first or second stage, with or without hæmorrhage ; (*b*) chronic quiescent phthisis, especially associated with bronchitis. With regard to cases with repeated tendency to pleurisy, it may be observed that not all patients are amenable to the strictness necessary for these cases to do well ; possibly such would do better in the Alps or the Grand Canary. (2) Asthma, especially associated with gout, bronchitis, or dyspepsia ; very great benefit results. Attention, in detail, to points peculiar to the climate and locality is essential. (3) Chronic bronchitis and emphysema. (4) Convalescents from pneumonia, not thoroughly cleared. (5) Chronic nasal and pharyngeal catarrh.

(*D*) Cases of mental strain and breakdown from overwork, especially if associated with gout and arteriosclerosis. Very great is the improvement visible from week to week.

(*E*) Excellent results follow also in such cases as dyspepsia, neuritis, neurasthenia, neuralgia (rheumatic, gouty, or malarial) and insomnia.

The great value of the climate of Upper Egypt in rheumatoid arthritis induces me to add to this paper Chart D (p. 128), showing the typical progress of a case of this illness during last winter. The patient, a married lady, aged under 40, had her first attack eleven years ago, and by successive advances of the disease almost every joint has been involved. Since January, 1909, the patient also had attacks of fever, reaching 100° F. in the mornings and 102° F. for two or three days a week in the afternoons, with shivering, sweating, and nausea. Contractions had set in in various positions, notably the elbows, fingers, and toes, and seemed to be starting at both knees. The pain suffered was very severe; 20 gr. to 30 gr. of aspirin were taken daily for fifteen months. The mitral valve had recently become affected, and there was œdema of the ankles. This lady I was asked to see last October, in consultation in England, with a view to deciding what good might be expected from a winter in Egypt, and I pointed out how serious her condition would be within six months if the contractions starting at the knees were not arrested. On my advice she went to Assouan, being carried into the hotel, as she could not move without help, and only very slowly. The patient had a lady companion, as she was also unable to dress, get into bed, or raise herself in bed, take out a cork, or turn on the electric light without help. On this Chart D is indicated the diminishing character of the pain to practically *nil*, and the progress, slow at first, is indicated by the first attempt to ride a donkey (letter R) on December 14, and by the fact that the lady companion was able to leave now. This attempt to ride was too premature and led to a rise of temperature and malaise. By January 22 improvement is well advanced, as shown in the fall of the evening temperatures (evening temperatures being only shown from now till March 10), diminished pain, requiring very little aspirin. Riding was resumed on January 28 on alternate days, with good results, and in February and March the patient had made such progress that exercises with an elastic stretcher were introduced with success, the rides were taken daily for two hours in the desert, and walks were also taken. The rides included galloping on a donkey, and the contrast now between the condition when seen in England and the present radiant activity, with good digestion, high colour, practically continuous normal temperature, freedom from pain, with good nights, was very striking. The treatment followed in other respects I have not thought it necessary to add, varying so greatly as it does in each case of this disease.

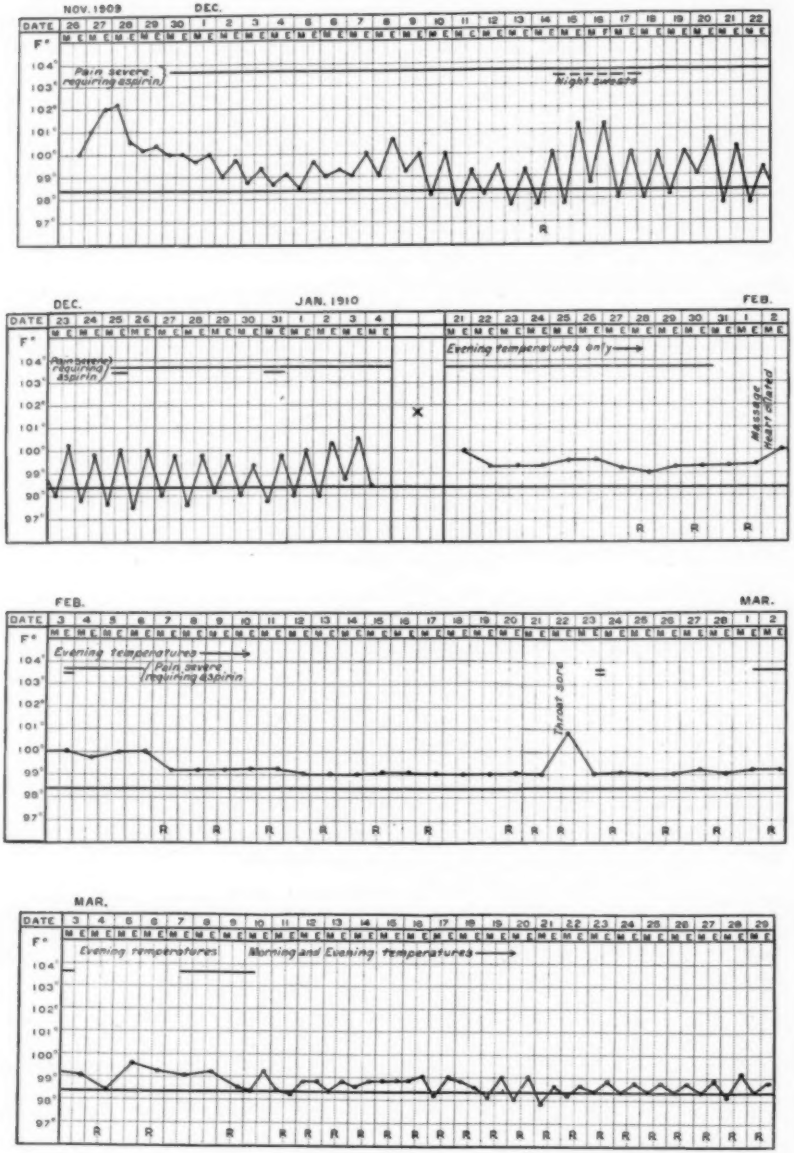


CHART D.

Case of rheumatoid arthritis. X indicates a gap in the recording of the temperature. R indicates a ride in the desert.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE THIRD

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1909-10

DERMATOLOGICAL SECTION



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1910

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Dermatological Section.

October 21, 1909.

Dr. COLCOTT FOX, President of the Section, in the Chair.

Multiple Scars simulating Leucodermia Syphiliticum.

By H. G. ADAMSON, M.D.

THE patient was a blacksmith, and presented over the side of the neck numerous, rounded, split-pea-sized, whitish patches, which on first view suggested leucodermia syphiliticum. There were, however, similar whitish patches on the forehead and over the right arm and shoulder, and on inquiry it was found that these were the result of slight burns produced by flying sparks from the anvil. There were two or three recent blisters thus caused. The white patches were less regularly placed and not so uniform in size as those of leucodermia syphiliticum.

Small Follicular Syphilide of the Corymbose Type.

By H. G. ADAMSON, M.D.

THERE had been groups of follicular papules centred by a lenticular papule. The case was shown as being a well-known though somewhat rare type of syphilide. A noteworthy feature was that the larger papules and the small follicular papules, which had now disappeared, had left pigmented scars.

A case of small follicular syphilide of the more ordinary type, with numerous patches on the trunk and limbs made up of grouped follicular papules, was also shown by Dr. Adamson.

Case of Lichen Scrofulosorum.

By E. G. Graham Little, M.D.

F. V., aged 9, was a patient who was now, and had been for three years, under the charge of the exhibitor's surgical colleague, Mr. Bowen, for enlarged glands in the neck which were presumably tuberculous. The boy's maternal grandmother and maternal aunt had died of phthisis and he himself was a delicate, undersized lad. The eruption for which he had been sent to Dr. Little for diagnosis had appeared recently, and was a beautiful example of the type described as lichen scrofulosorum. Several small collections of papules individually the size of a pin's head, and forming circinate groups the size of a shilling or a florin, were seen on the flanks, the back part of the trunk the buttock, and in one axilla. The papules were follicular in position, of a pale-pink colour, very slightly raised above the general level of the skin, and produced no subjective sensation.

The eruption was, in the exhibitor's experience, one of the least common of the tuberculides. He had seen it, however, in association with lupus vulgaris, with tuberculous dactylitis, and with tuberculous glands. It was usually an evanescent eruption, but none the less definitely tuberculous.

Acute and very Extensive Eruption of Erythema Iris Type in a Man aged about 35.

By E. G. Graham Little, M.D.

The lesions were too numerous to count and covered the shoulders, the back, the chest, the buttocks and thighs; the hands and feet and mucous membrane were entirely free. The lesions consisted of rings of variegated colour, such as had been described in erythema iris, but without actual central vesication; the rings varied in size from ¼ in. to 2 in.; the periphery was slightly raised, but there was no definite œdema or wheal-formation and no itching. Factitious urticaria, however, was well marked. No drugs or medication could be held responsible for the advent of the rash.

Urticaria Pigmentosa of very Exceptional Severity.

By E. G. Graham Little, M.D.

The mother's history was to the effect that the child had been born perfectly free from any eruption, but had begun to develop the present condition fourteen days after birth; it was now six months old, and the greater part of the body, the face, and scalp, was entirely covered with macules and small, shotty, hard papules the size of a split-pea and of a buff-brown colour. The papules as well as the macules were exceedingly numerous, and the child's skin presented a most extraordinary appearance. The whole surface was affected so that on no part of it could a sixpenny-piece be placed without obscuring a lesion—either macule or papule—except on the ankle and foot; here the skin was clear. With this exception the whole body was involved, but there were no lesions of mucous membranes.

The child had been seen first during the exhibitor's absence, and had been diagnosed as suffering from syphilis and consequently treated with anti-syphilitic remedies, which had not had any effect in the two months of trial of them. The child appeared to be very itchy, as the lesions were much irritated. The characteristic tumefaction and suffusion of the lesions, with change from brown to dusky red, occurred on friction. There was no history of syphilis, and no other children were affected in the same way.

Case of Cystic Rodent Ulcer.

By J. M. H. MacLeod, M.D.

The patient, a man, aged 77, came to Charing Cross Hospital in January, 1909, suffering from a rodent ulcer of a peculiar cystic type, situated on the left cheek immediately below the left lower eyelid and on the level of the left ala nasi. The appearance of the lesion is illustrated in the accompanying photograph (fig. 1). The tumour was about the size of half a cherry. It was semi-solid in consistence, partially translucent, and felt as if it were made up of a number of small cysts

containing a colloid substance. The skin over it was smooth and glossy, somewhat of the tint of ground glass, and presented a few dilated capillaries ramifying in it. The lesion was rounded in contour, with a slightly baggy appearance in its lower portion. At the upper and outer aspect of it there was a small solid "cartilaginous" border about ¼ in. in length, which had the typical appearance of the rolled edge of a rodent ulcer. When the growth was squeezed between the fingers small beads of sero-pus exuded from it below the cartilaginous



FIG. 1.
Cystic rodent ulcer.

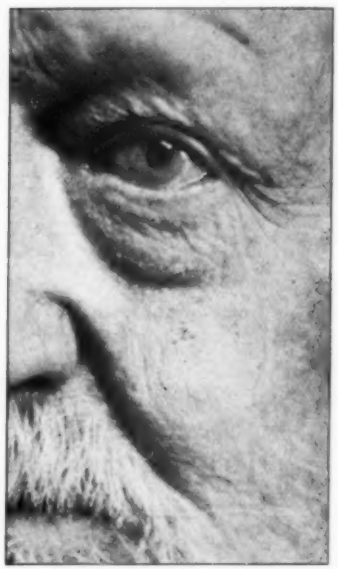


FIG. 2.
After treatment with radium.

border. There was neither itching nor pain associated with the lesion. According to the patient it began eight months before he came to the hospital. Treatment with radium was begun on March 18, when the lesion was exposed piecemeal, each part for half an hour, to 5 mg. of spread radium bromide of an estimated radio-activity of 1,800,000, so screened that the α- and soft β-rays were cut off. A characteristic reaction occurred in about twelve days. The lesion became soft, an

acute inflammatory halo appeared around it, and the skin on the surface broke down, showing the interior to be a multilocular cyst like a honeycomb. An adherent rupial scab formed, which separated towards the end of April, leaving the lesion reduced to half its original size. A second series of exposures of an hour's duration was given, and as a result a further diminution in the size of the lesion took place, and only a few small nodules were left, which were dealt with by several small exposures. By the beginning of August the lesion had disappeared and the condition was as shown in the second photograph (fig. 2).

The special interest of the case was the cystic character of the growth. The occurrence of small cysts filled with cellular debris has long been recognized as a characteristic of rodent ulcer, and been regarded as the result of a degeneration of masses of epidermic cells. A growth of the nature of the one exhibited—in which the most marked feature was the cystic formation, and in which the cysts, instead of being filled with cellular debris, contained a semi-fluid colloid substance—is unusual. The exhibitor is greatly indebted to his colleague, Mr. Charles Gibbs, for giving him the opportunity of studying and treating the case.

DISCUSSION.

Sir MALCOLM MORRIS said he had recently seen a man from India with a large growth of the lower eyelid, and on pricking a little nodule fluid came out, proving that it was cystic. It was a typical rodent ulcer, and reacted to radium perfectly. He thought a primary cystic condition was not rare.

Dr. LITTLE had had, five years ago, a case of a large cystic rodent ulcer of the back in an elderly gentleman who had had it for ten years. There were definite fluid collections in the parts of the site involved, and microscopic examination had shown typical histology of rodent ulcer as well as cysts in this section. This patient had done very well with salicylic-acid plasters worn continuously.

Dr. SEQUEIRA had seen several cases of cystic rodent ulcer. In rare instances the cystic condition was apparently primary, but it occurred more commonly in his experience in deep recurrences. In some cases it had followed apparent cure of superficial rodents by the X-rays. In two instances he remembered the cysts being in the superior maxilla and malar bones. The formation of cysts in this variety of malignant disease had been described in detail by Dubreuilh.

Thickening of the Skin of the Palms and Soles in a Woman aged 31.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.,
and S. E. DORE, M.D.

THE thickening had begun and gradually increased during the past two years. In addition to this hypertrophy there was hyper-pigmentation and great irritation of the skin of her body. When the patient became heated an itching erythematous rash appeared on the trunk. She had lost flesh and was weak and anæmic. Her mouth and tongue were normal and there was no abdominal tenderness. Her menstrual functions had been in abeyance for eighteen months. No diagnosis had been made, but some aspects of the case suggested early acanthosis nigricans. A further report will be made.

Curious Affection of the Finger- and Toe-nails.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.,
and S. E. DORE, M.D.

THE patient, an ironmonger, aged 40, had no very distinct recollection of the date or mode of onset of the disease, nor of the order in which the nails were attacked; but he thought it was three years since he noticed that the nails were not natural, and eighteen months since they had begun to give him trouble. The first thing he observed was a little black mark at the side of the nail, followed by a gradual destruction proceeding from the lateral and free edges, and accompanied by loosening and detachment of the nail from its bed. A new nail would then begin to grow and become affected in the same manner. All the nails of the fingers and toes were affected. They were raised, convex, filbert-shaped, of a greenish-brown colour, and showed slight transverse striation. Some were loose and easily raised from the nail-bed and painful when bent back, so that his work was interfered with. The nail of the right first finger had been detached, and an imperfectly formed new nail was growing. The toe-nails showed the same changes with

the exception of those of the great toes, which were hypertrophied and incurved, resembling a ram's horn (onychogryposis). The patient suffered from a bad circulation and excessive sweating of the hands and feet, conditions which were thought to have some bearing on the causation of his complaint. In other respects he was apparently healthy, and no member of his family had been similarly affected.

DISCUSSION.

Dr. DORE said he had made scrapings and examined for ringworm fungus, but the result was negative. He had also made cultures from under the nail, and they were mixed staphylococcus, chiefly *aureus* and *albus*, and some streptococci.

The PRESIDENT (Dr. Colcott Fox) pointed out that Sabouraud said nail diseases could be produced by both staphylococcus and streptococcus. He suggested that the nails be removed and the bed of the nail treated.

Dr. WHITFIELD said that in a case of hyperkeratosis subungualis the nails were removed and the whole of the nail-bed cauterized, the new and healthy nail becoming re-attached to the nail-bed.

Dr. PRINGLE said those conditions were often amenable to arsenic.

Dr. G. PERNET said he had seen a case something like that in a lady, in which all the nails were affected somewhat in the same way—toes and fingers—and he attributed it to a central nerve origin. He would, as Dr. Pringle suggested, try arsenic.

Report on Case of Extensive Acne with Scars.[1]

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.,
and S. E. DORE, M.D.

SIR MALCOLM MORRIS said that he had shown the case a few weeks ago. A vaccine had been made by Dr. Eastes, and yesterday he had had a letter from the man and from the doctor in the North saying that every trace of pustulation had entirely gone and that the condition was now practically well. He had had seven weekly injections, and had volunteered to come and show himself later on.

[1] See *Proceedings*, 1909, ii (Derm. Sect.), p. 165.

A Series of Cases of Scrofulodermia and Lupus treated by Tuberculin Injections.

By G. T. Western, M.D.

(Introduced by Dr. Sequeira.)

Of the five cases here shown, the first four illustrate the value of tuberculin inoculations in scrofulodermia. Of these four cases, it will be seen from the notes that they were all cases of infection of the skin secondary to infection of the lymphatic system. Cases I, II, and III have definite histories of tuberculous glands having been scraped or having burst spontaneously, and afterwards, instead of healing, having spread to form more or less extensive ulcers, in spite of various lines of treatment. Case IV appears to have had the primary infection on the hand and subsequently a gradual extension up the lymphatics of the arm as far as the axilla, many ulcers forming as the skin over the lymphatic channels broke down.

The treatment in these cases has been injections of minimal doses of tuberculin at intervals varying from six to ten days. By minimal doses one means the smallest dose which will produce a beneficial result. The opsonic index has been taken at the commencement of treatment, and again, should improvement cease, to indicate whether the dosage is correct. It has not been taken regularly throughout the treatment.

Two different varieties of tuberculin have been used, T.R. and B.E., both made from human strains of bacilli. Cases I and II were treated with B.E. and Cases III and IV with T.R. It appears that these two varieties are equally efficient in bringing about healing.

The local treatment has been, in Cases I, II, and III, the application of a solution containing 3 to 5 per cent. sodium chloride and 1 per cent. sodium citrate. A piece of lint is cut to fit the ulcer, and is wrung out in the warm saline solution and applied under a piece of gutta-percha tissue. This dressing should be changed frequently, as it soon becomes soaked with lymph owing to the difference in surface tension between the tissue fluids and the salt solution. Before applying this solution the skin around should be well greased to protect it from the irritation of the salt. When healing is nearing completion the saline dressing is replaced by eucalyptus ointment.

In Case IV the axillary abscess was incised. No other local or general treatment has been given during the period these cases have been under inoculation treatment.

Case V is one of lupus vulgaris. The treatment of non-ulcerated lupus by inoculation has been somewhat disappointing, except in association with the Finsen light. This is one of the more or less severely ulcerated cases which have been treated successfully by inoculation with tuberculin.

Case I.—M. C. Tuberculous glands of neck (right side) were opened and scraped in August, 1906. The wound was sewn up, but did not



FIG. 1 (Case I).

heal well. A few weeks later the wound broke down, and an ulcer formed which spread upwards and downwards. In November, 1908, she was sent to the Inoculation Department at the London Hospital by Dr. Sequeira. The ulceration was considerably more extensive than is shown in fig. 1 (from a photograph taken July 7, 1908). The opsonic index was fluctuating, probably owing to the reaction following the application of X-rays. Tuberculin injections were commenced in November, 1908, and have been continued until now. The dosage has varied from 0·001 mg. to 0·005 mg. An occasional injection of

staphylococci has also been given. Locally, a dressing of hypertonic-salt solution was used at first, and later this was changed to eucalyptus ointment. Under this treatment the whole of the ulcerated area has completely healed (*see* fig. 2, from a photograph, taken October 26, 1909).

Case II.—A. A., aged 34. In July, 1908, the patient had broken-down tuberculous glands of the neck. They were opened, scraped, and the wound sutured. The lower end of the scar broke down three weeks later. Two chronic ulcers formed in the middle line of the neck, which resisted all treatment for nine months. In May, 1909, she was transferred to the Inoculation Department by Mr. Mansell Moullin. Tuber-



FIG. 2 (Case I).

culin was given in doses commencing with 0·00005 mg. and increasing to 0·005 mg. In August the ulcers had completely healed. A local dressing of hypertonic-salt solution was used at first and later eucalyptus ointment.

Case III.—J. G., aged 6. According to the mother's account, a lump appeared on the right side of the face. This she poulticed until it broke down. Since then the child has been attending various hospitals. The condition has steadily become worse, the ulceration spreading in all directions until June, 1909, when she came to the London Hospital.

A large ulcer about 3½ in. in length was then present on the lower jaw on the right side. The edges were heaped up and keloidal, the floor was covered with unhealthy granulations, the base was infiltrated. Injections of tuberculin have been given for three months, during which time there has been considerable improvement. The case is still under treatment.

Case IV.—Mrs. G., aged 45, sent to the Inoculation Department at the London Hospital by Mr. F. S. Eve in November, 1906. The patient's history was that a small red patch appeared on the thumb in 1899. This became ulcerated. Similar patches appeared on the forearm and arm, each breaking and forming an ulcer. In November, 1906, the whole of the left arm and forearm was swollen and œdematous and



FIG. 3 (Case V).

covered with chronic granulomata surrounded by scar-tissue. Many of the ulcers were secondarily infected. The axillary glands were enlarged and breaking down. The condition was so severe that amputation (fore-quarter) was considered. However, it was decided to try inoculation first. The tuberculo-opsonic index was 0·5. On November 27, 1906, 0·0001 mg. tuberculin was given, and the axillary abscess was drained. On December 13 the arm was free from pain for the first time for three years, and the ulcers had commenced to heal. In January, 1907, the arm completely healed. Eight injections of tuberculin were given, the largest dose being 0·0002 mg. There has been no recurrence.

Case V.—H. H., aged 24. A patch of lupus vulgaris appeared in 1897 on the right cheek. It spread to the neck and round the mouth

and nose. Patches of ulceration also appeared on both arms. In September, 1905, he was transferred to the Inoculation Department by Dr. J. H. Sequeira. Both cheeks presented severe ulceration, which extended downwards on to the lower jaw and on to the nose (fig. 3). The dorsal aspects of the arms also presented large ulcerated areas. The tuberculo-opsonic index was 0·8. Injections of tuberculin have been given, commencing with 0·0002 mg. and increasing to 0·02 mg. In September, 1906, the arms had entirely healed and the face had made good progress. Since then progress has been steady though slow until the present time, when healing is practically complete.

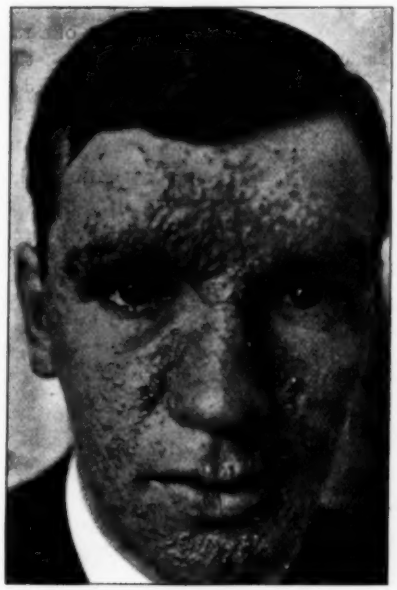
Case for Diagnosis.

By W. Kenneth Wills, M.B.

J. E., aged 20, of dark complexion with many pigmented nævi, presented a symmetrically arranged mass of pigmented, acuminate, sebaceous papules on the nose and contiguous cheeks, forehead, malar regions and chin. The lesions were dark brown, acuminate papules, of the size of a double-B shot, situated round a sebaceous follicle, so that the dilated mouth of the follicle occupied the summit of the papule. The lesions were discrete for the most part, especially in the recently developed areas; but in the older part they are so closely aggregated as to raise the level of the intervening depressions. All the lesions are intensely sebaceous and are remarkable for their deep-brown pigmentation, which extends in some instances outside the base of the papules themselves. The groups on the cheeks are pustular and confluent owing to a superadded infection. From the follicular openings a thinnish oily fluid has been seen to exude, but they are not plugged with fatty comedones. The skin intervening between the papules and beyond the areas involved is not particularly sebaceous, nor are the follicles patent more than is usual. On the back and chest are a few scattered acne papules of an ordinary nature, without pigmentation of any kind. Associated with this rash was a severe necrosis of gums and onychia on several of the nails on the hands and feet.

The patient came under observation two and a half years ago, when the rash only occupied the tip and alæ of the nose and appeared like an acne rosacea on a darkly pigmented skin. As ordinary treatment for

acne rosacea had no effect upon the lesions, and taking into account the pigmentation, antiseptic treatment was tried, without effect other than increasing the gingival necrosis. Biopsy added little or nothing to the diagnosis, though the picture was compatible with tubercle. So von Pirquet's test was tried and injections of T.R., without reaction being obtained. Calmette's test was not applied, as he had frequently recurring attacks of conjunctivitis. His own vaccine was used for the



Case for diagnosis.

pustulation of some of the lesions which occurred occasionally, but this had no effect upon them. Of the various treatments adopted, X-rays was the only method which could claim any good result. This succeeded in flattening the papules without removing the pigmentation or having any effect upon the spread.

The suggestion is offered that the case is a follicular tuberculide; and compatible with this is the fact that he lives in the same house as a brother who is suffering from chronic fibroid phthisis.

DISCUSSION.

The PRESIDENT (Dr. Colcott Fox) said it was suggestive to him of a condition of which several members had published examples—namely, of a form of seborrhoid—and those cases yielded with the greatest readiness to sulphur preparations.

Dr. J. M. H. MACLEOD said that the case suggested a tuberculide, and somewhat recalled cases which had been shown at the Dermatological Society of London under the heading of "Acne agminata."

Dr. LITTLE well remembered Dr. Pringle's case of a rare seborrhoid, and at first sight thought the case resembled Dr. Pringle's; there was, however, in Dr. Pringle's case more papulation, and, as far as he could remember, no definite scarring. The amount of the latter made tuberculosis probably an underlying factor.

Dr. WILLS replied that he had employed both sulphur and salicylic acid locally.

Dermatological Section.

November 18, 1909.

Dr. Colcott Fox, President of the Section, in the Chair.

Dermatitis Herpetiformis in a Man aged 58.

By G. W. Dawson, F.R.C.S.I.

The eruption began last July, and when seen in October there were numerous cherry-sized bullæ at the base of neck, upper part of chest, both arms, and one on each outer canthus of the eye. Some were single, others grouped; they came up very rapidly on an erythematous base and ran their course in a week. The itching was intense, but had of late improved. He got considerably better on Fowler's solution and inunctions of sulphur ointment, but had minor relapses, and a few days ago became as bad as ever. The contents of a small recent bulla were examined bacteriologically by Dr. Hare and smears made on agar-agar, when a pure culture of the bacillus shown under No. 1 microscope was found, and a subculture on blood-serum made from it which is shown under microscope No. 2. The first was recognized as a degenerated form of diphtheria bacillus, the second was typical of that micro-organism.

Two guinea-pigs, weighing 350 gr. each, were inoculated, one with 0·5 c.c. broth culture of the bacillus and the other with 1 c.c. Both died within twenty-eight hours. Two other guinea-pigs, but of much less size, were then each inoculated with 1 c.c. of the emulsion and 1 c.c. of antitoxin (diphtheria); they are both alive now. A section of a bulla was also made which shows the entire epidermis lifted off the underlying corium.

Case of Pruritus.

By G. W. Dawson, F.R.C.S.I.

The patient, G. S., aged 65, a garden labourer, came under observation last June complaining of severe itching, which he said began the previous August. There was nothing to be seen beyond a few scratch-marks on the shoulders, and therefore it was diagnosed as a case of pruritus or urticaria, although there were no wheals to be seen. During last August the back and flanks assumed a slate-coloured discoloration, and there are now some patches on the chest and about the clavicles. Gradually the discoloration assumed its present hue, which might easily be mistaken for tinea versicolor, so much does it resemble that disease in colour and distribution. He also showed enlargement of the glands in the axillæ, groins, and in the sterno-mastoid region. The urine was examined: Specific gravity, 1,010; no sugar or albumin. A biopsy showed pigment in the basal layer of the epithelium and in the corium.

The exhibitor remarked that similar cases of pigmentation have been described as following urticaria.

Dr. T. D. Savill said he thought the pigmentation was started by phthiriasis. He had seen the condition occasionally in old people, and this man was aged 68. Sometimes it extended to other parts of the body.

Hypertrophic Lichen Planus of Two Months' Duration in a Boy aged 10.

By S. E. Dore, M.D.

The points of interest in the case were: (1) the comparative rarity of the disease in childhood; (2) the well-marked implication of the tongue and buccal mucous membranes; (3) the annular formation of the lesions on the limbs; (4) the association of follicular keratosis with the lesions of lichen planus.

Case for Diagnosis.

By E. G. Graham Little, M.D.

The patient was a man aged 50. He had had the eruption for about three weeks, and was first seen on November 11, when he had a number of lesions forming complete circles and imperfect segments of circles, the circles being of a diameter of about 2 in. in the smaller instances; in the largest—that on the right side between the iliac crest and the lowest rib fold—there was an imperfect segment with a diameter of about 8 in. Other patches varied in area between these limits. The circles were formed by a periphery of continuous scales in some instances, the limiting edge being about ½ in. wide; in the largest plaque the periphery was composed of a broken series of small circinate lesions constituting the segment of a wide circle. The area of skin enclosed in the ring was quite normal; the edge of the figured patches was slightly scaly and in some instances excoriated, possibly by the finger-nails, although the patient did not appear to suffer from sensations of itching. There were in all about twenty such circles, or segments of circles, distributed on the limbs, the arms and legs fairly equally, but not symmetrically on both sides. On the scrotum and the pubic area there were some small, scaly papules not arranged in ring form. The man gave a history of having contracted syphilis thirty years previously, but he had not had any recent symptoms, and a Wassermann-Fleming test proved negative. He gave and reiterated a positive statement that the lesions had all developed within the space of about nine days, and that he had been quite free from eruption previously. In the course of the time during which he had been under observation the lesions had certainly paled and become less scaly; no treatment had been adopted except a mild stomachic—"mistura alkalina" of the hospital pharmacopœia. Scales from the lesions had been examined for fungus, but with negative result. The patient had not taken any drug previously to the appearance of the eruption, and several lesions were situated out of the reach of his own hands, so that an artificial cause could not be accepted; moreover, he had not ceased his occupation or in any way sought to create sympathy for his condition.

The PRESIDENT suggested the possibility of psoriasis. Other suggestions were a circinate erythema; tertiary syphilis, although there was no ulceration and the lesions were multiple and bilateral, and the history of rapid onset did not confirm this opinion.

Lupus Erythematosus with Symptoms indicating the possibility of the Transformation of the Circumscribed Type into that of the Acute Disseminated Form.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a woman, aged about 40. The disease had commenced on the face last April and had rapidly spread, so that there were several patches on each cheek, the size of a shilling, of the circumscribed discoid type. The scalp was quite free. More recently she began to have small papular lesions on the hands and feet and on the forearm up to the level of the elbow. These papules in some instances looked like lesions of lupus erythematosus, but in others there was umbilication, and in shape and size they strongly resembled the early lesions of acne scrofulosorum or "folliclis." These lesions were about $\frac{3}{16}$ in. in diameter, a bright bluish-red, non-itchy, and fairly numerous—about thirty all told; the skin around them was rather congested, the circulation being poor in both extremities. No other lesions of tuberculosis were identified.

The PRESIDENT suggested the possibility of the more recent nodules being really "folliclis," and invited the exhibitor to make a biopsy. There was some discussion as to the treatment to be adopted, and general agreement that this should not be local or of such a nature as to risk increase of inflammation in the parts affected.

An Unusual Condition of the Lips.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed., and S. E. DORE, M.D.

THE patient was a lady aged 29, whose home was at Seattle, U.S.A., whence she had come for treatment. The trouble began when she was aged 18, and appeared to have resulted from a habit of biting the lips and tearing off the skin. She discontinued the habit, but the condition persisted, and she had been under treatment for it practically ever since.

The exhibitor could not settle in his mind what the condition was; he had only seen one or two other cases comparable to it. Seven years ago she had had treatment by means of various ointments, and four years ago she had three months' treatment with the Finsen light, but that produced a strong reaction. Dr. Nevins Hyde, of Chicago, produced temporary benefit in the condition. Two years ago the patient consulted Professor Lesser, in Berlin, who said it was a chronic inflammation of the lips, and prescribed an ointment, but after four or five applications there was no benefit. The patient then went to Professor Unna, of Hamburg. Sir Malcolm saw her for the first time on September 22, eight weeks ago, when the whole of the lips were covered by a thick accumulation of scales, which caused the lips to protrude. When the scales were removed the lips were blue instead of red in colour. There was a certain amount of burning pain, but she complained most of a dead feeling in them, and the general irritation made her feel very desperate and hopeless. He did not regard it as cheilitis, but as belonging to a different group. In the latter Dr. Crocker said in his book that there was often an association with seborrhœa of the scalp. But there was no seborrhœa in this patient, nor was there any indigestion. She had, however, sluggish circulation in the extremities and was somewhat neurotic. To call the condition eczema of the lips would be a misnomer, and it bore no relationship to Fordyce's disease. He had had a case something like it, which had lasted a long time, in a man from Johannesburg. He had tried radium in the present case, with only nickel screening and india-rubber, and for not more than five minutes at a time at first, which was last time increased to seven minutes. It had improved enormously as a result, but his anxiety was lest the benefit might be merely temporary. One of the characteristics of the condition was the presence of a mucoid secretion, but that was present in many people who did not breathe well through their nose. The process seemed to be an enormous exfoliation, and then the forming of epidermis over it and huge thick scales. Was it, as had been suggested, seborrhœa of the lips? He did not regard it as an inflammatory condition in the ordinary sense of the term. The man to whom he had already referred, he treated with radium, and he apparently got well.

DISCUSSION.

The PRESIDENT said the improvement in the condition produced by the radium seemed encouraging. Such cases often seemed obstinate, because it was such a difficult site to treat.

Dr. PRINGLE said he confirmed the statement about the subjective symptoms which such patients had. He thought the condition was parallel with that of the finger-tips seen in some persons with "chilblain circulations" where there was paræsthesia and a fine scaly condition of the tips of the fingers.

Case of Tertiary Syphilis (Gummatous Teno-synovitis).

By J. H. SEQUEIRA, M.D.

THE special interest in this case lay in the severe and recurrent affection of the tendon-sheaths and large synovial sac of the right hand. The patient, a healthy-looking man, aged 47, unmarried, contracted syphilis fifteen years ago. He was treated with mercury for rather under a year. The secondary eruption was apparently slight, and when it had cleared up the patient did not continue under treatment. Six years ago there was an outbreak of gummatous ulcers on the inner aspect of the left thigh and on the back of the right hand, and on the extensor and flexor surfaces of the right forearm. The ulceration healed up entirely in three months under specific treatment. The mucous membranes have not been affected since the early secondary stage.

The patient remained quite well until November, 1908, when Dr. Sequeira first saw him in consultation with Dr. Macgregor. At that time there was an enormous fungating swelling involving the whole of the little finger except the ungual region. This tumour extended down the back of the hand. On the outer side it reached the base of the middle finger. At first the swelling was very tense and of a dark purple colour, but it broke down rapidly into dirty ulcers covered with a yellowish-brown slough. The photograph shows the condition very well. It will be remarked that it closely resembled the appearances seen in severe tuberculous dactylitis, but the whole process was much more acute, the duration of the tumour formation and ulceration being a little over three weeks. Under mercurial inunction and the internal administration of potassium iodide and weekly pastille-doses of the X-rays the swelling disappeared, and the ulceration healed in four weeks. The treatment by mercury was, however, not continued sufficiently, and there have been three relapses, but all have cleared up under the same treatment. The relapses consisted of smaller purplish swellings on the right hand and one on the left side, and on the dorsum of each foot.

Three weeks ago the right hand began to swell. The swelling appeared in the palm and above the wrist. It occupied the region of the larger synovial sac, extending from the roots of the fingers to the lower border of the anterior annular ligament, and for 2 in. to 2½ in. above that band. The swelling was brawny and painful and of a dull red colour. Large bullæ appeared later on the surface. The dorsum of the hand was enormously swollen, œdematous, and pitted deeply on pressure. There has been considerable pain. The patient was in this condition when shown at the meeting. There is no doubt that the present



Gummatous Teno-synovitis.

affection lies in the large synovial sac of the hand and wrist. The fact that there is a depression in the site of the annular ligament is proof of this. The great œdema on the back of the hand is similar to that seen in deep suppuration of the palm.

The lesions of the fingers in November, 1908, and at later dates have also been in the tendon-sheaths. The tendons of the affected fingers have become fixed as in whitlow, and the movements are impaired. The X-rays show that the bones themselves are quite unaffected. The Wassermann reaction was positive.

Symmetrical Plantar Nævoid Tumours in an Infant.

By J. H. Sequeira, M.D.

The patient, a healthy infant, aged 6 weeks, was brought to the Out-Patient Department at the London Hospital a week ago. At birth the nurse noticed that there were swellings on the sole of each foot, and the mother discovered them on the third day. On the inner side of each sole on a level with the tubercule of the scaphoid bone there are subcutaneous, mobile, oval tumours of the size of a small almond. The skin over them is of normal colour. Pressure does not diminish their size, and they feel like lipomata or fibrolipomata. No member of the family has been known to suffer from a similar affection.

My colleague, Mr. R. Warren, informed me that he had seen a similar case, and on cutting into one of the tumours found that it consisted of fat. The tumours will be removed, as they would prevent the child from walking properly. The character of the lesion will be reported on at a later date.

Dr. Pringle said that many models of the condition were labelled syphilitic lymphangitis at the St. Louis Hospital. He had under his care now a case of many years' duration, in which crops of vesicles appeared from time to time.

Dermatological Section.

December 16, 1909.

Dr. COLCOTT FOX, President of the Section, in the Chair.

Case of Probable Pemphigus Vegetans.

By P. S. ABRAHAM, M.D.

A. E., AGED 42, female, married, was sent to the West London Hospital on October 13, 1909, by Dr. Timmins, of Acton. She was admitted as an in-patient, as her condition appeared grave. There were numerous bullæ, large and small, situated on various parts of the trunk, especially on the abdomen, upper arms, shoulders, thighs, and legs, but none on the forearms, hands, or feet. Many of these bullæ were hæmorrhagic, and some showed small fungating granulations where the bullæ had ruptured. The mouth was particularly sore, the lips swollen and covered with hæmorrhagic crusts and with several flat bullous lesions on the mucous membrane within the buccal orifice, on the cheeks and tongue, and the fauces congested. She complained particularly of the soreness of the mouth and throat. No lesions were found about the vaginal and anal orifices, perinæum or groins, or axillæ. The pulse was 100, respirations 22, and temperature 99·2° F.

Family history: Her father had died of "cancer" and a brother of "general paralysis"; her mother and other brothers and sisters alive and well.

Previous history: She had suffered from "anæmia" when young; otherwise she had been always well and strong until the early part of this year. She had been married twelve years and had had four children; one had died at five months, the others were healthy, the youngest three months old. The patient lost all her teeth a year ago

Note.—In the report of the last meeting of the Dermatological Section in November, published on p. 22, Dr. Pringle's remarks refer to Dr. Sequeira's case of tertiary syphilis.

and had not masticated well with her false ones; she had long suffered from severe constipation. No history of venereal disease in herself or her husband.

Dr. Timmins had kindly given Dr. Abraham the following information: She consulted him first early in September for severe ulcerative stomatitis, which no treatment seemed to relieve, and after three weeks she went to St. Mary's Hospital, where various mouth-washes and medicines were tried without benefit. Dr. Timmins was summoned to see her again in the second week of October, when she was in bed, her body "covered with large blebs," which had developed within a few days. Her mouth was in a worse condition than in September.



FIG. 1.
? Pemphigus vegetans.

On admission at the hospital she stated that the vesicles on the body and limbs had appeared three weeks previously—at first on the thighs and in crops ever since. There was considerable itching as they came out. The patient was at once put upon liquor arsenicalis, commencing with ℥iij t. d., p. c., and gradually increasing to ℥ix t. d., with iron and strychnine, and the skin-lesions seemed to improve, first under boracic fomentations, and then with orthoform dusted on and a compound creolin, lead, zinc and mercury ointment, but the mouth still remained sore, the conjunctivæ congested, and excoriations and bleeding

from the nasal mucous membrane. The dose of arsenic was then lessened. The voice became so much altered that Dr. Davis, of the Throat Department, was asked to examine the larynx. He found the condition so curious that he asked Dr. Abraham to let him show the case at the Laryngological Section of this Society. His note was: "The laryngeal appearance is unique. The cords are a brilliant red, almost scarlet; they are thin and meet readily. There is no swelling of the arytænoids or ventricular bands. Epiglottis excoriated. The palate, tongue, tonsils, lips, and the anterior edges of the septum on both sides are affected and ulcerated. The patches resemble secondary syphilis, but are redder, more angry, and broken." He informed Dr. Abraham that the diagnosis of pemphigus vegetans was accepted at the Section. The patient had been gradually improving.

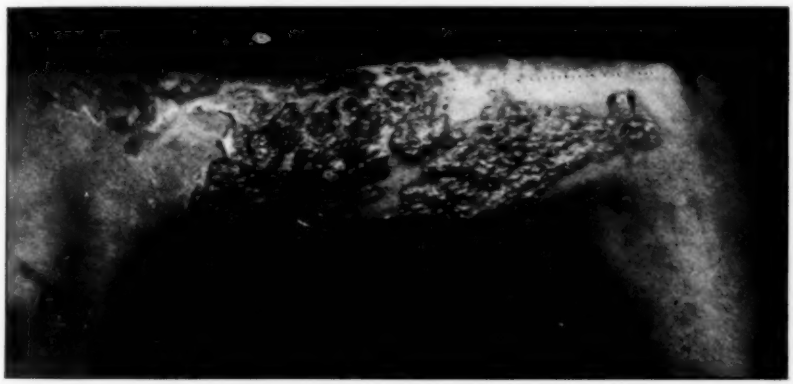


FIG. 2.
? Pemphigus vegetans.

Several pathological examinations had been made while she was in the ward. No eosinophile bodies were found in excessive amount in the secretion scraped from the raw surface beneath the blebs, and cultivations attempted on two occasions proved negative for micro-organisms.

Several medical men who saw the case suspected it to be syphilitic, from the implication of the mucous membrane of the mouth and from the constant and dark colour of the older lesions. Dr. Abraham did not take that view, but he had nevertheless asked Dr. Bernstein to try the Wassermann reaction. This had proved negative.

In the exhibitor's opinion it was a case of pemphigus vegetans, differing in certain respects from two others that he had had under care within the last few years at the West London Hospital, especially in the great amelioration which had taken place under treatment and in the absence of lesions about the perinæum, groins, and axillæ. Both the other patients had died.

Case of Dermatitis Herpetiformis.

By P. S. Abraham, M.D.

L. W., a little girl, aged 10, began to develop "sores" on the legs and arms in June last. In September she came to the Out-patient Department of the West London Hospital with several bullæ upon raised red bases, some of them hæmorrhagic and ulcerating, and with pigmented scars of old lesions. The first diagnosis was pemphigus, then epidermolysis bullosa was suggested, and finally Dr. Abraham came to the conclusion that this case probably came under the category of Duhring's dermatitis herpetiformis. The lesions have not so far appeared on the trunk; they are accompanied with considerable pruritus and pain. In every other respect the child is, and has always been, quite well. She was admitted as an in-patient on October 8, the sore place treated with boracic fomentations and then with a mild antiseptic ointment, and iron and arsenic internally, and was discharged with everything healed on November 5.

She returned about ten days ago with the affection as bad as ever, with new groups of bullæ, and some of the former lesions again active.

Case of Dermatitis Herpetiformis.

By G. W. Dawson, F.R.C.S.I.

This was the case exhibited at the previous meeting,[1] in the bullæ of which the Klebs-Loeffler bacillus had been discovered. Diphtheria antitoxin had since been injected (first injection November 17), and by December 4 the eruption had completely disappeared, although the inunctions of sulphur ointment and the administration of Fowler's solution were stopped. On December 1 the antitoxin was reduced to 2,000 units every second day, and on December 10 there had been a fresh outbreak of bullæ, but not nearly so extensive as previously.

[1] *See* p. 15.

Case for Diagnosis.

By G. W. DAWSON, F.R.C.S.I.

THE patient was a healthy young man, aged 25, who noticed eight years ago some discoloration of the legs below the knee. There was now to be noticed a more or less symmetrical yellowish discoloration of the entire lower extremities from the knees downwards, and a roundish patch on the inner side of each thigh, in addition to which there were a few small indefinite patches on his back. The margin of the coloration was sharply defined. There were no symptoms, and in other respects the skin was quite normal.

DISCUSSION.

Dr. PRINGLE had very little doubt that the case was identical with those described by Dr. Radcliffe Crocker and Dr. Pernet as xantho-erythrodermia perstans. In the two similar cases which had come under his personal observation the results of treatment of most varied sorts had been disappointing. He was quite unable to give any explanation of the essential nature of the disease, but referred for further information to Dr. Pernet.

Dr. PERNET said this case appeared to come into the category of the condition he had named xantho-erythrodermia perstans, instances of which had been brought before the Dermatological Society of London by the late Dr. Radcliffe Crocker and himself.[1] Dr. Pernet was indebted to Dr. Pringle for calling his attention to the present case, which he should like to examine in ordinary daylight.

Bazin's Disease in a Man aged 36.

By G. W. DAWSON, F.R.C.S.I.

THE disease began on the lower part of the right calf in the year 1900 whilst acting as a mounted trooper in South Africa. There were now numerous circular scars, some of them the size of a sixpence, on the lower part of the leg, back and front, accompanied by considerable pigmentation. The left leg was less affected, there being a few scars over the tendo Achillis and a patch on the inner side of the tibia. The lesions began as indefinite pea-sized tumours under the skin, some of which ulcerated. The pain was trifling.

[1] For details see *Brit. Journ. Derm.*, 1905, xvii, p. 119.

Case of Dermatitis Artefacta.

By T. Colcott Fox, M.B.

The patient was a young girl, aged nearly 16, with two excoriated patches on each cheek about the length of a terminal digit and about the breadth of the digit. When first seen the superficial excoriation was apparently covered by a thin blood-crust, but to-day he noticed the covering was darker and suggested the application of some coloured substance. The patient said the lesions would not heal up. He had not the opportunity of interviewing the mother, but he understood the girl was highly excitable, that her menstrual functions were irregular with periods of amenorrhœa, and that at one time she had constant



Neurotic excoriations.

vomitings, once with blood. She had already applied at several hospitals, and the condition had continued off and on for about three years. The case was an example of a not very rare artefact condition arising in so-called hysterical girls, and the distribution and arrangement and the contour of the excoriations were very characteristic of one type of artificially produced "neurotic excoriations." The exhibitor had now seen a number of these cases.

The term "neurotic excoriations" probably included two types: the one produced in "hysterical" young people without obvious reason, of which the present case was an example, and the other in which great irritation was sought to be relieved by determined tearing with the nail. Such excoriations were smaller and had not the larger size and special distribution and significant arrangement of the first category. The primitive lesions were often difficult to recognize, and were attributed

sometimes to urticaria. Dr. Adamson had reminded the exhibitor of a paper by Dr. Brocq on acne vulgaris systematically excoriated.[1]

DISCUSSION.

Dr. SEQUEIRA asked the President's permission to examine the sensibility of the soft palate in this patient. He found it to be completely anæsthetic. The patient felt nothing, and there was no reflex on rubbing the surface of the soft palate with a pen-holder. Anæsthesia of the soft palate had been demonstrable in every case of factitious dermatitis *in the hysterical subject* Dr. Sequeira had examined. Associated with this he had occasionally found stocking and glove anæsthesia and also hemi-anæsthesia. Alteration in the visual field as tested by the perimeter had also been met. These concomitant signs of hysteria are a valuable aid in diagnosis, and the condition of the palate is easily examined.

Dr. PERNET suggested the possibility in some instances of an alternation of personality in this class of case.[2]

Case for Diagnosis.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a middle-aged man with a chronic granulomatous-looking affection involving the greater part of the skin of the right buttock. The history given was as follows: The patient denied any history of syphilis; he was apparently quite sincere and positive in this denial. The disease of the buttock had never got well, but had varied in severity during the past ten years. Dr. Turnham, of London, who had sent the case up for an opinion, had operated three years ago, removing a portion of the indurated mass. At first sight the resemblance of the disease to actinomycosis was strong, but no evidence of that disease was forthcoming on microscopical examination. A Fleming-Wassermann's reaction was reported to be positive, and on this authority the diagnosis of syphilis was offered, and the man had to-day been put upon anti-syphilitic treatment for the first time.

The resemblance to actinomycosis was remarked upon, but the diagnosis of syphilis was accepted in spite of the absence of history and the curious persistence of the lesions.

[1] "L'Acné excoriée des jeunes filles et son traitement," *Revue générale de clinique et de thérapeutique, Journal des practitiens*, 1894.

[2] Pernet, "The Psychological Aspects of Dermatitis Factitia," *Trans. Amer. Derm. Assoc.*, Meeting Philadelphia, 1909.

Pigmented Moles in a Female Child aged 2 years.

By E. G. Graham Little, M.D.

An unusual number of pigmented moles were scattered over the body, with the following general distribution and appearance: The lesions varied in size from that of a threepenny-piece and smaller up to growths an inch and a half in diameter, and were distributed irregularly over the trunk, shoulders, buttocks, and limbs, and were mainly deeply



Pigmented moles.

pigmented moles without growth of hair. On the left buttock were two large hairy moles close together. There was a large lesion not denuded of hair on the vertex of the scalp.

The history given was that several of the larger moles had been present at birth, but that several had come out since then, and were still coming. The way in which they developed, as described by the mother, was striking. The first sign was usually a small, deeply red lesion, not unlike the lesion of urticaria pigmentosa; several of these were demonstrable on the skin at the present time, and similar lesions had become converted, according to the mother, into the deeply

pigmented corrugated excrescences which were also present. The resemblance to urticaria pigmentosa offered by these initial patches was remarkable. No other member of the family was affected with moles; the mother herself was free, and she gave no history of fancies during pregnancy, which are sometimes supposed to cause birth-marks in offspring. There was no spina bifida.

The rapid advance in number and size of the moles constituted the interest of the case. Brocq cites a case of Audry's under the heading "Lentiginose profuse"[1] and remarks on the rarity of the condition. The prognosis seemed not entirely free from anxiety in view of the malignant tendencies of pigmentary tumours.

It was suggested, and the suggestion was accepted, that a biopsy should be performed and the result reported later.

Dr. ADAMSON stated that he had seen a closely similar case of multiple moles in a child. On subsequently referring to the notes of the case he finds that it was the same patient. It is interesting to note that photographs taken at that time (October, 1908) indicate that the lesions have but little, if at all, increased in size or number.

Case of Kératodermie palmaire et plantaire symétrique et congénitale of Besnier.

By E. G. GRAHAM LITTLE, M.D.

THE patient was a male infant, aged 16 months, and the condition for which he was shown was present at birth. No other members of the family were similarly affected, and no hereditary history could be obtained. Both soles and to less extent the palms were affected, the skin in those parts being thickened, deeply pigmented, and broken up by the lines of cleavage of the skin into a mosaic as described by Besnier.[2] There was no erythematous border, however, around the thickened and pigmented area, which in the foot was notably accentuated in the site of the tread, the instep being comparatively free, the balls of the toes affected. There were also small circumscribed patches of symmetrical pigmented keratodermia on the outer sides of the knees and the elbows, but nowhere else. There was no trace whatever of ichthyosis, the skin being elsewhere perfectly smooth and fine. The hair and nails were quite normal.

[1] *Traité élémentaire de dermatologie pratique*, Par., 1907, ii, p. 578.

[2] Kaposi's "Diseases of the Skin," 2nd edit., ii, p. 40.

A Section of Melanosis Cutis.

By G. Pernet, M.D.

The sections were from a slowly-growing melanosis cutis. The pigment-cells were well shown, and an early epithelial degeneration could be seen.

Case of a Mixed Tumour (Tricho-epithelioma Papulosum and Syringo-cystadenoma).

By J. E. R. McDonagh.

E. D., aged 48, housewife, came up to St. Bartholomew's Hospital, under the care of Dr. Adamson, to whom I am indebted for being allowed to show the case, with a patch of psoriasis on the right leg. She was noticed also to have some pale, slightly raised nodules, about the size of a lentil, on her lower eyelids. When attention was drawn to these, patient said that they were of no moment, gave rise to no inconvenience, and that she had had them for years. The mother and one sister had the same condition; three other sisters were free, and no male member of the family was affected.

Clinically, the diagnosis made was a form of Brooke's epithelioma adenoides cysticum, or Jarisch's tricho-epithelioma papulosum. A piece was excised for microscopic examination. A section showed a benign circumscribed tumour, an adenoma of the lanugo hair-follicles and of the sebaceous glands. The cells of the lanugo hair-follicles were mature; the gradual transition from the stratum spinosum to the horny layer was complete. In the centre lay a lanugo hair surrounded by a considerable amount of horny material, containing eleidin granules. The sebaceous glands were embryonic, consisting of masses of epithelial cells, only few of which had become true sebaceous gland-cells. Around the adenoma was a considerable infiltration of both small round cells and plasma-cells.

In cutting the block further down and staining some of the sections the appearance given above was completely altered. It showed an adenoma of the sweat-ducts, a syringo-cystadenoma. There were some small solid masses of epithelial cells, none of which branched. There were also some small cysts, many of which were filled with colloid

material, others quite empty, the wall of the cyst in every case being made up of two layers of cells. That part of the inner layer facing the cavity was hyaline in appearance. There was no membrana propria, but the cysts were surrounded by a layer of very fine spindle-cells with a long nucleus quite distinct from the connective tissue around. These spindle-cells had been described several times, and were looked upon by most observers as being muscle to contract the sweat-ducts. Deeper down in the subcutis were the sweat-glands, which were quite normal in structure. Here there was a case of a mixed tumour—a form of tricho-epithelioma papulosum on the one hand, and a syringo-cystadenoma on the other. The first section, in the exhibitor's opinion, differed only from the cases described by Brooke, Jarisch, and others in that the cells of the tumour were more mature, more benign, and if one could speak of a cutting-off of epithelial cells, then the cells represented in the section could be said to have been cut off at a later date in embryonic life than those represented in Brooke's and other cases. In Brooke's cases, for instance, none of the epithelial cells had become transformed into true sebaceous gland-cells, and the epithelial cells surrounding the lanugo hairs did not show the transition forms from the stratum spinosum up to the stratum corneum as one saw in normal epidermis.

The name tricho-epithelioma did not mean malignancy, since the term "epithelioma" was used on the Continent for any collection of epithelial cells, innocent or malignant.

The great peculiarity of this tumour was its mixed character; its further significance was that it formed a link and connected all the cases from those first described by Kaposi under the name of lymphangioma tuberosum multiplex—viz., Jacquet and Darier's hydradénomes éruptifs, the cellulomes épithélials éruptifs of Quinquaud, the hæmangio-endothelioma of Jarisch, the endothelioma tuberosum colloides of Kromayer, the nævi cystepitheliomatosi disseminati of Gassmann, and the acanthoma adenoides cysticum of Unna.

Case of Acroteric Sclerodermia.

By T. D. Savill, M.D.

The patient was a woman, aged 56, who had coldness of the extremities, and to some extent of the nose. The condition had come on during the last two and a half years. Her mother was subject to

angioneurotic œdema, and died after an attack in the throat; and the patient was much afraid of similar attacks coming on in herself. A third generation—i.e., the patient's daughter—was also subject to attack of angioneurotic œdema, showing a distinct family strain. The patient presented three evidences of that vaso-motor neurosis. Alternating with the angioneurotic attacks she had been subject, almost throughout life, to obviously migrainous attacks, the headaches lasting twenty-four hours at a time. Thirdly, she had been subject from time to time to what she called "burning patches" (patches of transient erythema) on different parts of her body. At the present time all four extremities were in a hide-bound condition, but the feet were less affected than the hands. The case had been shown before the Clinical Section,[1] but Dr. Savill believed it would interest the members of this Section also. He had tried many things, including ergot, calcium lactate, thyroid extract, fibrolysin, massage, galvanism, pituitary body tabloids, and salicylate of soda, without effect. Latterly she had had a continuation of the massage, and one of the complex iodine bodies, viz., periodo-tetrahydro-paramethyloxychinolin periodide, 5 gr., thrice daily. Under this treatment she had at first seemed to improve, but she had not done quite so well lately. With regard to the pathology of the condition, it was, in his belief, a toxi-angioneurosis—i.e., a vaso-motor instability chiefly referable to the ends of the extremities, associated with some toxin, which, circulating through that part of the extremities where the blood-stream was slowest, had more effect in causing a sclerosis of the tissues. He had not found any gastro-intestinal derangement.

DISCUSSION.

The PRESIDENT said he did not regard it as a case of the classical sclerodermia. The hands were suggestive of it, but the limbs were in a state of chronic doughy œdema, and the skin was not involved in the sclerodermic process.

Dr. PRINGLE did not wish to traverse Dr. Savill's diagnosis, but the case was certainly not a typical one of sclerodermia. The induration appeared to him to be subdermic rather than dermic, and the tips of the fingers were quite soft to palpation. Dr. Pringle had, however, himself observed some cases beginning in a similar manner and ultimately eventuating in fairly typical sclerodermia; and in a certain number of these cases the initial phenomena were paroxysmal and of the nature of angioneurotic œdema.

Dr. PERNET said such a case was likely to go on to true sclerodermia. He considered it came into the category of sclerodactylia of Ball.

[1] *Proceedings* (Clin. Sect.), 1909, ii, p. 200, and 1910, iii, p. 33.

Dr. PERNET, answering Dr. Galloway, reminded Dr. Colcott Fox of a case he (Dr. Fox) had seen at Fulham Infirmary years ago, and diagnosed as one of Raynaud's disease. When Dr. Pernet saw the case several years later the condition had become distinctly one of sclerodermia of the sclerodactylia type. Dr. Pernet had observed other cases go on much in the same way.

Dr. SAVILL, in reply, said the name "sclerodermia" was not the one he really sent in, but "acroteric sclerodermia" (a name given to the affection by Sir Jonathan Hutchinson), and he had intended asking whether he was justified in regarding this histologically as the same change as ordinary sclerodermia, like that met with in cases of morphœa. In favour of that view was the fact that at one time the patient had a patch of sclerodermia on the clavicle, the remnants of which could now be seen.

Two Cases of Mycosis Fungoides presenting quite different Aspects of the Disease.

By J. H. SEQUEIRA, M.D.

IN the first the mycosis tumour was single, and in the second the greater part of the cutaneous surface was affected.

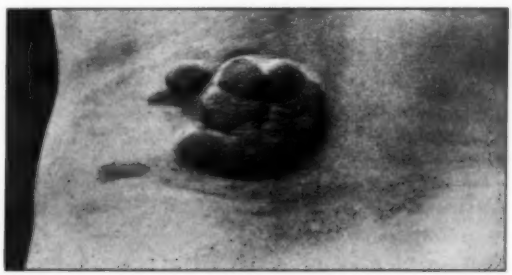
CASE I.

W. P., aged 56, a railway guard living at Enfield, attended the London Hospital for the first time on October 14, 1909. The patient was married, but had no children. The wife had had no miscarriages. There was no history or evidence of syphilis. Until four years ago the patient had enjoyed good health. Since 1905, however, there had been severe itching all over the body, sometimes chiefly in one part and sometimes in another. The pruritus was so intense that the patient could not forbear scratching. Scratching always increased the itching. He had never noticed any skin eruption except the lesions to be presently described. Twelve months ago he detected two dark-brown patches on the skin of the lower part of the back, both together covering an area of about a crown-piece. These lesions were described as looking at first "like bruises." Later they became raised and formed lumps, and the colour changed from a brownish tint to red. The lumps itched "terribly."

On admission to the hospital the patient's general health was good, and there was no evidence of visceral disease. Between the twelfth

rib and the crest of the ilium there was a swelling beginning 1 in. to the left of the spine and extending transversely for 3½ in., and measuring vertically 2 in. The colour of the lesion was reddish brown; its outline was irregular and its surface lobulated. It stood out above the surrounding skin for about ¾ in. in the centre. The swelling was moderately hard, and moved freely on the deep structures. The skin round the tumour was pigmented. To the outer side and below it there was a flat ovoid plaque of pinkish colour, about 1 in. in its long diameter. This area on palpation gave the impression of infiltration. Factitious urticaria had always been present.

The patient had had four pastille doses of X-rays at intervals of a week, and when shown at the meeting the tumour was decidedly flatter, less nodular, and the inner margin less defined. The pigmentation of



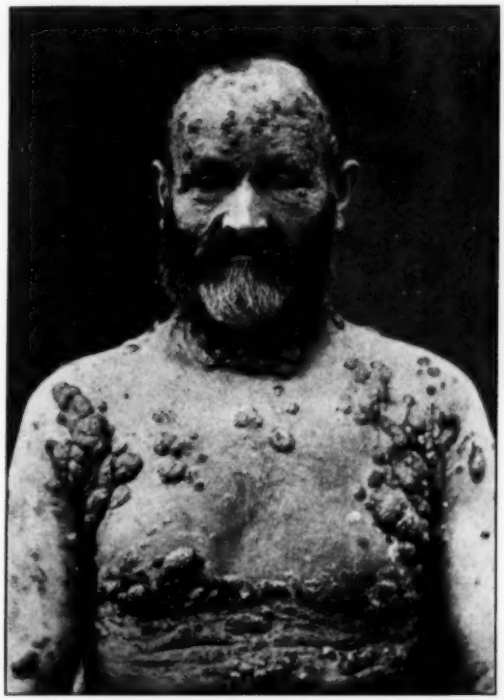
Mycosis fungoides (Case I).

and around the swelling had distinctly increased under the X-ray treatment. There had been no obvious enlargement of the lymphatic glands. A blood examination was made by Dr. Panton, who reported as follows:—

	October 14	December 14
Erythrocytes	5,500,000	5,612,000
Hæmoglobin	90 per cent.	95 per cent.
Colour index	0·8	0·9
Leucocytes	9,000 per c.mm.	10,000 per c.mm.
Polynuclear neutrophiles	83 per cent.	78 per cent.
Eosinophiles	0 ,,	0 ,,
Small lymphocytes	9·5 ,,	14·5 ,,
Large lymphocytes	5 ,,	3 ,,
Large hyaline cells	2·5 ,,	3·5 ,,
Transitional cells	0 ,,	0 ,,
Coarsely granular basophiles	0 ,,	1 ,,
	100	100

CASE II.

G. C., aged 58, caretaker, admitted to the London Hospital, November 13, 1909. The patient lived near Salisbury. He had been married twice. His first wife had died of phthisis. The second wife had had no children. There was one healthy child by the first marriage. The



Mycosis fungoides (Case II).

patient had good health until five years ago, when he began to suffer from eczema. The eruption had appeared in patches, and for the past eight months the whole of the cutaneous surface had been affected. In September, 1909—that is, only two months before the photograph reproduced was taken—a number of swellings appeared first on the head and neck and later on the trunk and extremities. The patient complained

of great irritation, which was worse during the eczematous stage. On admission the condition was obviously grave. The patient, though cheerful, appeared distinctly ill, though there was no pyrexia. The whole of the skin was erythematous and in parts evidently infiltrated. An enormous number of lobulated nodules were present, chiefly on the head and neck, in the axillæ, across the chest, and on the thighs and groins. Scattered tumours were also present on the back. The majority of the lesions were ½ in. to 1 in. in diameter, and about ¼ in. to ⅓ in. above the surface. Many of them had a remarkably bossy surface. The lesions were dry except upon the face and scalp, where there was an eczematous exudation. The buccal cavity was not affected. The lymphatic glands were not obviously enlarged. The urine showed no abnormality.

Blood Examination (Dr. Panton).

Erythrocytes	5,650,000
Hæmoglobin	90 per cent.
Colour index	0·8
Leucocytes	9,400
Polynuclear neutrophiles	68·8 per cent.
Polynuclear eosinophiles	2·6 ,,
Small lymphocytes	12·2 ,,
Large lymphocytes	8·2 ,,
Large hyaline cells	8 ,,
Transitional cells	0 ,,
Coarsely granular basophiles	0 ,,
Finely granular basophiles	0·2 ,,

When shown at the meeting the patient had already improved a great deal. The erythema was less in the upper part of the trunk and on the face, and some of the tumours were decidedly flatter. The upper part of the trunk and the head had been exposed to the X-rays, but no part had received more than two pastille doses in the month the patient had been under treatment.

Sections taken from one of the tumours showed the growth to consist of round cells, of somewhat varying size, in a reticulum. No giant cells were met with in the sections observed.

The rapid evolution and extent of the tumour formation in this case were in great contrast to the localized lesion in Case I.

Dermatological Section.

January 20, 1910.

Dr. T. Colcott Fox, President of the Section, in the Chair.

Case of Mycosis Fungoides.

By P. S. Abraham, M.D.

The patient, J. J., aged 50, was an electrician by trade. He came to the West London Hospital in November, 1909, with a severe general dermatitis, and several ulcers looking like broken-down gummata, and scars of old ulcers on the legs and elsewhere. He gave a history of syphilis twenty-five years previously, and of having had a wart on the penis seven years ago. He had been otherwise always in good health. His mother died when aged 78. His father, aged 82, was still alive. He had taken much alcohol and smoked a great deal. His work subjected him to great changes of temperature.

The present trouble began about fourteen months ago: red eczematous patches appeared on the right knee and on both forearms, and afterwards on the abdomen and other parts. Last June a swelling appeared on the left tibia, and soon after others showed themselves in many places. He said that the first tumour was as big as a cricket ball. These growths had ulcerated and had left depressed scars. He had lost much weight, and also much of his hair, especially of the scalp. His skin all over was in a condition of obvious inflammation, and at times "wept" all over most copiously; there was then extreme pruritus. When the "weeping" ceased, a dry stage set in, and there was an enormous desquamation, including the hands and feet, in large flakes. A few weeks ago there had been an extensive vesicular and pustular

eruption on the arms and forearms, thighs and legs. At the present time there were several tumours to be seen of different sizes, some of them breaking down, together with the scars of those which had healed. There was considerable adenitis, and one large mass occupied the left groin. A blood-count had given: Red corpuscles, 5,850,000; hæmoglobin, 90 per cent.; leucocytes, 13,000. Differential count: Polymorphonuclears, 54·7 per cent.; mononuclear cells, 37·9 per cent.; eosinophiles, 7·3 per cent.

Mr. McDonagh had examined the blood for the Wassermann reaction and had found it positive. He had also made sections of one of the smaller tumours, now exhibited under the microscope, which he would describe.

The patient had had four applications of X-rays, the effects of which had not been judged yet, though they thought the groin tumour was somewhat softer as the result of the rays.

DISCUSSION.

Dr. PRINGLE asked if there was any enlargement of the spleen. The marked general involvement of the lymphatic glands, as well as the results of the blood-count, appeared to him exceptional, and to point towards the condition being identical with that previously described as "Lymphodermia perniciosa."

Mr. MCDONAGH discussed the pathological aspect from Dr. Abraham's case, from which he demonstrated some sections. The tumour appeared to consist of a plasmoma, well defined, and limited beneath and on either side by strands of new connective-tissue cells and normal plasma-cells. The plasma-cells of the tumour were increased in size, some had lost their contour, and the protoplasm had become granular. The nucleus was also increased in size, often filling up the whole cell, and staining faintly and showing one or two nucleoli. A gradual transition from a normal into a degenerated (?) plasma-cell could be seen. The plasmoma also contained new blood-vessels, similar to those met with in sarcomata—of an embryonic type. For comparison with this section there was one of a late syphilide just before ulceration. There was a plasmoma, but the plasma-cells were not in such great numbers. They were breaking down, but the protoplasm always remained hyaline, never became granular. The nucleus did not increase in size, and no nucleoli were to be seen. There were also no new blood-vessels. If such a difference could be demonstrated in further cases in the way a plasma-cell becomes changed, it will be a useful sign for diagnosis and prognosis—distinguishing innocent and malignant disease—therefore he asked that he might make an appeal for any material bearing upon the subject.

Case of Lichen Simplex Chronicus of Vidal.

By H. G. Adamson, M.D.

The patient was a young woman, a general servant, aged 24. The eruption had been present for about eight years. It had begun on the neck, had after some years appeared upon the thighs, and more recently behind the knees. On the outer and front aspect of each thigh was an elongated oval patch, measuring 3 in. to 4 in. by 1½ in. to 2 in., violet-red in colour, infiltrated, quadrillated, and here and there excoriated, and framed by a narrow band of pigmentation—in fact, a typical patch of so-called "lichenification," or "lichen chronicus simplex." The long axis of these symmetrically-arranged patches on the thighs was from just below the anterior and superior spine in a direction downwards and slightly inwards. On the back of the neck at its lower part were also two symmetrically-disposed similar but smaller patches; and there was also a patch behind each knee, these latter being less infiltrated, probably because of more recent origin.

The patient said that the patches itched intensely, and that she could not help rubbing these parts, which she did through the clothes. There was no itching of other parts of the skin, and firmly stroking the skin revealed no vasomotor irritability. The patient was otherwise in good health. Sometimes, for long periods, the itching had become less, and then the eruption had faded, though the patches had never quite disappeared at these times.

The case was in every respect a very typical example of lichen simplex of Vidal. A simple treatment for these patches was to cover them with adhesive plaster to prevent scratching, but X-ray applications were perhaps the best form of treatment. After a single Sabouraud-pastille dose the itching was usually relieved within a few days, and infiltration cleared up within three or four weeks. In some cases a second or third application might be necessary at intervals of a month to six weeks.

Multiple Keloid associated with "Neurotic Excoriations" of the "Dug-out" Type of Colcott Fox.

By H. G. Adamson, M.D.

The patient was a young woman, C. B., aged 24, a domestic servant. She had had an eruption for three years, and she had been under observation at St. Bartholomew's Hospital since September, 1909. When first seen the eruption had consisted of about a dozen typical keloid growths irregularly distributed over the body and limbs, and five or six recent split-pea-sized "dug-out" excoriations. While under observation these excoriations had healed, some with red, thickened scars, others with flat, pigmented scars, and fresh crops had continued to come out.

The patient admitted that she produced these lesions with her nails. She had said that a little itchy spot appeared, which she could not help scratching. On one occasion she had presented on the face a small red papule with an erythematous halo-like lesion of lichen urticatus, which, she said, was how they all came. The question of the relationship of these cases of "dug-out" excoriations to those presenting larger elongated oval excoriations had been discussed by Dr. Colcott Fox at the last meeting,[1] and by Dr. Adamson when exhibiting cases on former occasions.[2] The present case was shown on account of the multiple keloids, a complication which had been previously noted.

Dr. Pringle did not consider that the case was one of true neurotic excoriation. He was familiar with the condition, especially in neurotic young women, and considered it as an example of so-called acne urticata, in which there were present distinct, deeply-seated papules, and the depth of the lesions resulted from their being *dug* at with the patient's nails rather than being merely scratched.

[1] *Proceedings*, p. 28; *Brit. Journ. Derm.*, 1910, xxii, p. 17.

[2] *Proceedings*, 1909, ii (Dermat. Sect.), pp. 67 and 158; *Brit. Journ. Derm.*, 1909, xxi, pp. 21, and 296.

Case of Onychia Sicca Syphilitica.

By H. G. Adamson, M.D.

The patient, J. W., a house painter, aged 56, first attended the hospital in August, 1909. He then presented a squamous syphilide on the scrotum, and some oval, shallow ulcerations, and flat condylomata upon the tongue. There was also an affection of all the nails of the fingers and toes of three months' duration. At that time the distal two-thirds of the nail was normal, while the proximal third, the newer growth nearest to the root, was replaced by an opaque, roughened, irregular condition of nail, sharply marked off from the older normal portion. The patient was treated with mercury and iodide of potassium, and the eruption on the scrotum had disappeared and the mouth condition improved. He had then ceased to attend, and had not appeared at the hospital until written for a few weeks ago. He had then presented the condition of nails now seen. The roughened opaque growth of nail had gradually advanced, pushing the normal portion before it, until many of the nails were entirely replaced by the diseased condition, while only a narrow band of normal nail remained at the distal ends of others. Two or three of the nails remained for about half their length, and these were the seat of roughened, blackened pittings, in linear arrangement from before backwards. The nails which had grown up and gradually replaced the normal structures were opaque, ridged vertically, with coarser and more wavy transverse ridgings, friable and easily split at their free edge, so that this had a denticulated appearance. The condition of nails seen had obviously arisen from some interference with their growth at their roots, and was not due to a primary affection of the bed or to one spreading inwards from the surrounding skin. A diagnosis of syphilis, psoriasis, eczema, or ringworm would be impossible from their appearance, but the association of the affection with syphilitic lesions, the absence of any evidence of eczema or of psoriasis, the failure to find ringworm fungus after repeated search, suggested very strongly that it was an onychia sicca of syphilitic origin. The fact that all the nails of the fingers and toes had been affected simultaneously was also opposed to the diagnosis of tinea, and in favour of a general nutritional disturbance as the cause of the

nail dystrophy. Apart from the lesions observed, there was nothing in the patient's history pointing to syphilis, so that the date of the nail lesions in regard to infection could not be arrived at.

Onychia sicca syphilitica, or scabrities unguium syphilitica — i.e., onychia unassociated with periungual or subungual syphilitic lesions— was apparently a very rare disorder, for Heller, among 8,000 to 9,000 syphilitics, found only three cases. Fournier, however, had described it (onyxis craquelé) as of somewhat frequent occurrence with syphilis in women.

DISCUSSION.

Dr. PRINGLE agreed with the exhibitor that the condition of the nails was syphilitic—i.e., that they would not have existed if the patient had not contracted syphilis. It was common knowledge that nail lesions, whether specific or not, responded very slowly to any form of treatment. He was rather astonished to hear that the condition was so rare as Dr. Adamson stated it to be, as he had had several examples of it in his practice, although not of such marked severity as that exhibited.

Dr. WHITFIELD said he had a case at King's College Hospital which he believed to be syphilis of the nails. The man had had syphilis about twenty years; the nails were friable, and there was a good deal of perionychia. The case resembled trichophytosis unguium so closely that he had on several occasions examined tissue for fungus, but always with a negative result. On the other hand, anti-syphilitic treatment seemed quite unavailing.

Dr. MACLEOD asked Dr. Adamson if he regarded the onychia as a characteristic syphilitic lesion due to syphilitic disease at the nail-matrix, or simply a dystrophy due to malnutrition resulting from his disease. Dr. MacLeod had seen the same type of onychia in connexion with general malnutrition and an imperfect peripheral circulation, and considered that it could not be regarded as characteristic syphilide.

Dr. ADAMSON said that writers were not agreed as to whether the onychia sicca in syphilitics was due to local lesions of the nail-matrix or to general nutritional disturbance. The simultaneous affection of all the nails in this case suggested the latter. The difficulties in the way of microscopical examination in these cases made it almost impossible to decide the point.

Lichen Planus in a Diabetic.

By J. Galloway, M.D., and A. Winkelried Williams, M.B.

Dr. Williams said that the patient, Mrs. —, aged 61, had presented, when first seen, in October, 1909, an old linear lichen planus on the left foot, and a diffuse eruption of typical lichen planus papules over the legs, and a few papules also upon the arms. The main interest of the case was the presence of a large quantity of sugar in the urine. The patient had had diabetes for many years. A strict carbohydrate-free diet was prescribed, and an ointment of carbolic, perchloride of mercury and zinc, and salicin internally. A week later Dr. Williams saw her again. Her breath then reeked of acetone; her urine gave a deep diacetic-acid reaction and had a strong chloroform-like smell; a large quantity of sugar was still present and also albumin, and the patient showed distinct symptoms of air-hunger. The lichen planus had spread with intense rapidity. The diffused papules had coalesced into sheets of eruption, covering the entire thighs, the upper arms, the whole of the back, and great part of the abdomen. The general condition of the patient was obviously most serious. She suffered a great deal of distress from the eruption, but, on account of the albuminuria, Dr. Williams had feared to give her either arsenic or mercury treatment. The main distress was relieved by menthol lotions locally and phenacetin internally. The patient, after remaining in a serious state for two weeks, began to improve. The diacetic-acid reaction cleared up, the air-hunger vanished, and the glycosuria improved. She had then consulted Dr. Galloway, who found the glycosuria had quite gone. The glycosuria and albuminuria were still absent when Dr. Williams examined the urine later, and had remained so for a month. She had vastly improved under internal treatment with ♏ xx doses of liq. hydrarg. perchlor. three times daily. Dr. Williams had never previously seen a case of lichen planus associated with glycosuria, nor a case so extensive or spreading so acutely. On searching the literature he was only able to find one paper on the association of lichen planus and diabetes—i.e., by Dr. E. Hoffmann, an abstract of which was published in the *British Journal of Dermatology*.[1] He records four cases—one of Brocq's, one of Saalfeld's, and two of his own. Dr.

[1] *Brit. Journ. Derm.*, 1906, xviii, p. 298.

Williams regretted that he had not had made a quantitative analysis of the urea elimination. Radaeli had stated that lichen planus under treatment by arsenic improved as the urea elimination diminished. This fact suggested to his mind the possibility of the explanation of the severity of the rash in this case being due to the large urea output of a diabetic, and the necessarily high nitrogenous diet.

DISCUSSION.

Dr. GALLOWAY said that he had been consulted by this lady on the recommendation of Dr. Cohen, and subsequently had been in communication with Dr. Williams respecting her condition. He had little to add to the account already given by Dr. Williams; it was of interest, however, to note that the attack of lichen planus seemed to be giving less trouble, and she was certainly in a much better state so far as the effects of the diabetes mellitus were concerned. The point that seemed to be of special interest in this patient was not the simple coincidence of lichen planus and diabetes, but that in this case of long-standing diabetes of the "hereditary" type, when, on account of various circumstances, there had occurred an excessive formation of oxybutyric acid in the tissues, with the evidence of acetone and diacetic acid in the urine, then the outbreak of lichen planus—and it had been one of the most extensive and severe outbreaks the speaker had witnessed—rapidly appeared. The occasional association of lichen planus and glycosuria had been noted, and in the case of this particular patient, with the family history she gave of many relatives being affected with the disease, there was a greater numerical probability of the coincidence of any other affection with diabetes; still, even after making this allowance, the outbreak of severe lichen planus when she was in greatly depressed health as a result of her disease seemed to be more than a fortuitous occurrence. The power of the toxæmia in diabetes of producing severe forms of peripheral neuritis should be borne in mind by those who may be inclined to support the nervous origin of lichen planus. The urine was now free of sugar and had been so for some time. It was to be hoped that the attack of lichen planus would continue to disappear.

Dr. ADAMSON suggested that the pruritus, which was a common symptom of diabetes, might have acted as the exciting cause of the eruption of lichen planus. There were some authorities who believed that the eruption of lichen planus was determined by the scratching in a predisposed person. Hallopeau had recorded a case [1] of lichen planus developing concurrently with scabies, and the speaker had seen a similar case.[2]

Dr. WHITFIELD said, with regard to the theory quoted by Dr. Adamson that the eruption in lichen planus was the mere result of scratching, he

[1] *Annales de Derm. et de Syph.*, Par., 1903, 4me sér., iv, p. 352.
[2] *Brit. Journ. Derm.*, 1903, xv, p. 378.

considered that this theory was disproved by the fact that some of the most acute and widespread cases did not itch and were not scratched.

Dr. ADAMSON said that he did not hold the view that the eruption of lichen planus was solely the result of scratching; but it was generally recognized that local irritation helped to determine the incidence of the eruption on particular localities, and it was conceivable that an intense pruritus, with resulting scratching, might be just sufficient to turn the scale and induce an outbreak in a person who might otherwise have escaped.

Dr. WILLIAMS, in reply, confirmed the statement that the patient had had diabetes a long time, and added that two of her sisters were at present ill with it. The lichen planus had supervened comparatively recently.

Two Cases of Urticaria.

By G. W. DAWSON, F.R.C.S.I.

THE patients, a young man and a young woman, who were companions, both suffered from urticaria when exposed to the cold. Blisters came out on the hands and under the eyes, but disappeared when they got warm again.

Dr. STOWERS suggested that as a common cause must be the explanation of the condition, the two patients might have been simultaneously affected by a toxin of dietetic origin.

Case of Rodent Ulcer.

By G. W. DAWSON, F.R.C.S.I.

THE patient had a superficial rodent ulcer of the inner canthus of the eye. Sections (exhibited) showed numerous cell nests in an otherwise typical rodent ulcer growth.

Sclerodermia of the Diffuse Type in a Woman aged 58.

By E. G. GRAHAM LITTLE, M.D.

SHE began to show signs of illness two years ago, when persistent vomiting was present for a week, and, though temporarily stopped, this recurred from time to time ever since. The vomiting took place immediately after food, which caused pain and retching. At about the same time the hands and fingers began to "faint"—i.e., get blanched—

and subsequently congested, the alternation being attended by some pain. The fingers began to be "shiny" about eighteen months ago, the wrists to get stiff about nine months ago. She had had constant pains in the joints (the knees, ankles, and neck), but there was no sclerodermic skin in any of these positions.

The present appearances were as follows: The fingers of both hands were uniformly affected, the fingers swollen, tense, and very shiny; slight flexion at the pharyngeal joints was still possible, but the metacarpal joints were stiff. The hand was also swollen and shiny and very hard; no pitting was possible. The wrist was quite immobile, the skin over the forearm being firmly adherent to the underlying tissue, which was much wasted, so that the forearm was thin, the surface white, very smooth, and shiny. The altered skin reached as far as the elbow; above this area the skin appeared normal. Sensation of touch, of pain, of heat and cold, were present in the altered area. The hands were excessively cold to the touch. No other portions of the body were as yet affected. On the little finger of the right hand there was a small ulcer, the result of local injury, which appeared very intractable.

The origin of sclerodermia in an infective toxæmia had been maintained by some authors—e.g. Leredde. The commencement of this case with chronic vomiting would seem to lend some colour to the possibility.

Dr. Stowers asked whether the early stages of the condition were associated with any acute disorder or disease. A similar case (of which he had drawings), but more severe, and of upwards of twenty-four years' duration, he exhibited at the Dermatological Section of the International Medical Congress held in London in 1881.[1] The patient, a married woman, was well in general health until her first pregnancy, at the age of 23, when she suffered severely. From that time cutaneous disorder followed, and typical generalized sclerodermia (sclerodermia diffusa) developed.

Sclerodermia of the Circumscribed Variety with unusually numerous Patches.

By E. G. Graham Little, M.D.

The patient was a boy aged 18; he had begun to have the disease, to his knowledge, six months ago, the earliest symptom having been itching in the parts, afterwards becoming sclerodermic. Movements

[1] Vide *Transactions* (7th Session), Lond., 1881, iii, p. 189.

began to be impeded later, and there was already some atrophy and wasting. The following was the distribution of the disease :—

Right arm : A large area of the forearm was involved over the middle two-thirds of the posterior surface, and there was a small oblong inlet of sclerodermic tissue on the anterior surface ; above the elbow the greater part of the external and posterior surfaces of the upper arm were involved in sclerodermic change, and, again, a small patch on the upper anterior aspect, over the prominence of the biceps muscle.

Left arm : Very similar and quite symmetrical distribution as on the right upper extremity.

There was a patch of sclerodermia over the lower third of the sternum, and another over the lower part of the sacrum ; there were several small isolated patches in the internal tibial groove on the back. There was some pigmentation on both sides of the neck in brown patches the size of from sixpence to a shilling, with small areas of sclerodermia in immediate contiguity.

There was a small patch of sclerodermia on the inner side of the right leg just above the knee, and a larger area just below the inner condyle of the femur, and a patch the size of the palm of a hand on the external surface of the right leg. The left leg was very similarly affected, the patches being again curiously symmetrical with the right side. In all the areas affected the skin was whitish yellow, intensely smooth, thin, and atrophied, no hairs or hair-follicles being visible on the surface, and the underlying tissues were obviously wasted. Redness had never been present, and there was no semblance of the "lilac ring" described as morphœa.

Case of Recklinghausen's Disease.

By E. G. Graham Little, M.D.

The patient was a man aged 20, rather short of stature and somewhat under the average of intelligence. The tumours were extraordinarily numerous, although small for the most part, and better felt than seen. On the forearms numerous lines of small tumours could be felt running down for the most part along the course of nerves ; over the abdomen small shotty tumours could be felt and seen on lateral inspection, forming a veritable mosaic. There were very few larger, but nevertheless small, isolated, fleshy "molluscous" tumours ; most of these were

hard, small swellings, very slightly, if at all, tender. Pigmentation, the second cardinal symptom of this disease, was scanty, and limited to a circumscribed efflorescence of small freckle-like macules on the part of the abdomen just above Poupart's ligament on each side, with some few larger isolated "café-au-lait" patches on the thighs and lower part of back. There were no tumours in the mouth.

The patient had been treated with injections of fibrolysin, a capsule (2·3 c.c.) having been injected intramuscularly about twice a week while he had been an in-patient for about four weeks; at longer intervals, generally of about a fortnight, while he had been out of the hospital. The injections appeared to be followed by headache, and had not so far been attended by any manifest improvement in the size of the tumours or otherwise.

Case of Syphilitic Erythema Nodosum.

By J. E. R. McDonagh, F.R.C.S.

A girl, aged 21, had come up to St. Bartholomew's Hospital, complaining of a sore lip. The sore had appeared at Christmas, and three weeks after the patient had enlarged glands in the neck. Fourteen days after the sore was noticed a rash appeared on the trunk and limbs.

On examination the sore on the lip had proved to be a chancre and the rash a typical syphilitic eruption. From the knees to the ankles on both legs, both in front and on the sides, there were circular, bluish-red papules about the size of a shilling. When they first appeared they were slightly painful, the pain only lasting for two days, without disappearance of the lesions. The papules were hard, well circumscribed, discrete, and not tender. Where the skin was normal, subcutaneous hard nodules could be felt in the course of the veins.

Under the influence of mercury the lesions had become scaly and tended to disappear without ulceration. The appearance of the lesions, and the absence of pain and tenderness, and their coming out with the rash, left no doubt that they were of syphilitic origin, especially as subcutaneous nodules (phlebitis) were palpable in the normal skin.

The exhibitor was indebted to Dr. Adamson for kindly allowing him to show the case.

DISCUSSION.

Dr. WHITFIELD said that he had seen one case in a woman which ulcerated, but she had a good deal of varix. The ulceration was in patches, but never very severe. He deprecated the use of the term "erythema nodosum syphiliticum," and considered that it was a nodular syphilitic phlebitis.

Dr. MACLEOD said that he regarded the name "syphilitic erythema nodosum" as an unfortunate one and apt to lead to confusion. He thought that the condition might be better described under the heading of "syphilitic nodular phlebitis," as it corresponded to the affection minutely described by Philippson in connexion with tuberculosis, to which he had given the name of "phlebitis nodularis necrotisans."

Mr. MCDONAGH replied that he called it "erythema nodosum" because, when first seen, that was the first condition which occurred to one. It was also so described in text-books.

Chancre of the Tongue.

By J. E. R. MCDONAGH, F.R.C.S., and COUBRO POTTER, M.D.

DR. POTTER said the sore had appeared at Christmas, and was accompanied by a sore throat. Soon after the patient noticed the sore a gland had begun to swell on the right side of the neck opposite the thyroid gland (on the opposite side to the tongue lesion). On January 18 a rash had appeared over the trunk, the tongue lesion being nearly healed by this time but densely infiltrated, and glands were palpably enlarged on both sides of neck and submaxillary regions. The rash was a typical early syphilide.

Mr. McDonagh had not been able to find the *Spirochæta pallida* by the dark-ground illumination method, probably because the ulcerated surface had granulated over, but Wassermann's reaction had given a strong positive result.

Non-ulcerative Tuberculides following Lupus Vulgaris.

By J. H. Sequeira, M.D.

The patient was a male aged 20; he was born at Ipswich, and lost his parents in early childhood, and therefore could not give any particulars as to the family history. When he was aged 9 a patch of lupus appeared on the end of the nose, and gradually spread until the whole of the nasal skin and both cheeks were involved. He had had four operations (erasion) before he came to the London Hospital in 1906. The nose and adjacent parts of each cheek were then the seat of a remarkably warty lupus, but at the edges of the affected areas there were characteristic "nodules." In the central parts there was some scarring, the result of the previous scrapings. Dr. Sequeira did not remember to have seen a more warty lupus, and a preliminary curetting was performed, followed by a few applications of the X-rays. After the surface treated had healed the Finsen treatment was applied. The patient attended the Light Department for some fifteen months, and the lupoid lesions were then quite cured. The parts were rather badly scarred, and there had always been an excess of pigmentation in them. The general health had been good throughout.

From time to time the patient had come up for inspection, and eight months ago it was noticed that there was an eruption of a papular character on the forehead and behind both ears and the adjacent part of the neck. The lesions must have appeared very rapidly, as a month only had elapsed to the time they were noticed from the previous inspection. Some of the spots on the forehead had disappeared, leaving small depressed cicatrices. The eruption consisted of hemispherical papules all about one size, about 2 mm. in diameter. They were remarkably symmetrical, and about thirty or forty in number. They had a purplish-brown colour, and felt like shot imbedded in the skin. There had been no ulceration, and those which had disappeared had done so spontaneously. One nodule had been excised, and the following report of the microscopical appearance had been furnished by Dr. Turnbull, Director of the Pathological Institute at the London Hospital:—

"The skin is horny, and furnished with hairs and sebaceous gland. Sweat-glands are also present. There are a very few cells with yellow

pigment-granules close beneath the epidermis, and there are some fine pigment-granules in the Malpighian layer throughout the section. In the dermis is a large rounded cellular nodule. This does not quite reach the epidermis. Passing down to its summit is a hair-follicle. In the upper part of the nodule, close to the termination of the hair-follicle, is a large rounded space. This contains concentric lamellæ of horny substance, and is bounded by one or two layers of flattened squamous epithelium. No stratum granulosum is seen in this epithelium. The nodule is well defined, but not sharply limited. The stroma of the nodule consists of a reticulum of fine fibrils, with spindle and elongated oval nuclei. In the central part the nuclei are very abundant. The fibrils are very fine and stain yellow with van Gieson; in the periphery the nuclei are less numerous and the fibrils stout and stained red. The fibrils at the periphery merge with the collagen fibres of the dermis. This reticulum contains capillaries, and is densely infiltrated with free cells. The majority of these are lymphocytes, but eosinophile polymorphonuclear leucocytes are present in very large numbers. In the lower part of the nodule there is one small area in which there are large lumps of an extra-cellular yellow pigment. In three sections stained by the Ziehl-Neelsen method no tubercle bacilli were found. No giant cells were observed."

The lesions were clearly not lupoid, and their symmetrical arrangement and chronicity had led to the diagnosis of "tuberculides." In spite of the absence of any tendency to break down, the members present agreed with the view expressed by the exhibitor.

Dr. PRINGLE suggested that a guinea-pig should be inoculated for the purpose of confirming the diagnosis.

Case of Chronic Tuberculosis Cutis.

By J. H. STOWERS, M.D.

THE patient, a man, was a commercial traveller, aged 64, sent by Dr. C. Owen Fowler, of Thornton Heath. Ten years ago the left hand was struck against the sharp lid of a tin tea-canister. A small piece of skin, the size of a pin's head, was dislodged, and stamp adhesive paper was applied at once, followed by other unauthorized applications. The

wound did not heal, but a slow process of superficial ulceration followed. The diagnosis had been confirmed by microscopical examination. The ulcer which now exists over the metacarpo-phalangeal joint of the left forefinger is of irregular shape and measures 1½ in. by 1¼ in. A few small pale-coloured islets of new skin are visible upon the surface, and, although some attempts at healing is discernible, there is a tendency to spread on the radial side, and much pain is complained of. The bones are unaffected. The patient had had X-ray treatment without apparent benefit. At the present time the limb is carried on a splint and dressed with hydrarg. oleat. 5 per cent., with antiseptic covering, &c.

The question was raised whether Finsen light or radium treatment was likely to assist recovery.

DISCUSSION.

Mr. DAWSON said he would be inclined to cut the lesion out and then graft.

Dr. STOWERS replied that he had a case of localized tuberculosis cutis under his care three years ago, in the person of a lady who came from India with the history that she injured the back of her right hand against a tooth of a pet cow she was in the habit of feeding. Microscopical examination proved the nature of the disease, and it was excised with permanent good result. An early incision in the case he was now exhibiting would have been the best treatment.

Dermatological Section.

February 17, 1910.

Dr. T. Colcott Fox, President of the Section, in the Chair.

Case of Alopecia Areata.

By P. S. Abraham, M.D.

(Exhibited by J. E. R. McDonagh, F.R.C.S.)

The patient was a woman, aged 46. Her hair had commenced to fall out ten years ago just before a severe attack of typhoid fever, the scalp having been very scurfy previously. During and after the fever the alopecia had increased and become complete. The patient had been under Dr. Abraham's care for two years, and during this period a great deal of hair had grown. The treatment had been with antiseptic ointments, stimulating lotions, and occasional application of Burt's fluid with iron, arsenic, and nux vomica internally. The marked improvement had been since applications of Hersheimer's lithantrol had been employed ; but consequent upon its use the follicles of the still nude area on the vertex were filled with comedo-like plugs, which were easily expressible—simulating tar acne on other parts of the body without the concomitant inflammation. The alopecia had been a complete one, not in areas, and now the hair had grown completely all round with a few sparse hairs on the vertex.

Case for Diagnosis.

By H. G. Adamson, M.D.

The patient was a married woman, aged 43. She had had six children, the youngest of whom was aged 5. She was somewhat stout. Except for the eruption to be described, she appeared in good health.

The eruption had begun two months ago as a red spot in the skin, 1½ in. above the right nipple. In the course of the next three weeks the spot had grown to a patch about 2 in. long and 1 in. broad. One month ago a second patch had appeared upon the outer side of the right arm just above the elbow, and a third upon the dorsal surface of the left foot, just at the junction of the fourth and fifth toes. Quite recently the margins of her ears had become slightly swollen and red. The lesion on the right breast was irregularly rectangular in shape, and measured about 2½ in. by 3½ in., the nipple occupying the centre of the lower half. It consisted of a bluish-red, sharply margined patch, slightly raised, but presenting no feeling of infiltration to the finger, most of the colour and elevation of the lesion momentarily disappearing on firm pressure. It suggested, not a solid infiltration, but rather an erythema with exudation. Just within the upper margin of the patch were some firm millet-seed-sized rounded nodules standing out pale and pink against the dark background of the patch, and firm to the touch. On the right arm, just above the elbow, there was a similar, but smaller, rounded patch of the size of a two-shilling-piece. It was less deeply blue in tint than the patch on the breast. It presented the same ring of nodules towards its margin. The patch on the left foot was similar in appearance, and of an even more dusky hue than that on the breast. Two ill-defined nodules could be felt at its margin. The margins of the ears were of a dusky-red colour, with here and there some feeling of lumpiness. Since the patient had been first seen eight days ago some fresh lesions had appeared in the form of hemp-seed-sized nodules with a dusky-red halo. There were two on the left breast, and one on the right. At the middle one third of the lower lip, immediately below and parallel with the red margin, there had appeared a bluish-red band with slight elevation above the skin around.

A biopsy and sections of one of the nodules on the arm had been made by Mr. McDonagh, who had reported as follows: "There is marked œdema of the epithelial cells, especially in the region of the hair-follicles, this œdema extending down as far the papilla. There is a hardly perceptible small round-cell infiltration in the corium. The section somewhat resembles an urticaria. There is no evidence of granuloma."

The members present agreed with the exhibitor that at present it was impossible to make a diagnosis.

Case of Granuloma Annulare.

By Haldin Davis, M.B.

The patient was a male infant, aged 16 months. Apart from the skin-lesions it was healthy; it was the first child of its mother, who had had no miscarriages. The mother was healthy, and there was no suspicion of a tuberculous or syphilitic taint. The eruption, which had persisted six months, was chiefly situated on the buttocks, but had spread down the legs almost as far as the ankle. The appearance was that of radiation from the anus, as it was there that the lesions were most numerous—in fact some of them merged into one another; on the legs the individual lesions were quite isolated. Each of the lesions consisted of a ring slightly raised above the level of the surrounding skin, and distinctly palpable to the finger. Each started as a nodule, but as the periphery extended outwards a cup-shaped depression with normal skin was left in the centre. The fully developed ring consisted of a number of nodules closely set together. The eruption did not appear to cause any pain or other inconvenience to the patient.

The case was interesting because it appeared to be the youngest patient in which that condition had been noted. It would be remembered, too, that granuloma annulare was commonly found on the hands and feet, but apparently in young children it was not infrequently found on the buttocks and thighs, as in this case, for Dr. Adamson had supplied the exhibitor with a list of four other cases with the eruption occurring in that situation.

Case of Xerodermia Pigmentosa with Unusual Features.

By Willmott Evans, F.R.C.S.

The patient was a girl, aged 11, and, according to the mother's account, early in the first year of life she developed some brownish pigment-spots on the face. At first they were thought to be freckles, but they increased greatly. Every winter, at first at least, there was slight amelioration, but in the succeeding summer the condition always grew worse. She complained of no symptoms, but she was somewhat worried by the appearance. The child showed a large number of pigment-spots on the face, especially on the forehead, surrounding the

nose, and on the chin. On the neck there were very few, and none on the arms, body, or legs. The pigment-spots were of a deep-brown colour, and in parts almost black. For the most part they were discrete, but near the inner canthus of the right eye there was a brown "flash." The lips were also deeply pigmented, and pigment in large amount was also to be seen on the mucous membrane of the cheek, but there was none on the tongue. No atrophic areas were present, and only one or two telangiectases. No warts were to be seen.

Mr. Evans said that he considered the case to belong to the same group as ordinary cases of xerodermia pigmentosa, in spite of the absence of telangiectases, atrophic spots and warts, and in spite of the absence of pigmentation on the forearms and legs. That the pigment-spots were not ordinary freckles was, in his opinion, rendered certain by the marked pigmentation of the mucous membrane of the lips and mouth. Cases of xerodermia pigmentosa of various degrees of intensity had been described, and this case was not much less severe than some of the more slightly marked cases which had been recorded, and in respect of the intra-buccal pigmentation it was especially well marked. There was no family history in this case.

DISCUSSION.

The PRESIDENT (Dr. Colcott Fox) said the case was interesting, especially in regard to the involvement of the buccal mucous membrane; but he was not inclined to accept the diagnosis of xerodermia pigmentosa when lentigines were the only symptom, though he allowed the diagnosis was often a nice one in slight or abortive cases of xerodermia pigmentosa.

Dr. PRINGLE asked if the patient had been unduly exposed to the sun, as occurred in most children who had xerodermia pigmentosa. There appeared to be no family history of the disease in the case exhibited, and he thought the diagnosis could only be firmly established by further developments.

Dr. F. PARKES WEBER said he believed that the freckle-like spots on the face (especially around the mouth) and the black spots in the buccal mucous membrane might be merely a form of pigment nævus. Dr. H. D. Rolleston had drawn attention to a very similar condition of pigmentation of the circum-oral skin of the face and of the buccal mucous membrane in an anæmic young man, exhibited by him at the Clinical Section of the Royal Society of Medicine on October 8, 1909.[1] In that case, however, the skin of the hand was likewise pigmented and had a slightly atrophic appearance, so that Dr. Weber suggested at the meeting in question that the condition might be one of "incomplete" xerodermia pigmentosa.

[1] *Proc. Roy. Soc. Med.*, 1910, iii (Clin. Sect.), p. 9.

Disseminated Lupus in a Girl aged 4½.

By E. G. Graham Little, M.D.

The patient, I. S., had had measles two years ago, and, as the eruption of measles died away, the present lesions were noted, appearing almost like an enanthem, the small lupus nodules developing nearly at the same time. At present the child had small nodules of definitely lupus type, giving the characteristic buff colour on pressure with a glass slide, distributed as follows: The largest patch was on the right cheek; now it was the size of a sixpence. This was said to have commenced in the same way and at the same time as the other and smaller nodules. In all there were about twenty-five such lesions, scattered widely and rather symmetrically over back and trunk. The nodules were the size of a split-pea, and appeared to have remained nearly stationary, with the exception of the face-lesion, which alone had grown much larger than the others. The father was seen, and was the subject of the most extensive and chronic tar acne, his employment being in a gas-works. There was no history of tubercle either in the family or in this patient. A somewhat similar case also occurring after measles had been shown some time ago by Dr. MacLeod.

The President said these phases of tuberculosis were of great interest in relation to the so-called tuberculides. As a rule nearly all the lesions died away spontaneously, but some might remain and spread to form lupus patches. In a case under his care with a great number of disseminated lesions, all died away except one on the hand, which persisted and grew into a warty patch.

Case of Syphilitic Onychia.

By J. M. H. MacLeod, M.D.

The patient was a middle-aged man who had contracted syphilis fifteen years before. After the disappearance of the secondary symptoms he had been free from any signs of the disease till five years ago, when his left hand had become affected with a typical scaly palmar syphilide. This resisted treatment for some months, but temporarily cleared up under injections of benzoate of mercury. Early in 1909 it appeared again, when it was associated with a suppurating perionychia of three

of the fingers of the same hand. At the time of exhibition a number of scaly, brownish pink macules and patches were present on the left palm, and the second, third, and fourth fingers were affected with the perionychia. The nail-wall of the finger was raised and inflamed, and at the lateral borders of the nail was superficially ulcerated. The nail-plate was thickened, opaque, and of a dirty-greyish colour, and anteriorly had softened and partly sloughed. The condition was the result of an inflammation and suppuration of the tissue around and beneath the nail, leading to destruction of the nail-plate and imperfect growth at the nail-matrix.

Pityriasis Rubra in a Man aged 21.

By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed., and S. E. Dore, M.D.

When the patient was aged 9 he noticed scaly spots scattered about his body and limbs, the largest patch, on his thigh, being about the size of the palm of his hand. There was nothing on his elbows or knees. As a result of treatment by chrysarobin, the skin of the whole of his body became acutely inflamed, the redness being followed by profuse scaling. The scaly patches did not, however, entirely disappear. He continued to have attacks of acute redness, followed by desquamation, once or twice a year, sometimes very severe and sometimes mild, until the age of 14. Various ointments were tried without benefit. In 1903 he improved under creolin ointment and baths, and his skin remained clear for six weeks. The condition, however, again relapsed, and was again treated with chrysarobin ointment. Since this time the attacks have continued, the longest interval being one of eighteen months, when he was having vapour baths. As a rule the attacks were not accompanied by constitutional symptoms, but in 1908 a more than usually severe attack was attended by a temperature of 104° F., and severe pains in the feet and hands, with swelling of some of the joints. Since 1908 his condition has been much as it is at present—i.e., universal redness with scaling, accentuation of the natural lines of the skin, with here and there slightly darker patches apparently corresponding to the sites of previous patches of psoriasis. From time to time areas of white skin appear, but the redness on the chest and face are permanent. Examination of the urine showed the presence of a substance which caused slight reduction

of Fehling's solution. The daily estimate of the urea varied from 2·3 per cent. to 2·7 per cent. The blood showed a slight diminution of red cells (4,500) and a slight increase of white cells (10,000). All external as well as internal drugs seemed to aggravate his condition, and he was being treated with simple inunction of vaseline. His sweat function is entirely in abeyance, and recently he has been unable to take vapour baths. Nothing abnormal could be found in his visceral organs.

Dr. GALLOWAY said that he was interested to note that, at any rate on one occasion, the urine of the patient had reduced Fehling's solution. One of the most severe cases of exfoliative dermatitis which had come under the speaker's observation, and which ended fatally, for some weeks before the fatal termination passed urine which constantly reduced Fehling's solution. This reduction was due in this case to the presence in the urine of homogentic acid, and the other substances producing the condition described as "alkaptonuria." On account of this phenomenon, the patient had been originally sent to Dr. Galloway with the suggestion that she was suffering from diabetes. So far as the speaker was aware, this was the only occasion on which the occurrence of alkaptonuria with severe disease of the skin had been noted, and he suggested that the urine in Sir Malcolm Morris's case might be re-examined with this observation in view.

Case of Lupus Disseminatus Faciei.

By J. H. SEQUEIRA, M.D.

THE patient, a married woman, aged 33, had had good health until eighteen months ago, when a number of spots appeared on the face. She had had four children, who were all healthy, and there was no history of tuberculosis in any recognizable form in any member of the family. From the history it would appear that the eruption had come out gradually during the past eighteen months. Spot after spot appeared, and as the eruption did not clear she came to the London Hospital for advice. On the middle part of each cheek and the adjacent sides of the nose there were discrete papules the size of a millet-seed, of a reddish-brown colour. There were twenty-one separate lesions on the left cheek and side of the nose, and fifteen spots on the right side. Four scattered papules were present on the forehead. In some places three or four papules occurred in a line or in a small group. They were distinctly palpable, and some had a tiny depression in the centre. Under the diascope they presented a semi-translucent appearance, but not exactly

of the apple-jelly character. The lesions did not conform to the characters of Barthélemy's acnitis and Radcliffe-Crocker's acne agminata. They appeared to the exhibitor to be of the disseminated follicular lupus type of Tilbury Fox. No biopsy had been made, and the reaction to tuberculin had not yet been observed.

Dr. PRINGLE considered the condition identical with what the late Dr. Crocker described as "lupus erythematosus nodularis." He had observed three cases of it for prolonged periods, all of which ultimately proved to be frankly tuberculous. Dr. Liddell, of Harrogate, had published a full account of a well-marked example with microscopic findings, which he (the speaker) had in the first instance erroneously diagnosed as an erythematous case.[1]

An Unusual Case of Lupus Erythematosus.

By J. H. SEQUEIRA, M.D.

THE patient, a Polish Jewess, aged 52, was shown as a case of lupus erythematosus of unusual type. She was married, and had had five children; four of them were healthy, and one had died in convulsions in early infancy. There had been no miscarriages, and there was no history or evidence of syphilis. There was also, so far as could be ascertained, no history of tuberculosis in the family, but owing to linguistic difficulties this point was not clear. Seven years ago a lump was said to have appeared on the left cheek, the patient scratched it, and some fluid was said to have come away. From this central spot the eruption had spread until it involved the whole of the left cheek and the left side and root of the nose. The area had apparently increased rather more rapidly in the past three years. The patient had never consulted a doctor or attended a hospital for the cutaneous affection, but several weeks ago she consulted a medical man about a hernia, and he advised her to have treatment for the face. The left cheek was covered with a dirty, brownish, scaly patch, which was limited above by a curved line passing below the eyelid and extending from the root of the nose to just outside the outer canthus. The outer margin was a nearly vertical line ending below at the level of the lobule of the ear. The lower margin curved downwards to the level of the upper lip and then passed upwards on to the middle third of the nose. The median border crossed to the right side of the nose at the upper part. The surface of the patch was scaly, and here and there

[1] See *Brit. Journ. Derm.*, 1900, xii, pp. 319 *et seq.*

were crusts which exuded a small quantity of thick pus. At the root of the nose was a deep depression surrounded by thick scale. The outer and lower margins of the areas were livid red in colour and slightly scaly. They had the characters of the lesions of lupus erythematosus. On removal of portions of the thick scale, conical depressions were found in the true skin. There was also a small oval area a little larger than a pea below the main patch between the angle of the mouth and the angle of the jaw. This spot had a red margin and a central cicatricial depression. On the buccal mucosa on the right side there was a patch of redness with some central cicatrization about 1 in. across, and on the left side there was a small white area not larger than a pea. The scalp and ears were unaffected, and there were no lesions on the hands.

The case presents some difficulty in diagnosis, but the evidence, in the exhibitor's opinion, on the whole favoured lupus erythematosus. It was unusual on account of the long duration with unilateral distribution. The buccal lesions were of the type which occurred in lupus erythematosus.

The diagnosis of lupus erythematosus was generally agreed to, and Dr. J. H. SEQUEIRA, in reply to a question as to whether syphilis could be entirely excluded, added that he felt little doubt that it was really the condition named, but he would have the Wassermann reaction tried.

Case of (?) Mycosis Fungoides.

By A. WHITFIELD, M.D.

THE patient was a man, aged 53, who had been under Dr. Whitfield's care for the last year. On first coming to the hospital he had stated that he had been suffering from an eruption on the body, limbs, and face for ten years. This eruption was then seen to consist of a redness of the skin over the trunk and limbs, accompanied by a little scaling. Almost the whole of the body was affected, but in places apparently normal skin was present, and in these places the eruption bordered on the healthy skin with a sharply defined and somewhat crescentic edge. There was slight infiltration to be felt, but this was of a very soft character and gave to the finger the impression of œdema of the epidermis rather than of infiltration of the true skin. A provisional diagnosis of mycosis fungoides was made, and a biopsy was performed, the patient being treated with sulphur and salicylic-acid ointment in case the eruption was of the nature of a seborrhœic dermatitis. The

biopsy was not in any way characteristic, there being only the slightest mononuclear infiltration beneath the epidermis, with no exaggeration of the wavy line joining the epidermis and the papillary body, such as one would expect if even very early mycosis fungoides were present. The infiltration was certainly not more than one would find with any dermatitis of so long standing. In the epidermis itself there were a very few non-leucocytic vesicles. Shortly after visiting the hospital the patient became rather ill and fell into bad circumstances, and entered the infirmary, where he had a somewhat severe attack of boils. Later he returned to the hospital, but no treatment did him any good (X-rays had not been tried). On the whole, perhaps, the eruption had become a little more marked while he was under observation, but the change was not striking.

On exhibition, the whole of the trunk was covered with a dusky-red eruption, with a few areas of white skin showing between the large patches. At these points the eruption showed a very definite edge of polycyclical outline. The arms and legs were similarly affected. Infiltration was present, but not marked, and in no part was there any tumour formation. Over the left clavicle, the left iliac region, and on the right side of the chest there were oval areas measuring about 2 in. by 1½ in., where the skin was of a dark-brownish purple, and scaling was marked, but over most of the body the scaling was very slight, being, perhaps, kept in check by ointments. Itching was very slight. The glands in the axillæ and groins were large and painless, but he had had generalized furunculosis and a slightly desquamating eruption for many years. Dr. Whitfield said that he was intending to do another biopsy, and would report on it later if the patient submitted to it. He was very doubtful if the case was in reality mycosis fungoides, but he thought it was suspicious. The excellent health of the patient and the almost complete absence of itching were, he thought, against it. If it was mycosis fungoides, it was certainly impossible to diagnose it when he made the first biopsy.

DISCUSSION.

Dr. PRINGLE had seen the case in his out-patient practice about four years previously, and had then arrived at the diagnosis made by Dr. Whitfield. The patches were then more discrete and more scaly, and he well remembered their peculiar livid bluish tint, which was now so prominent a feature of them.

The PRESIDENT remarked that the case was almost certainly one of mycosis fungoides, but in the pre-tumour stage the clinical evidence was often not decisive.

Dermatological Section.

March 17, 1910.

Dr. T. Colcott Fox, President of the Section, in the Chair.

Two Cases of Chronic Superficial Dermatitis in Patches with Symmetrical Distribution.

By H. G. Adamson, M.D.

These cases were examples of an eruption to which the exhibitor had drawn attention in the *British Journal of Dermatology*,[1] which was seen mainly in children, and which consisted of reddish, roughened, circumscribed patches, of from ½ in. to several inches in diameter, with roughly symmetrical distribution on the sides of the face, on the arms and legs, and sometimes on the trunk. These patches seemed to be made up of pin-head-sized follicular papules so that they resembled patches of lichen scrofulosorum, but on closer inspection it would be found that the apparent papules were really small superficial crusts. Histologically these were shown to be dried-up vesicles which had formed in the horny layers of the epidermis. A striking feature of the eruption was its *marked resistance to treatment*, and the exhibitor had had many cases under observation for years without having been able to cure the eruption. Chrysarobin would sometimes remove the lesions for a time, but it had always returned in the same areas. In the paper referred to Dr. Adamson had discussed the diagnosis of these eruptions from lichen scrofulosorum, from seborrhœic eczema, from parapsoriasis

[1] *Brit. Journ. Derm.*, 1908, xx, p. 109.

"*en plaques*," and from parakeratosis psoriasiformis, and he believed it to be distinct from each of these. It was possibly the same eruption as had been described by Boeck as "eczema scrofulosorum," but in not one of the cases had there been any evidence of tuberculosis, and two cases tested by Calmette's test had given negative results. The occasional association with otorrhœa and with impetigo contagiosa in the early stages had at first suggested to him a possible microbic origin, but repeated attempts had failed to demonstrate the presence of a micro-organism, while the failure to cure the eruption by local measures seemed to negative a local infection. Both the cases shown were typical examples as regards the character of the lesions, their distribution, and their persistence.

M. C., aged 4, had had the eruption some eighteen months. It had begun on one thigh, and the other patches had since gradually appeared. There were now about twelve circumscribed, red, rough patches, with almost symmetrical distribution on the face, arms, thighs, and legs. On each side of the face, in front of the ears, was a patch extending from the forehead nearly to the angle of the jaw—a characteristic position. On superficial examination many of the patches were strongly suggestive of lichen scrofulosorum, but closer inspection showed the pin-head-sized elevations to be minute crusts. The eruption did not give rise to itching. There was no deep-seated redness and swelling as in eczema, and the patches did not become eczematized when subjected to local irritation.

The second case, L. G., was a little girl, aged 12, who had been attending the hospital for the past two years. The first diagnosis had been lichen scrofulosorum, but it had soon been found that this was not correct, and that the case belonged to the class which had just been described. In this case the patches were typical and had the typical distribution on the sides of the face, the arms and the legs, but there was an unusually large number of patches on the trunk, which fact had probably led to the earlier diagnosis of lichen scrofulosorum. Among the applications which had been used were chrysarobin, salicylic acid and resorcin, pix carbonis preparata and Lassar's paste, but no treatment, either stimulating or soothing, had led to any but slight temporary improvement.

Case of Lichen Planus Annularis.

By H. G. ADAMSON, M.D.

THE patient was a man, aged 40. He had had the eruption for twelve months, and for six months he had taken arsenic in full doses without improvement. The eruption occupied mainly the forearms, the lower part of the back, the thighs, and the legs. It consisted of scattered papules of the ordinary flat-topped, angular type intermixed with ring-like lesions of all sizes up to that of a sixpenny-piece. The rings consisted of a narrow, raised, flat-surfaced, violet-coloured circular band enclosing a pigmented area. That they had arisen by the outward extension of single papules could be clearly seen by observing the different sized rings, from that of a papule, with a slight central depression, through all stages up to the largest rings. There were no lesions on the mucous membrane of the mouth, but on the glans penis there were three well-marked annular lesions of the size of a threepenny-piece to that of a sixpence. There was a distinctly annular lesion on the palm of the right hand. The case was now being treated by perchloride of mercury internally. The exhibitor had a few days ago seen another case which resembled this in all points, and in which also there were large ringed lesions on the glans penis. In a third case under observation at the present time the general eruption was that of ordinary lichen planus, but there were three or four large annular lesions on the back.

DISCUSSION.

Dr. MACLEOD said that he had shown a similar case of annular lichen planus in a man, aged 26, in January, 1908. As in Dr. Adamson's case, annular lesions were present on the glans penis and the rings were formed by partial extension of a single papule. He had had the case under observation ever since and had found it most resistant to treatment. At the present time the ringed lesions had disappeared, but there were several hypertrophic patches on the legs. New groups of papules still kept coming out from time to time.

Dr. WHITFIELD said he had noticed that if one of the lesions became annular, the others did so too. The only place on which lesions frequently became annular without the whole eruption doing so was the glans penis, where, he thought, annular lesions were common.

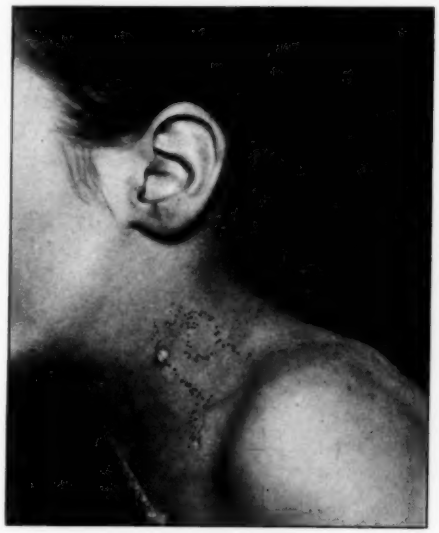
Dr. J. J. PRINGLE said that in his experience the intramuscular injection of soluble arsenates produced more rapid and more definite improvement than any other form of treatment in cases such as that shown, the first evidence of amelioration being the diminution or cessation of itching. He was fully aware of the bad results reported from this method of treatment, but he had never observed them, except in one case, when rather severe gastro-intestinal symptoms on two occasions followed 7-gr. doses of the drug employed (orsudan). In the same patient 4-gr. doses injected once a week caused no bad symptoms, and the patient rapidly improved. Dr. Liveing prescribed perchloride-of-mercury solution habitually for many years, but he (the speaker) had never been able to satisfy himself as to its efficacy, or, indeed, of any other drug except arsenic, which had to be pushed to its physiological limits if its beneficial action was to be obtained, and this course of action was often impracticable. The point ought not to be lost sight of that some cases of lichen planus spontaneously disappeared in the course of a few months, while others lasted apparently indefinitely.

Linear Nævus of Unusual Type (Nævus Acneiformis).

By HALDIN DAVIS, M.B.

THE patient shown was a little girl, aged 4. She was a healthy child, and the condition which was the occasion of her appearance before the Section gave rise to no inconvenience. On the left side of the neck there was to be seen a double row of black dots set at regular intervals and forming a figure on the skin of the same shape as a note of interrogation (?), but with the convexity pointing to the left instead of to the right. On closer examination each of these dots was seen to be a comedone, firmer and of larger size than the ordinary comedone, projecting from the mouth of a follicle. One of them had been infected by one of the skin-organisms and had given rise to a pustule of the kind commonly associated with an infected comedone in acne vulgaris. According to the patient's mother, the condition had been noticed during the first month of the patient's life, but the extent and size of the eruption and the size of the black dots had gradually increased to their present dimensions. From the history and the regular arrangement of the large comedones, a diagnosis was made of a linear nævus affecting the sebaceous glands. It evidently is of the same pathology as the case exhibited by Dr. Selhorst, of The Hague, at the International Dermatological Congress in 1896; but Dr. Selhorst's case was in a much older patient (also a female),

and the condition was far more extensive, and the adventitious inflammation due to infection with skin bacteria had caused pus-formation and scarring, which had interfered considerably with the pattern of the congenital nævus. Following the nomenclature of Dr. Selhorst, the present case might be called one of nævus acneiformis unilateralis. The treatment proposed by the exhibitor of the case was excision as far as possible (which would give the opportunity of a biopsy, the result of which he hoped to show the Section at a later date) and cauterization of the remaining follicles.



Nævus acneiformis unilateralis.

Case of Pseudo-xanthoma Elasticum.

By E. G. Graham Little, M.D., and J. H. Sequeira, M.D.

The patient had been seen by Dr. Little at St. Mary's Hospital about seven years ago, when she was aged about 12, and was subsequently shown to the Dermatological Society of London. The clinical appearance had not changed appreciably in the interval. She had now

come up to Dr. Sequeira's clinique for treatment. A section of the skin was obtained by Dr. Little seven years ago, and the specimen exhibited at the meeting showed the characteristic appearances of the disease as described by Balzer, Darier, and others. The elastic tissue seemed to form the bulk of the tumour; it was greatly increased in quantity, and was swollen and broken up into fragments and granular debris, which occupied the corium below the papillary zone.

The PRESIDENT said he remembered, many years ago, Sir Malcolm Morris kindly asking him to see two sisters from Ireland, in whom the diagnosis of pseudo-xanthoma elasticum of the neck was made. They looked exactly like the present case.

Two Cases for Diagnosis.

By E. G. GRAHAM LITTLE, M.D.

CASE I.

THE patient was a man, aged 22, who complained of painful areas at the back of the body, over the sacrum and buttocks. In those positions where pain was located there could be felt on deep palpation a circumscribed, cutaneous or subcutaneous swelling, about the size of a shilling; opinions differed as to whether this swelling was in or under the skin; the surface was not altered appreciably, and but for the pain complained of the swellings would probably have escaped notice; these were barely appreciable to sight, but quite definite to touch. The history which the patient gave was that he had been kicked in the groin some two or three months ago, that a swelling had formed on that situation, and that this swelling had been transposed to the back—an obviously fictitious explanation not unconnected with his desire to obtain compensation for the injury. It was not certain how long the swellings had lasted at the back; there was no inflammation round them, and they probably could not have been produced artificially. One of the swellings had been excised and was now under examination, and a histological report would be furnished later.

Dr. WHITFIELD said that on lifting the skin between the finger and thumb he obtained a sensation of marked lobulation, while the skin felt quite soft over the thickening. He believed that the case was one of multiple, painful lipomata, of which he had seen several instances.

CASE II.

The patient was a man, aged 48. He had had, three years ago, an affection of both legs, which had been treated for six months in St. George's Hospital as neuritis, for which he had been admitted. Six months later he noticed "discoloured places" on the legs, and at the present time the following clinical condition was noted: On the middle of the inner surface of each leg there was an irregular patch of bluish-purple skin raised above the surrounding level, not altered by superficial pressure upon it, and having persisted for the last two years unchanged. This patch had a map-like outline, covered an aggregate area of about 3 in., and at the free margins numerous small telangiectases were noticed —cayenne-pepper-like spots—which did not alter on pressure. Similar smaller patches were present on the skin just below the internal and external malleolus on both legs. There was, and had been, no ulceration of the surface, and there were no subjective symptoms. Varicose veins were present conspicuously on both sides. The tint varied from time to time, being darker or lighter. There were no lesions elsewhere. The colour, the outline, the symmetry, and elevation of the patches suggested the diagnosis of lichen planus hypertrophicus to the exhibitor; but no history or evidence of typical lichen planus could be obtained, and the small telangiectases at the margin were not suggestive of that diagnosis. The exhibitor had had another exactly similar case, with distribution similar to the present one, which had been shown to the Section. In that instance ulceration had finally occurred in the raised purplish patch, but rather recalled a peculiar disease which Schamberg [1] had described —an instance of which the exhibitor had shown [2] at the Dermatological Society of London. In both Schamberg's and this latter case a probably hæmorrhagic pigmentation had been noticed on the knees of a boy, with telangiectases at the margin of the main patch, which had been equally chronic in duration. In that case the diagnosis of lichen planus had also been suggested, but no opportunity of confirming it had occurred. A biopsy would be performed in the present case, and a report supplied later.

[1] *Brit. Journ. Derm.*, 1901, xiii, p. 1.

[2] *Ibid.*, 1902, xiv, p. 266.

DISCUSSION.

Dr. H. G. ADAMSON was of opinion that the affection was lichen planus hypertrophicus.

Dr. PRINGLE was perfectly familiar with the clinical condition presented by the patient. He had always regarded it as a "hæmostatic" phenomenon, associated with the dependent position of the part and a certain, but variable, amount of varicosity of veins. The term "dermatitis hæmostatica" had been aptly applied to it some years ago, he thought by Dr. Klotz, of New York. He could not admit that the disease presented any affinity, either pathological or clinical, to lichen planus. The essential lesions, which were easily visible at the edges of, and outside the main patches of, the case exhibited, were persistently dilated vascular telangiectases, from which the blood could not be expressed on diascopy. When these innumerable small lesions coalesced a certain amount of thickening and keratosis occurred, but the resemblance to lichen planus thereby produced was of the most superficial character. In his opinion the condition was analogous to, although, of course, not identical with, the "angiokeratoma" on a very large scale. If a pus infection supervened the patient would present an ordinary "varicose ulcer." He reminded the Fellows of the Society that there was no direct proportion between the amount of varix and that of dermostatic dermatitis present in any given case. In the man exhibited there was decided, although not very marked, varix.

Dr. WHITFIELD considered it more probable that varicosity of the veins was responsible for the alteration in the skin—all degrees of discoloration were seen in the skin of the legs affected with varicose veins. He regarded this as an exceptional degree of the quite common hæmorrhagic changes seen in varicose veins.

Case of Phlebitis Nodularis Syphilitica.

By J. E. R. McDONAGH, F.R.C.S.

S. W., A WOMAN, aged 38, had come up to St. Bartholomew's Hospital on September 4, 1909, with a typical secondary syphilitic eruption. Under treatment by injections of mercury salicylate the rash had rapidly improved. On December 1 the patient complained of severe pain on the inner side of the thigh. There was nothing to see, but on palpation one could feel, in the line of the internal saphenous vein, about 1½ in. above the knee, a hard, tender, spindle-shaped swelling, roughly 1 in. in length. The diagnosis of syphilitic phlebitis was made. As time went on this swelling came more and more to the surface, and finally ulcerated, giving the picture of a late syphilitic ulceration. Papules developed along the

course of the vein, both above and below the ulcer. "Nodules" of phlebitis were also found in both legs, some of which had come to the surface and ulcerated. The patient had varicose veins.

The exhibitor said that one should draw the distinction between this case and the one shown before the Society some time back,[1] under the name of "erythema nodosum syphiliticum." The syphilitic erythema nodosum resembled the true erythema nodosum so closely that it rightly deserved that name. It was a rare syphilitic eruption, which made its appearance before, or synchronously with, the secondary rash, quickly disappeared, showed no tendency to ulceration, and was limited to the legs, especially the anterior surfaces. The syphilitic phlebitis nodularis was a later manifestation. It most commonly affected the lower limbs because of the prevalence of varicose veins, and the lesions almost invariably ulcerated.

The exhibitor was indebted to Dr. Adamson, who had been kind enough to allow him to show the case.

Specimen of Spirochæta Pallida prepared with Indian Ink, with Demonstration of Method.

By J. E. R. McDonagh, F.R.C.S.

The specimen showed the *Spirochæta pallida*—white against the dark indian-ink background. There were also some red blood-corpuscles, which aided in the diagnosis of the specific spirochæta, since the diameter of a corpuscle was roughly half that of the organism. To prepare the specimen a glass pipette should be driven through the skin at the border of the chancre and along underneath it. This was better than getting the material from the surface, since one obtained no extraneous organisms. A little blood would run up the pipette, which should be kept vertical for a few minutes until the serum had separated, and then that part which contained the blood-corpuscles should be broken off so as to allow a drop of the serum (which would contain enough corpuscles) to fall upon a clean slide. Around the drop should be placed six or seven platinum loopfuls of distilled water and the same quantity of indian-ink (Günther and Wagner). The three substances should be thoroughly mixed and spread out on the slide with another glass slide, in the way that a blood-film was made, allowed to dry, and examined with an oil-immersion lens. This was not only the simplest but the

[1] *Proceedings*, p. 50.

most satisfactory way of demonstrating the spirochæta. It could be found in this way in every chancre, obviating the expense of a dark-ground condenser.

Case of Epidermolysis Bullosa.

By G. Norman Meachen, M.D.

The patient, a young married woman, aged 29, was sent to the Prince of Wales's General Hospital by Dr. J. Mansie, of Kingsland Road, where she was first seen by the exhibitor. Her father, aged 32, and a sister, aged 13, both died of phthisis. Another sister was now in a home for incurables with "tuberculosis of the joints." She had been married eight years; there were no children and no miscarriages. The special liability to blistering of the skin had been present "as long as she could remember." A bulla, which was occasionally hæmorrhagic, would arise upon any part of the cutaneous surface from slight traumatism or from the pressure exerted by tight garments. She was not aware of a like condition in any of her ancestors. The general health was good.

On examination the lower limbs were covered by excoriations and shallow ulcerations in various stages of healing, as well as several minute atrophic scars of older lesions. One or two fresh bullæ were present in the left axilla and upon the left thigh. The nails presented a very curious dystrophic condition, the distal portion of the nail-plate being broken away and the remaining proximal portion much thickened and heaped up, as well as being also longitudinally striated. All the nails, both of the hands and the feet, were similarly affected. A differential blood-count, made by Dr. MacDonald, pathologist to the hospital, showed that there was no eosinophilia, nor, indeed, any special feature, except that there were "large numbers of hæmatoblasts very much in excess of the usual." There had been no affection of the buccal mucosa nor of the conjunctivæ.

The President said there was some atrophy going on in the legs. There was great vulnerability of the skin, so that a little touch caused an effusion of serum and blisters formed. He would remind the members of a woman, long under his care, who first suffered in adult life from ordinary pemphigus, and for many years up to the present time the typical picture of epidermolysis had been established. Sir Malcolm Morris published a somewhat similar case with portrait in the *British Journal of Dermatology* some years ago.[1]

[1] *Brit. Journ. Derm.*, 1902, xiv, pp. 267, 268.

Case of Argyria.

By F. Parkes Weber, M.D., and R. H. Norman, M.D.

This case, when shown recently before the Clinical Section of the Society,[1] was thought to be an example of bluish hæmachromatosis of the skin, similar to the pigmentation in a patient who was formerly in a hospital of Montreal, where she was known amongst the inmates as "Blue Mary." The case of "Blue Mary" had been described by Dr. Maude Abbott,[2] and a Canadian doctor who had seen "Blue Mary" at Montreal had said that the pigmentation in the present case resembled her pigmentation, but was less in degree.

The present patient, an unmarried woman, aged 46, was remarkable for a decided, though slight, ashy-bluish tinge of her skin, which was best seen on the forehead, at the sides of the nose, around the mouth, and on the trunk, but was scarcely noticeable on the arms and legs. The mucous membrane of the mouth had a very slight slaty tinge, similar to, but less marked than, that of the face. On the parts flushed with blood, including the lips, tip of the nose, pinnæ of the ears, and the fingers, the peculiar coloration could only be detected when the blood was pressed out of the skin—for instance, by a flat piece of glass or a glass lens. There was no pigmentation of the sclerotics, such as was found in ochronosis. There was no chromidrosis or "seborrhœa (steatorrhœa) nigricans." There was no true cyanosis, except sometimes in cold weather, when the hands became blue. Nothing abnormal could be detected in the thoracic or abdominal viscera at present. Neither spleen nor liver was enlarged. In 1890, however, she was treated in a London hospital for gastric ulcer. Since then she has suffered occasionally from pains in the epigastrium or chest after eating. In May, 1907, she was operated on by Mr. E. C. Stabb, at the Great Northern Hospital, for hernia, a femoral hernia on the right side being found to contain an adherent vermiform appendix, which was excised. The bluish coloration of the skin was present at that time, and was thought to have come on gradually about six years previously (about 1901). Examination of the patient's blood (Dr. G. Dorner) gave the following results: Hæmoglobin (by Sahli's method), 80 per cent. of the normal; red cells, 4,653,330 in the cubic millimetre; white cells, 12,330 in

[1] *Proceedings* (Clin. Sect.), p. 140.
[2] *Trans. Path. Soc. Lond.*, 1900, li, p. 66.

the cubic millimetre. Stained blood-films showed nothing abnormal. By spectroscopic examination only the normal absorption bands of oxyhæmoglobin could be seen; there was certainly no band due to sulph-hæmoglobin or methæmoglobin. The blood-serum was clear. The urine was free from albumin, sugar, and other abnormal constituents, and did not darken on being kept. Systolic brachial blood-pressure, 135 mm. Hg. Menstruation natural. Excepting for chronic constipation and occasional headache, the patient had no complaints. She thought that the peculiar coloration of her skin was not progressing. She had no idea what could have caused it. No lotions (such as might have contained silver nitrate or carbolic acid) had ever been used. A patent medicine and some "liver pills," which she had employed for many years as purgatives, had been chemically examined for a trace of silver, but with completely negative results. The patient had now, however, produced an old prescription for a pill given her for gastric symptoms, and dated December, 1889. This pill contained $\frac{1}{4}$ gr. of silver nitrate and $\frac{1}{2}$ gr. of opium powder, and was to be taken three times daily. She thought she had taken this pill, on and off, for quite two years after coming out of the hospital in 1890, and had continued to take it occasionally for gastric pain until seven years ago. The present case was of interest in regard to those mentioned by Dr. Radcliffe-Crocker,[1] in which he was inclined to attribute the slaty pigmentation of the skin to hæmachromatosis, because no history of the ingestion of nitrate of silver was forthcoming.

Peculiar Form of Pigmentation of the Face and Neck in a Woman aged 40.

By A. Whitfield, M.D.

The history was that the patient had been under Dr. Whitfield's care about three years ago for chronic eczema of the arms, due to scrubbing, and had also returned about eighteen months ago for a recurrence of the same trouble. On the first occasion nothing peculiar in the way of pigmentation had been observed, but on the second occasion it was noticed that her cheeks were very dark. She had been kept under observation for the last eighteen months, and the pigmentation had certainly become more marked during that time. Quite recently, on

[1] Radcliffe-Crocker, "Diseases of the Skin," 3rd ed., 1903, pp. 614, 615.

stripping the neck, it was noticed that the thyroid was enlarged, and it was then found that she had a slight, fine tremor of the hands, so that it was possible that she was developing Graves' disease, though she had no eye symptoms. Arsenic had not been given at the hospital, and no history could be obtained of her having taken any.

The pigmentation occupied the posterior parts of both cheeks immediately in front of the ears, and extended from the line of the malar eminence down to the angle of the jaw. Below this it was limited to the sides of the neck, and hardly affected the front at all. It did not extend much on to the chest. There was no marked pigmentation elsewhere if one took into consideration the fact that the patient was a dark woman. On the face the pigmentation was an even sheet with small white areas of about ¼ in. across dotted all over it, strongly suggestive of arsenical pigmentation. On the neck the pigmentation was less even, being arranged in an irregular pattern not unlike that of lichen planus, but no papules had ever been seen upon it. In a side-light the pigmented areas could be seen to have a peculiar sheen upon them almost as if they were polished.

The patient was in good health, with the exception of a little neuralgia, for which she had taken headache powders, but Dr. Whitfield said that he did not think most of such powders contained arsenic. He was of opinion that the case was in reality early Graves' disease associated with pigmentation. Dr. Whitfield said that this association was well known, but that in his experience most of the pigmented cases had been either in the hyperpigmentary stages of leucodermia or had been due to the arsenic with which the patients had been treated.

Case of Kératodermie Blennorrhagique.

By J. H. Sequeira, M.D.

The patient was admitted to the London Hospital under the care of Dr. F. J. Smith at the end of February, 1910. Dr. Sequeira was asked to see the case, and, on his recognizing the condition, Dr. Smith very kindly gave his consent to the patient being brought before the Section. Dr. Sequeira desired to tender his grateful thanks to his colleague for the opportunity of demonstrating the first instance of this remarkable condition which has been shown in this country.

W. L., aged 31, a clerk, unmarried, was attending the London

Hospital sixteen years ago with acroasphyxia and phenomena suggesting Raynaud's disease. Cicatrices were still present on the nose and lobules of the ears, left by the conditions then observed. The patient had enjoyed good health until six years ago, when he had an attack of gonorrhœa, which ran a simple course. In December, 1909, one week after exposure to infection, the patient again noticed pain on micturition and discharge. He treated himself with sandalwood oil, and in two weeks the discharge ceased, and had not returned. At the time of the appearance of the urethritis a small superficial sore, evidently a soft chancre, appeared on the glans penis. With the suppression of the gonorrhœal discharge, the right knee became swollen and painful, and about a week later the patient consulted a medical man, who advised rest in bed. He had been in bed almost continuously since. There has been no history of rash, sore throat, or falling hair. Wassermann's test was performed, with negative result. In the first week in February small swellings appeared on the soles of both feet; the patient described them as feeling like "pimples." At the same time the right elbow, right shoulder, and right sterno-clavicular articulations became swollen, very painful, and tender. There was also painless swelling about both ankles. After several weeks' treatment with "tonics" the patient was sent to the hospital, and admitted under Dr. Smith.

On admission, there was no discharge from the urethra, but the skin of the penis and scrotum was in an eczematous condition, and there was the small scar of the soft chancre on the penis. There were synovitis and peri-articular swelling of the right knee, accompanied by extreme tenderness. The left knee was also swollen, but not tender. The right elbow could neither be flexed beyond a right angle nor fully extended. The right sterno-clavicular articulation was swollen and tender. The other joints were free. The temperature was 100·5° F., and had varied between that point and 99° F. throughout. The patient was in a very cachectic condition, anæmic, and wasted. There was no evidence of visceral complications. The soles of both feet presented a remarkable appearance. Along the inner border of each was an irregular horny mass with a nodular surface. Smaller masses were present along the outer side of each sole, and the intervening areas were covered with a brownish-yellow parchment-like thickening of the epidermis. The nodules were of a dark-brown or purple-brown colour. The individual swellings measured 0·3 cm. to 2 cm. across from base to base. The masses, as a whole, resembled very closely a mountain range on a relief map, a description which has been given by French writers. Although most developed on

the soles, the excrescences have crept towards the dorsum on both the inner and outer aspects of the feet. The area affected was sharply limited by a narrow zone of hyperæmia. With the exception of small isolated nodules at the root of the great toes, the toes were free. The lesions felt like horn, and, on puncture, no fluid could be withdrawn from them. The palms were quite unaffected.

The patient had been under treatment by gonococcus vaccine and the arthritic symptoms were abating. Some of the small nodules, however, had been observed to increase slightly in size. Since the patient was shown at the meeting a portion of the carapace on the sole of the right foot had separated, leaving a reddish-brown patch.



Kératodermie blennorrhagique.

An early nodule was removed for histological examination, and the following report had been received from Dr. Turnbull: "The nodule is covered by a very thick, horny cap. The interpapillary processes beneath this are much lengthened. The papillæ are very œdematous and much infiltrated. In the subjacent dermia there is no œdema, but a little infiltration especially around the vessels. The infiltration in the dermia consists of lymphocytes with a large number of mono- and bi-nuclear plasma-cells (Unna, Pappenheim). There are a few neutrophile polymorphonuclear leucocytes in the upper part of the papillæ and many in

the capillaries. Only one or two eosinophiles were found. No mast-cells were seen. In the epidermis the lower part of the Malphigian layer shows clear prickle borders; karyokinetic figures are present in the basal layer. Varying numbers of neutrophile leucocytes are seen in these layers. A stratum granulosum is only recognizable in places. At this level there are very large numbers of neutrophile polymorphonuclear leucocytes, in masses occupying rounded spaces formed by the degeneration of epithelial cells. The superjacent horny layers contain many flattened nuclei, and collections of neutrophile leucocytes lying in strands between the horny layers. A superficial layer of horny substance without nuclei is present in part of some of the sections."

[*Note by Dr. Sequeira on the Literature of this Affection.*—Kératodermie blennorrhagique was first described by Vidal[1] and several cases are recorded by other French observers. It only occurs in the course of gonorrhœa with severe articular and general manifestations. The urethritis has been a minor feature in all. Occasionally visceral complications have been observed, and grave debility and cachexia are the rule. The cutaneous manifestation, which presumably must be considered as evidence of toxæmia, occurs in the first few weeks of the disease at the same time as the joint affections. The feet and hands, and rarely the trunk, are affected. As a rule, the condition appears to last two or three months, when the plaques fall off, leaving reddish-brown macules. In Jacquet's case[2] the patient had six attacks of gonorrhœa. In the first there was urethritis only, in the second and third urethritis and arthritis, in the fourth, fifth, and sixth attacks urethritis, arthritis, and keratodermia. An interesting case was described by Chauffard in 1897.[3] The exhibitor has been unable to find any record of a previous case shown at any of the London societies.]

DISCUSSION.

The PRESIDENT said that, so far as he was aware, this was the first case shown in this country, at any rate the first case brought before the Dermatological Section or Society.

Dr. WHITFIELD advised the use of the X-rays for the lesions on the feet. It was a wonderful therapy in gonorrhœal warts.

Dr. SEQUEIRA agreed that X-ray treatment should be tried, bearing in mind the histological resemblance of the lesions to gonorrhœal warts.

[1] *Ann. de derm. et de syph.*, Par., 1893, 3me sér., iv, p. 3.
[2] *Bull et mém. Soc. méd. des Hôp. de Paris.*, 1897, 3me sér., xiv, p. 93.
[3] *Ibid.*, 1899, 3me sér, xiv, p. 569.

Dermatological Section.

April 21, 1910.

Sir MALCOLM MORRIS, K.C.V.O., Vice-President of the Section, in the Chair.

Elephantiasis associated with Tertiary Syphilis.

By H. G. ADAMSON, M.D.

THE patient was a woman, S. W., aged 36. She had been pregnant at the age of 18, the child having been "born dead." Swelling of the legs had begun four or five years later, first of the right, then of the left leg. Both legs were now enormously enlarged from just above the knees down to, and including, the feet. Pitting was only obtained on very firm pressure. There was no warty growth, as sometimes occurred in cases of elephantiasis. There were now present on the legs flat papular, crusting and ulcerating lesions, with circinate grouping, and many old scars of former lesions. A positive Wassermann reaction had been obtained by Mr. McDonagh.

The case was shown with the view of eliciting an opinion as to the nature of the association of the elephantiasis with syphilis. This association was well known, and was mentioned in books, but not much attention was given to it. The exhibitor recalled a paper by Dr. Arthur Francis, of Hull, entitled "Cases of Elephantiasis associated with Tertiary Syphilis."[1] In this paper were recorded seven cases of elephantiasis of the lower extremities associated with syphilis. Elephantiasis of the upper lip (leontiasis syphilitica) and of the genitals was a recognized form which was generally regarded as due to a syphilitic lymphangitis, and which disappeared under mercury treatment. The question now

[1] *Brit. Journ. Derm.*, 1894, vi, p. 225.

raised was as to whether elephantiasis of the lower limbs in syphilitic subjects was of the same nature and due to a diffuse syphilitic lymphangitis, or whether it might not be the result of a streptococcic infection, such as Achalme, Sabouraud, and others had shown to be the case in many instances of elephantiasis associated with non-syphilitic ulcerations of the leg. In elephantiasis associated with ulcers of the leg there was always a history of repeated attacks of local inflammatory swelling, with more or less general febrile disturbance—"elephantoid fever"—and it was during these attacks that the streptococcus was found in serum from



Elephantiasis in a syphilitic patient, showing crusted nodular tertiary syphilide and old scars of former lesions.

the tissues. All Dr. Francis's cases of syphilitic elephantiasis had had these attacks of "elephantoid fever," and so had the patient now exhibited. This patient had not, however, been seen during a febrile attack, so that there had been no opportunity of making cultural examinations. Similar attacks of "elephantoid fever" occurred, of course, in filarial elephantiasis, and it had been suggested that these also were due to secondary microbic infection—probably streptococcic. The question was of practical importance in regard to the results that might be

expected from treatment. Diminution of the swelling might be hoped for from mercurial treatment if the elephantiasis were strictly of syphilitic origin, but not if it were due to streptococcic infection. Mr. McDonagh had made a biopsy in this case, and microscopical examination showed a dense feltwork of fibrous tissue, made up of very fine fibrils, with abundant connective-tissue cells, numerous plasma cells, and many pigment cells scattered among the fibrous bundles. There were also collections of round cells and plasma cells around the blood-vessels, and here and there dilated lymphatics. Sweat glands were present, but no sebaceous glands or hair follicles were seen in the section. The large number of plasma cells did not necessarily suggest a local reaction of syphilitic origin, because this was also seen in elephantiasis unassociated with syphilis.

DISCUSSION.

Dr. MACLEOD said that it seemed to him to be a matter of dispute that the elephantoid condition was directly due to the action of the spirochæte of syphilis, and thought it more probable that the elephantiasis was an epiphenomenon probably due to the action of a streptococcus in a tissue the vitality of which was lowered by the disease, and that it had much the same pathogenesis as the elephantoid thickenings which occasionally occurred in association with tuberculosis.

Dr. PRINGLE said that many examples of the condition exhibited were in the Saint Louis Museum, labelled syphilitic lymphangitis, although he was unaware of the anatomical findings which justified the title. He had under his care at the present time a case identical with Dr. Adamson's, but in which one *arm* only was affected, which he thought was a very rare state of affairs. It was certainly not necessary for a visible surface lesion to be present, and he thought that recurrent streptococcal infections could not adequately account for all the features of the condition, which was by no means rare.

Keratosis Palmaris et Plantaris associated with Generalized Ichthyosis.

By H. G. ADAMSON, M.D.

THE patient was a girl, N. S., aged 13. The condition had been present since birth. There was a very marked horny thickening of the skin of the palms, but not of the whole palm, for on each hand there

were two broad patches of quite normal skin. The margin of the horny area where it abutted on to the patches of normal skin was bordered by a narrow erythematous zone. On the soles the thickening was confined to the points of pressure—i.e., to the heels and the anterior part of the soles. There was a hyperkeratosis of the beds of the nails, lifting up the nails at their anterior halves. The whole body and the limbs were markedly red and the skin thickened, without, however, showing any marked scaling as in ordinary ichthyosis, but everywhere an exaggeration of the normal markings of the skin. Another point of difference from the common form of ichthyosis was that the flexures were affected. The anterior part of the neck, the axillæ, and the bends of the elbows were especially the seat of closely set papilliform elevations, which again appeared to be formed by an exaggeration of the normal furrows and ridges of the skin. The patient was well nourished, a feature which was also unlike ordinary ichthyosis.

The only other member of the family who was known to be affected was a younger brother, who presented on each palm a horny streak which suggested a linear nævus.

[Since the case was shown Dr. Graham Little has reminded the exhibitor that Brocq, in 1902, described a similar case under the name of "Erythrodermie congénitale ichthyosiforme avec hyperépidermotrophie," and, from the account of these cases in Brocq's handbook[1] it seems certain that the case belongs to that type.]

Myxœdema in a Woman aged 41.

By G. W. Dawson, F.R.C.S.I.

When first seen, on April 11, she walked with unsteady gait, her speech was slow and deliberate, her face was puffy, particularly under the eyes, her hands were enlarged and massive, in addition to which she had an eczematous eruption on different parts of the body. Her illness began three years ago. She was given thyroid extract 1½ gr. three times daily. In four days the unsteady gait and the slowness of speech had almost entirely disappeared. The case illustrated the remarkable and rapid effects of thyroid extract in that and kindred affections.

[1] *Dermat. Pratique*, Par., 1907, ii, p. 703.

Monilithrix in a Boy aged 3 years and 9 months.

By S. E. Dore, M.D.

The child was the only one of five affected, the remaining four having been seen at the Evelina Hospital. The parents, and, as far as was known, their antecedents, were also healthy and free from any disease of the skin or hair. The patient was a fair-complexioned child, born prematurely at eight months, his mother having had a fall when she was seven months pregnant. He had never been very strong and had suffered from rickets, and also from an attack of scarlet fever eighteen months ago. He had a little hair at birth, but this "rubbed off a month or two afterwards." When aged 9 months his mother noticed a rash, consisting of itching red pimples, on the back and sides of his head. This condition did not alter, and the scalp remained bald until two years ago, when hair began to grow on the top of his head, but did not grow properly on the areas affected by the eruption until two months ago. The hair on his eyebrows was normal. On his back and on the extensor surfaces of the limbs the follicles were slightly prominent, and he was subject to occasional itching in these parts. The temporal, inferior parietal, and occipital regions of the scalp were scantily covered with short, stumpy hairs, producing a semi-bald appearance at first sight suggestive of ringworm. On close inspection the hair follicles in these areas were seen to be reddened and prominent, producing the feeling of a nutmeg-grater to the touch. The hairs varied in length, some being simply atrophic and shorter than normal, but for the most part they were bent and twisted, only projecting a few millimetres above the follicle, and presented a beaded appearance just visible to the naked eye. Under the microscope they showed a more or less regular succession of constrictions alternated with fusiform swellings, the latter varying from about three times to twice the diameter of the former. The constrictions were lighter in colour, apparently owing to absence rather than to increase of pigment of the "nodes," and the cuticle was thickened at the "internodes," which often showed partial fracture. Many of the hairs terminated in a brush-like extremity, like those of alopecia areata, and showed an atrophied root in the less affected hairs which did not break on extraction. Staining gave negative results

as regards the presence of micro-organisms, and cultures on agar were either sterile or showed a few staphylococci.

The chief points of interest about the case were that there was no history of heredity and no other members of the family were affected, that the patient was a premature child, and that the affected areas were also the site of keratosis of the follicles which had preceded the monilithrix by three years.

These facts seemed to point to the keratosis possibly affecting Huxley's layer of the inner root-sheath, as shown in Beatty and Scott's case, as the cause of the moniliform condition of the hairs, the keratosis itself being possibly due to an error of development consequent upon the premature birth of the child.

Linear Nævus of Unusual Character in a Boy aged 18.

By S. E. Dore, M.D.

The patient was first seen at the Polyclinic, where he was taken by Dr. Paramore, who kindly agreed to the suggestion that he should be shown at the meeting of the Dermatological Section.

On the inner aspect of the right thigh there was a series of linear lesions of varying length arranged in parallel lines and extending from Poupart's ligament above to the junction of the middle and upper thirds of the thigh below. They consisted of raised papules or nodules of a violaceous colour, some rounded, others flat and arranged like strings of beads, suggesting the diagnosis of lichen moniliformis (Kaposi). The presence of flat-topped shiny papules in or interspersed between the streaks, and of white atrophic patches extending in a linear direction down the inner side of the lower two-thirds of the thigh, corresponding to the areas from which the lesions had disappeared, also lent colour to the diagnosis of lichen planus. On the other hand, the fact that the condition had existed since the age of nine months, and had remained without altering in recent years, and the entire absence of lesions elsewhere, strongly negatived this diagnosis, and the conclusion arrived at was that the case was one of linear nævus.

Two Cases for Diagnosis.

By E. G. Graham Little, M.D.

Case I.

The patient was a clergyman, aged 51, who for the past six months had had an eruption on the forehead, for which he had had treatment from his medical man, Dr. Eaton Lascelles, to whom the exhibitor was indebted for permission to show the case. The eruption was confined to the outer third of the right half of the forehead, between the eyebrow and the scalp, and did not affect the hairy scalp. There was a patch about the size of a two-shilling piece with some raised follicular, warty-looking growths upon a reddened area, flat, but with no signs of atrophy. Near the main patch were other smaller, warty eminences, with follicular plugs which were easily removed by scraping. The affected skin was of a deep reddish-blue; it was not itchy or painful in any way. There were no lesions of mucous membrane. There was some redness with exaggeration of follicles of the neck near the junction of the scalp with the smooth skin.

The exhibitor was of opinion that it was a case of hypertrophic lichen planus of very unusual distribution. The clinical appearances exactly coincided with the well-known type of lichen planus hypertrophicus described by Brocq under two headings, according as to whether there are or are not present as well typical papules of lichen planus. In this case there were no such papules, and the patch had some resemblance to lupus erythematosus, except that there was no atrophy. The distribution on the face of this type of lichen planus had not been previously recorded as far as the exhibitor could find on reference to the fullest description of the disease readily available, that of Brocq in the *Pratique Dermatologique*. There its occurrence is recorded, in certain "rare cases," on the thighs, hips, forearms, sacrum, scrotum, penis; the great majority of instances of this form of lichen being found on the lower limb. The exhibitor had had one case of an isolated patch of lichen planus hypertrophicus on the middle of the thigh, and one in which there were hypertrophic patches (in the course of an eruption of lichen planus) on the forearms. The diagnosis in the former case had been confirmed by a biopsy.

The diagnosis of lichen planus hypertrophicus was supported by the majority of members present.

Case II.

The patient was a little girl, aged about 8. Nothing unusual had been noted until two months ago, when the mother had observed some thickness and stiffness of the cheek. The patient had been seen by the exhibitor's colleague, Dr. Frederick Langmead, to whom his best thanks were due for his permission to show the case. The child had peculiarly red cheeks, and was well nourished, and even fat. Over large areas of the body there was evident thickening of the integument, and probably of the subcutaneous tissues, so that the skin, in place of being easily movable over the underlying fascia, was picked up with difficulty, and, indeed, seemed to form one piece with the muscle, which also seemed thickened. The skin was not changed on the surface in any way; there was no shininess, extra smoothness, or atrophy. A large part of the body seemed to be involved, the only actually free areas in which the peculiar doughy thickness of the skin and muscles was not evident being the buttocks, the forearms, the abdomen, chest and back, the hands and the feet. There was no change in the tactile sense apparently, and not the slightest appearance of the surface-phenomena associated with sclerodermia; no immobility of the joints, shiny skin, depression of the affected areas as compared with the unaffected parts, and no change of coloration.

The exhibitor had hesitated between the diagnosis of an early œdematous stage of sclerodermia of an unusually extensive distribution and localization and a myositis, the latter diagnosis being suggested by a very extraordinary case which he had seen under Dr. Archibald Garrod in the Great Ormond Street Hospital. On reading the literature, however, and after hearing the opinions expressed at the meeting, he was inclined to regard it as an instance of generalized sclerodermia.

Dr. F. Parkes Weber suggested that the case resembled that described by Dr. F. E. Batten in a boy, aged 6, under the heading "Myositis Fibrosa,"[1] and possibly was of the same kind as that of a child shown by Dr. Walter Carr at the Medical Society of London on April 11, 1910.[2] Dr. F. E. Batten had discussed the literature of the subject.

[1] *Trans. Clin. Soc. Lond.*, 1904, xxxvii, p. 12.
[2] *Lancet*, 1910, i, p. 1072.

Lichen Planus in a Child.

By J. E. R. McDonagh, F.R.C.S.

(For P. S. Abraham, M.D.)

The patient was a little girl, aged 5, who was brought to the West London Hospital on February 11, 1910, with the history that a fortnight previously "pimples appeared in the middle of the back," which caused considerable irritation. On examination there were numerous scattered, plane papules, many of which showed the distinctive character and tint of lichen planus, chiefly aggregated upon the trunk; but several lesions were also seen upon the forearms, being specially grouped together in the ante-cubital spaces. The general health did not appear to have suffered in any way. At the time of exhibition the condition had lost a good many of its features, as the eruption had materially improved under the administration of small doses of arsenic and weak tar applications.

Case of Dermatitis Herpetiformis.

By J. E. R. McDonagh, F.R.C.S.

The patient was a boy, aged 17, who had come up to the West London Hospital on March 29, 1910, complaining of a rash more or less covering the body. The eruption had first made its appearance on January 10, 1910, and had begun as a raised red ring just below the navel. The lesion had itched very much, and, according to the patient, as a result of scratching a blister had formed. Similar lesions had come elsewhere, always accompanied with itching, and subsequently blister formation. Once the blisters had formed, the itching had ceased. Since the patient had been under observation, bullæ had appeared on the glans penis, on the tongue, and on the mucous membrane inside the mouth, on the uvula, and on the soft palate, causing some pain on swallowing. The patient had never had the complaint before, and there was no family history. Dermatographism was marked, a typical wheal appearing on pressure. The bullæ which had recently appeared had not been preceded by an erythema. The rash fitted in in every way with the

diagnostic signs as formulated by Duhring — viz., polymorphism, pruritus, chronicity, excellent health of the patient, and herpetiform arrangement of the lesions.

The exhibitor was indebted to Dr. Abraham for his kindness in allowing him to show the case.

DISCUSSION.

Sir MALCOLM MORRIS said it was strange how rarely the condition attacked the conjunctiva, although it attacked the mucous membrane of the mouth.

Dr. PRINGLE said that with regard to the ultimate prognosis of such cases, it had been his happy experience that a considerable number of such cases, although by no means all, got permanently well. This accorded with Duhring's original statement. One patient recently under his observation, who had suffered from the disease in its severest form for more than ten years, had completely recovered, although at the expense of well-marked arsenical pigmentation, with palmar and plantar keratosis. Previously to coming under the speaker's observation he had been treated with 10-minim and larger doses of Fowler's solution three times daily, but it was found that 6-minim doses were invariably efficacious in arresting the frequently recurrent exacerbations, each of which was accompanied by marked indicanuria.

Cases of Syphilis illustrating the Importance of the Wassermann Reaction.

By J. E. R. McDONAGH, F.R.C.S.

THE exhibitor said that the cases he was about to show brought out very clearly the importance of the Wassermann reaction. The cases themselves did not present any interesting lesions, but they were of extreme importance from an economic point of view.

CASE I.

Mrs. A. B., aged 46, came up to St. Bartholomew's Hospital complaining of a rash on her right arm. The rash had appeared about Christmas, 1909, and some short time before she had had some sore places on the right leg. The lesion on the arm was a gumma, and the right leg was covered with the scars of gummatous ulceration. This

patient was aged 21 when she married, and neither before her marriage nor since, until the date above-mentioned, had she ever experienced the slightest evidence of syphilis. She had had four miscarriages; eleven children were born, two of whom were still living—the second and fourth pregnancies. All the other children had died within six months after birth as the result of syphilis. Her last pregnancy had been a miscarriage, immediately after which her leg became bad; this period also corresponded to the change of life. The patient had given a strong positive Wassermann reaction. Her second pregnancy was a son, who had given a negative Wassermann reaction, as had also his wife and child. The fourth pregnancy, a daughter, had given a negative Wassermann reaction. Neither child had shown the least taint of the disease. That these children had never been syphilitic was more than probable, since congenital syphilitics tended to give a strong positive reaction, which remained so throughout life. The point to be brought out in this case, and in several others similar, was the fact that the Wassermann's reaction had shown that Colles's law was incorrect. The patient had undoubtedly been infected through her child *in utero*, because, had she been infected directly from her husband, she would have had evidences of syphilis, or have given a history of such. Colles was right in meaning that the woman was immune, but was not under the impression that she was syphilitic or likely to show signs of the disease later. The exhibitor demonstrated that they do show no signs of the disease until the menopause, when tertiary lesions appear, often as not severe in nature.

The important feature in the case was that had the patient been recognized as having contracted syphilis, say from her first child, and had she been treated throughout each successive pregnancy, the probabilities were that many of those children who were born syphilitic would have been born healthy. That similar cases were numerous there could be absolutely no doubt. Could not, therefore, something be done in the way of using the Wassermann reaction in women who had aborted to see if they were syphilitic or not?

Case II.

L. B., aged 47, had come up to the West London Hospital complaining of sores on the calf of the right leg. The sores or ulcers were typical gummata. As in the preceding case, the ulcers had appeared just after "the change of life." The patient had been pregnant nine

times; the children had been mostly premature; some had been born alive, others born dead, but not one had lived for more than three weeks. Wassermann's reaction had also definitely proved syphilis in the third generation, and of all the cases known it had always been the mother who was the congenital syphilitic parent. It was all very well to say that only a small percentage of infant mortality was due to syphilis, but in the statistics a miscarriage or a child born dead were not included, and that syphilis was the chief cause of such was undoubted. Therefore, surely more attention should be paid to this question than has hitherto been the case. Mr. McDonagh summed up his remarks by saying that women infected with syphilis from their child *in utero* showed no signs of the disease until the child-bearing period was over or the menopause had set in, when tertiary manifestations made their appearance. Since it was the rule that such women gave a positive Wassermann reaction, they should throughout each successive pregnancy be treated with mercury.

The exhibitor was indebted both to Dr. Adamson and to Dr. Abraham for their kindness in allowing him to make use of their cases.

Since the meeting of the Section the exhibitor's attention had been called to an article in which Viannay[1] had drawn attention to the frequency with which women with no antecedents of syphilis were liable to get tertiary manifestations after the child-bearing period.

DISCUSSION.

Dr. PERNET said he did not admit that what Mr. McDonagh had said disproved what had been called Colles's law at all. On the contrary, the employment of the Wassermann reaction had confirmed what Colles had written. Colles himself had merely recorded the fact that he "had never seen or heard of a single instance in which a syphilitic infant (although its mouth be ulcerated), suckled by its own mother, had produced ulceration of her breasts. . . ." These are Colles's own words.[2] Colles had said nothing about immunity or formulated a law. He had merely recorded the results of his own observation. Dr. Pernet had found on several occasions that writers and others who referred to Colles's law had never taken the trouble to look up Colles's original reference to the point. The mother of a congenitally syphilitic infant did not contract syphilis from her child when she suckled it, because she herself was syphilitic. Calling this condition "latent syphilis" makes no

[1] *Ann. de Derm. et de Syph.*, Par., 1898, ix, p. 879.

[2] "The Works of Abraham Colles," New Sydenham Soc., 1881, p. 271.

difference. As to congenitally syphilitic women, they are very liable to repeated abortions. Dr. Pernet had published an observation bearing on this,[1] and Jullien, in his monograph,[2] had collected other cases.

Sir MALCOLM MORRIS asked, if it were true that a syphilitic mother could bear children who showed no sign of syphilis, what significance was considered to belong to the fact.

Mr. MCDONAGH replied that the significance was that such children might yet grow up and produce syphilitic progeny.

Dr. WHITFIELD said that if the reaction persisted *in every case* throughout life it was against the value of the reaction, as congenital syphilitics were not always immune throughout life. As a matter of fact, some of the published statistics seem to show that the Wassermann reaction died out, at any rate in some cases.

Case for Diagnosis.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed.

THE patient was a man, aged 50, whom he saw on June 16, 1909. The eruption was symmetrical on the arms; the lesions covered the backs of the arms and extended down the fronts of the forearms. It was now less red than formerly. It was retiform, and the first idea which would occur to the observer was that it was lichen planus. But in other parts there were lesions not like those of lichen planus. The general eruption had remained very much what it now was since last June. During the winter the symptoms might be said to be semi-Raynaud. There was now an extension of the eruption on the legs. It almost disappeared on pressure, except for the staining which was left behind, and which did not reach below the knee. He had suffered badly from chilblains, and did so now. The mouth was not involved. He could recall two or three cases of retiform eruption shown before the old Society. The patient was dyspeptic, and the disease very resistant to treatment. The condition had lasted nearly ten years. He believed it to be a case of localized parakeratosis variegata. The condition was very rare. He believed that travelling to town daily in the very cold weather had something to do with the cause. He hoped to report the case in full later.

[1] *Brit. Journ. Derm.*, 1899, xi, p. 459.

[2] *Congrès de Derm.*, Par., 1900, p. 350.

Dr. MACLEOD said that he believed the case to belong to the type described by Unna and others as parakeratosis variegata, and placed by Brocq in a group of rare affections of the skin to which he gave the name " parapsoriasis." The name " parapsoriasis " was an unfortunate one as applied to cases such as the one exhibited, as they did not seem to have any clinical or pathological relation to psoriasis and were much more resistant to treatment.

Lupus Erythematosus associated with Erythema Iris.

By J. H. SEQUEIRA, M.D.

THE patient, a married lady, aged 41, had been under the care of Dr. Pugh, of Brighton. She enjoyed good health until the spring of 1908. The previous winter, which was a severe one, was spent in Paris, and the patient suffered very much from the excessive cold. In March, 1908, she developed lupus erythematosus, which affected the nose, ears, and scalp. This ran a chronic course, with exacerbations in the cold season of the years. The patient has always felt the cold very much, and had suffered from chilblains on the feet and hands. There was history of phthisis in her father's family, and one of her brothers has died from consumption. Her two children were quite healthy. There was no evidence of tuberculous disease in the patient herself, and no history of enlarged glands, &c., in childhood.

In February of this year a different type of eruption had appeared on the backs of both wrists and forearms. The lesions began as circular disks about the size of a shilling or a two-shilling piece. The erythema was soon followed by vesication. When first seen by Dr. Sequeira the spots had the characteristic appearance of erythema iris. There was some soreness about the mouth, where a few vesicular lesions appeared on the red margin of the lip. There were no lesions in the buccal cavity.

The case was shown in view of the interest attaching to the association of erythema multiforme with lupus erythematosus, both being of probably toxic origin. It is interesting in this connexion to note that G. Define has reported a positive von Pirquet reaction in a case of erythema iris.[1]

[1] *Giorn. Internat. d. Sci. Med.*, Napoli, 1909, xxxi, p. 1061; abstract *Brit. Med. Journ.*, 1910, i (Epitome No. 218, p. 62).

DISCUSSION.

Sir MALCOLM MORRIS raised the question whether a patient with lupus erythematosus was more prone to get erythema multiforme than another person. He had looked back upon a large number of lupus cases, but the number of cases of erythema multiforme was very small, therefore he did not think the two were necessarily associated. It seemed to be rather more common now, owing to influenza and gastric attacks.

Dr. PRINGLE said he thought the distinction generally drawn between erythematous lesions on the hands which left scars and those which did not was, as constituting an essential differentiating feature between lupus erythematosus and erythema multiforme, a purely academical one and unsound clinically.

Case of Darier's Disease.

By A. WINKELRIED WILLIAMS, M.B.

THE patient was a railway porter, aged 19. The duration of disease was three years. He had been first seen six months ago at the Sussex County Hospital. He had then presented a papular eruption distributed on the trunk as follows: on the back a band down the middle line from the buttocks to the neck, and extending about half-way across the back on each side; in front a somewhat triangular distribution with the base of the triangle between the iliac spines, and the apex at the centre of the upper border of the manubrium sterni. The papules were discrete, averaging about the size of a hemp-seed; most of them were flat, but a few were acuminate and almost spinous. They were of dusky grey colour, and a few showed a slight pink glow through the dusky grey tint. There were also some crusted rupia-like lesions; these were mainly on the back between the shoulders in the middle line. There were also a few along the rib lines. Removal of the crusts in a few cases caused slight bleeding, but the majority showed under the crusts a red surface covered with a thin, shiny layer of epithelium; these crusted lesions rapidly got well under a weak, white precipitate ointment. There was an ulcer on tonsil and some adenitis which had existed a few days. The urine and temperature showed no abnormality. The throat rapidly got well under salicin treatment.

For six months the papular lesions had been vigorously treated with salicylic preparations, &c., and the only result had been to reduce the

acuminate to plane lesions, and the eruption had extended somewhat. Internal treatment with salicin, iodide and mercury, quinine, arsenic, had not altered the rash in any way.

Histology: There was very great thickening of the horny layer, which in its lower part retained acid dyes—e.g., eosin and acid fuchsin, more deeply than in the regions beyond the thickened part. Here and there rounded bodies were embedded in it, but these did not show any special affinity for the acid dyes. The plug-like masses of this horny tissue dipped down deeply into the rete. Below the horny masses the granular layer was thicker than in other parts, and there was also slight thickening of the rete and the cellular elements of the papillary layer of corium were slightly increased. The rounded bodies of Darier were found in moderate numbers just below the horny cells. Sections from the centre of the plug showed some quite typically; their double contour was well marked; near the borders they were less distinct. They stained feebly by acid stains. On the whole they were not so numerous as in some cases.

Sir MALCOLM MORRIS, who saw Darier's original case in 1889, and Dr. PRINGLE concurred in Dr. Williams's diagnosis, while agreeing that further observation was necessary to render it a matter of certainty.

Dermatological Section.

May 31, 1910.

Dr. T. Colcott Fox, President of the Section, in the Chair.

Lupus Vulgaris Erythematodes.

By S. E. Dore, M.D.

The patient was a man, aged 73. The disease began on the back of his neck twenty years ago, but his face was not affected until the year 1899, after an attack of "erysipelas." The scalp was attacked soon after the face, and independently of it. The patient was shown at the February meeting of the Dermatological Society of London in 1904, when the area of distribution of the disease was practically the same as at the present time, affecting nearly the whole of the face and neck and the top of the head, with the exception of a narrow band of healthy skin extending across the forehead above the eyebrows. This peculiar distribution, together with the superficial character of the infiltration, made the resemblance to lupus erythematosus a close one, but the presence of numerous patches showing a definite lupus deposit and scarring, on the limbs, and the slight ectropion—which has since become more apparent—of the lower eyelids, led to the diagnosis of lupus vulgaris, and this was confirmed by microscopic examination. In addition to the skin affection, the patient had had a parotid tumour, which was removed four years ago. Two years ago a small warty growth appeared on the back of the neck; this was excised and sections made, but there was no malignant change. It recurred a year later and was again excised, a small, ulcerative lesion now being present at the site of the scar. He also had a curious condition of the right wrist-joint, the nature of which was not quite clear, but there was much

deformity and swelling externally, and the joint was shown to be disorganized by screen examination, the changes being compatible with tuberculosis.

The patient had attended Dr. Pringle's out-patient department at the Middlesex Hospital since 1904, and had considerably improved under treatment with X-rays and thyroid feeding. Much of the disease had disappeared, especially from the scalp, leaving the skin tense, smooth and shiny, and free from disease. On the face, and also in some parts of the scalp, collections of lupus nodules could still be seen.

Case of Lupus Erythematosus almost exclusively limited to the Scalp.

By Sir Malcolm Morris, K.C.V.O., F.R.C.S.Ed.

The patient was a woman, aged 29. The first thing she had noticed was a small bald patch on the top of her head eight years ago. This had been treated for ringworm by blistering and afterwards for eczema with various applications. The patch had gradually increased in size until nearly the whole scalp had become bald, only the hairs in the frontal region and a fringe round the sides of the head remaining. The nose had become affected about a year after the scalp, but the disease in this situation had not spread and now presented a small red atrophic patch affecting only the tip. In addition to the smooth white shiny appearance of the scalp there were numerous areas of active disease, chiefly at the periphery of the bald area, where there was in parts a well-defined festooned margin. Towards the anterior part of the scalp the follicles were especially picked out, each one being represented by a little brown dot, giving the surface a curious stippled appearance.

An interesting feature of the case was that the posterior cervical chain of glands was enlarged on both sides, and there were scars on the left side of the neck, resulting from an abscess which had been incised when she was 9 years of age. There was no history of consumption in the family; the mother was living and healthy; the father died eleven years ago and had been a healthy man. Five sisters were living and well. A twin sister had white patches on the skin of her neck and chest, but none of the others had any affection of the skin. The patient looked weakly and anæmic, but said her health was good. She felt the

cold very much, and in winter her fingers became red and swollen, especially at the tips. Her ears had never been affected.

The complication of enlarged glands with lupus erythematosus was not uncommon, but it was unusual for the posterior cervical glands to be thus affected.

Case of Lupus Erythematosus.

By Sir MALCOLM MORRIS, K.C.V.O., F.R.C.S.Ed., and WILFRID FOX, M.D.

PATIENT was a seaman, aged 57. The eruption had started twenty years ago on the bridge of the nose and ears, and had spread slowly on to the cheeks. It followed a slight frost-bite, which was contracted off the east coast of North America in the course of his profession. The frost-bite never really healed, but passed gradually into this condition. The eruption, with long intervals in which it remained stationary, had steadily progressed during the whole time, and, although it varied in intensity, it had never vanished. The rash was of the "fixed" type and fairly evenly distributed over both cheeks; there was a patch involving the right eyebrow, which dated from four years ago. The ears were affected in the usual manner, but had been damaged by an accident, so that it was difficult to say how much of the destruction was due to the disease. The scalp was not affected, nor were any of the mucous membranes.

His past history gave no suggestion of tubercular disease in any form, nor had any relatives suffered from consumption. He admitted a chancre of doubtful nature thirty years ago, for which he was treated with mercury for two months, but which was accompanied by a bubo, and was not followed by either rash or sore throat. He occasionally suffered from slight rheumatic pains, but had never had acute rheumatism. There was no albumin present in his urine. The patient stated that change of climate, either extreme heat or cold, seemed to affect the eruption more than any other circumstance.

Sir MALCOLM MORRIS said he suggested that Dr. Wilfrid Fox should bring the case because of the atrophic condition of the ears. Sir Malcolm was in Germany in the winter of 1870-1, which was an intensely cold one. In the

huts where he was with patients the thermometer was as low as 30° F. below zero. He had seen frost-bites with the whole of the ears gone and half the nose after exposure of only an hour. The stump left afterwards was much like the stump slowly produced by erythematosus lupus. He had come across many young men who got a curious atrophic condition of the ears, which they attributed to football. He had cross-examined several of those men, and found that they got their ears torn and a fissure produced at the back. That healed up, but in another game it got torn again. Discussing lupus erythematosus generally, he said it seemed that in the extensive chronic cases it was a toxæmia of some kind. All the patients were more or less out of health, having poor circulation, and being generally feeble. He always treated his patients with lupus erythematosus on that hypothesis. He was certain that the change of treatment from his early days, when the methods were more violent, was an improvement. He saw a case a short time ago, one that he had not seen for twenty years, which presented a white scar all over the face. On being asked what she had done in the meantime, she replied that she lived in Cornwall and could not afford to come to London, and therefore had had no treatment except using mild lotions. That it should have died out in that way was encouraging.

Lupus Erythematosus with Epithelioma.

By J. H. Sequeira, M.D.

The patient, a single woman, aged 27, had suffered from lupus erythematosus for the past nine years. There was a history of consumption on both sides of her family, and her father had died from the disease. There had been no evidence of tuberculosis in the patient. Her opsonic index, taken several times, had always been about normal. There had been no reaction to tuberculin on two occasions. The patient enjoyed good health until 1901, when she became easily tired and drowsy. The eruption first appeared on the lobules of the ears, but spread rapidly over the face. She had been under treatment at two special hospitals before she came to the London Hospital in 1902. There were then extensive areas of the skin affected, involving the whole of the face, the backs of the hands, and lower parts of the forearms, and the fronts of the legs and the feet. There was also an X-ray burn upon the upper part of the chest. The patient had attended the London Hospital at somewhat irregular intervals since. She had been frequently in the wards, and had also spent

long periods at the Margate Sea Bathing Infirmary. There had been no real improvement. Salicin internally appeared to suit her best; quinine, arsenic, and general tonic treatment was tried without benefit. At one period she had prolonged treatment by sour milk, as there was dyspepsia. The local treatment had been, at different times, the Finsen light, regular painting with tinct. iodi, the application of salicylic plasters, a few treatments with solid carbonic acid, and most of these methods temporarily improved the areas treated. In the winter she suffered a great deal, and could rarely come out of the house, the feet and the hands and the lower part of the face breaking down into superficial excoriations, rarely actual ulcers, but exceedingly painful and requiring constant dressing.

In January, 1910, an epithelioma developed between two visits, three weeks apart. The tumour was situated above the ramus of the jaw, at the level of the angle of the mouth, and about an inch distant from the right commissure. The tumour was excised, and sections proved it to be epitheliomatous. Recurrence *in situ* occurred in four weeks, and spread rapidly. A surgeon was asked to see her, but he thought it impossible to make a complete removal. So far there had been no infection of the lymphatic glands.

Lupus Erythematosus of Eleven Years' Duration Localized to the Scalp.

By J. H. Sequeira, M.D.

The patient was a married woman, aged 35. She had two healthy children. There was no history of tuberculosis in the family, and no evidence of such in the patient. One day before the appearance of the skin affection—i.e., twelve years ago—she had appendicitis, for which an operation was performed. There were no details as to the nature of the appendix lesion. The cutaneous eruption began as a small spot on the scalp. This gradually increased, and a second area was attacked, and, finally, a small patch appeared behind the right ear above the root of the mastoid process. The case had been watched by Dr. Sequeira for several years, but at no time had the face or any other part been affected.

Lupus Erythematosus with Sclerodactyly.

By J. H. Sequeira, M.D.

The patient, a married woman, aged 43, had had tuberculous abscesses in the glands of the neck, and there were extensive scars left by the operations necessary to their treatment. The patient's mother died of phthisis. The lupus erythematosus developed fourteen years ago and had never cleared up. The hands had always been severely affected, and there were extensive chilblain-like areas on the dorsum. The little finger of the left hand was thin and tapering, like an elongated radish, and its surface was pink, smooth, and shining; it could neither be flexed nor extended. The terminal parts of the third and fourth fingers of the same hand were slightly affected. A less marked condition was present on the fingers of the other hand. There had been no recurrent asphyxia and no hæmoglobinuria.

Dermatological Section.

June 16, 1910.

Dr. T. Colcott Fox, President of the Section, in the Chair.

Linear Nævus with combined Verrucose and Vascular Elements.

By H. G. Adamson, M.D.

The patient was a boy, aged 7. The lesion was a linear nævus of the ichthyosis hystrix type. It extended in a broken line down the outside of the left thigh, across the front of the knee, and along the inner side of the calf. It was made up of elongated patches of from 1 in. to 2 in. in width and consisting of a dark-brown projecting papillated warty growth. Here and there around the base of the warty patches, and along the spaces between them, the skin was of a deep purple colour due to the presence of nævus tissue of the type of the "port-wine mark." Along the line of the nævus there could also be felt deeply seated, doughy swellings, evidently subcutaneous vascular nævi, in some parts beneath the warty growth, but also between the warty patches. Both the deep-seated vascular nævus and the verrucose growth were particularly marked over the front of the knee, so that an appearance of swelling and deformity of the knee-joint was produced. The superficial "port-wine stain" was more apparent on the outer side of the thigh and on the inner side of the calf. The condition had existed in some form since birth—apparently only the vascular part, the warty part, as far as could be judged from the mother's account, having appeared more recently.

The case was brought to the meeting on account of the interest which attached to the occurrence of the vascular type of nævus with linear arrangement and in association with the verrucose nævus, a combination with which the exhibitor had not previously met.

Since the meeting Dr. MacLeod had called the exhibitor's attention to a drawing of a Baretta model,[1] which showed exactly the same condition, and which was called "vascular nævus verrucosus of the leg." In this case sections had been made, and these had shown the usual thickening with folding and invagination of the epidermis, with, in addition, the structure of a deep-seated and, in parts, superficial vascular nævus.

Case of Granuloma Annulare.

By T. Colcott Fox, M.B.

The patient was a girl, aged 4, with a nodular eruption spreading in rings on the fingers, very similar to one he had demonstrated to the Dermatological Society of London fifteen years ago. On the dorsal aspect of the proximal phalanx of the right forefinger was a projecting nodule, about the size of a hemp-seed, firm, of a reddish tint, indolent, not itching, and painless on pressure. On the dorsal aspect of the proximal phalanges of each little finger was a circinate nodulation with similar characters, and presumably formed by the eccentric spread of nodules. These ringed lesions were not composed of an aggregation of whitish nodules undergoing involution in the central parts to form rings. There was no history in this case of any constitutional disturbance shedding light on the ætiology. Radcliffe-Crocker doubted whether the two phases of ring-formation here referred to belonged to the same disease.

Case of Lichen Planus in a Child.

By Haldin Davis, M.B.

The patient was a boy, aged 9, who was attending the Children's Hospital at Paddington Green. The eruption, which had already persisted for three months when it first came under observation, consisted of typical lichen planus papules which were widely distributed over the trunk, while a few were to be seen on the outer aspects of the

[1] *St. Louis Atlas*, edited by Dr. Pringle, plate xliv, p. 259, Baretta Model, No. 1772, patient of Dr. Gaucher.

knees and elbows. The pruritus was sufficiently severe to prevent the patient from sleeping. The principal point of interest in the case lay in the youth of the patient.

Case for Diagnosis.

By HALDIN DAVIS, M.B.

THIS was the case of a little girl, aged 4, attending Paddington Green Children's Hospital, who had had measles two months before she came up to the hospital. She had developed an eruption consisting of small brownish papules, in the middle of which, in some instances, the opening of a follicle could be seen, scantily distributed over the limbs, for the most part upon the flexor surfaces, and with a few upon the trunk. One or two were found upon the palms of the hands and upon the plantar surfaces of the feet. When they had once appeared they seemed to persist in the same place. There were absolutely no subjective symptoms as far as the skin condition was concerned, but the mother said that the child was not "well in herself," that she would not eat, and that at times she seemed without power in her legs. This paretic state, however, only lasted for a time, varying from about ten minutes to half an hour.

As regards the diagnosis, the condition did not seem to fit into any previously described category; at the meeting the prevailing opinion tended towards one of the forms of lichen urticatus, but the exhibitor remained unconvinced that this was the correct diagnosis. The patient has now been admitted into the hospital for observation, but it is hoped that the course of the case may throw some light upon the condition.

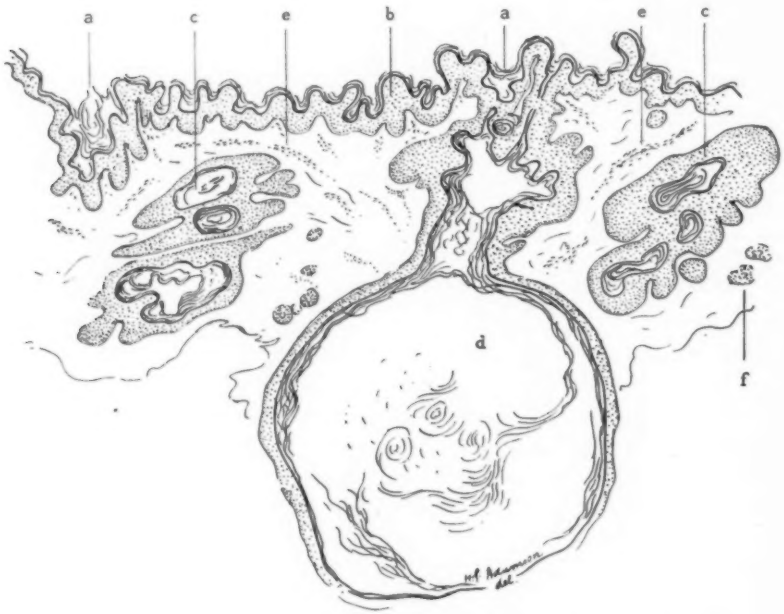
Sections from a Case of Linear Nævus.

By HALDIN DAVIS, M.B.

THESE sections were taken from a case of linear nævus which was shown to the Section at the March meeting.[1] It was considered to be an example of the variety known as the "nævus unilateralis acnei-

[1] *See* p. 69. Photograph of case.

formis," of which the best instance was the case shown by Dr. Selhorst from The Hague at the International Congress of Dermatology in 1896. The sections showed, however, that the black dots which formed the conspicuous feature of the condition, and which were thought to be comparable with comedones, really were composed of horny material, and formed plugs which closed the mouths of spherical crypts, which were lined with an epithelium like that of the external skin. The nævus is therefore composed of rows of these crypts, of varying size. It is of interest to note that the scar of the incision made for the purpose of removing material for the biopsy has developed keloid.



Section of linear nævus (with acneiform lesions). (a) Thickened horny layer, especially marked in the depressions in the epidermis (hyperkeratosis); (b) Hypertrophy of the prickle-cell layer (acanthosis); (c) Irregular masses of epidermis with central cavity lined with horny cells (these masses appear to be produced by down-growths or foldings-in of the epidermis: note the absence of hairs and of sebaceous gland structure); (d) A cystic dilatation whose walls are epidermis and whose contents are horny cells, evidently an exaggeration of the epidermic mass seen at (c); (e) Cell infiltration in long, narrow masses (these cells resemble those seen in a soft mole); (f) Sweat gland.

Acne Vulgaris with Keloids.

By E. G. Graham Little, M.D.

The patient was a young lady, aged 20, who had had the eruption of acne vulgaris, distributed especially on the back of the body, for at least three years. She had been under Dr. Radcliffe-Crocker's care at the earlier stages of the disease. No incisions had been made in any of the lesions, which had been treated with local applications; but very early in the eruption of acne many of the sites of the disease had developed keloidal growths, of which there were now some eight or nine, the largest the size of 1 in. by ½ in., the smallest the size of a large pea. The growths were firm and hard and apparently tended to increase slowly in size, and fresh growths were making their appearance from time to time. They were not itchy or painful. There was no tuberculous history in the family. There was much eruption of acne on the face, but hitherto no keloidal growths had developed in this position. Dr. Little wished to thank Dr. Morris, of Bognor, for his courtesy in allowing him to show this case.

Lichen Planus of Anomalous Aspect and Course.

By E. G. Graham Little, M.D.

The patient was a woman, aged 46. She was otherwise in good health. Eighteen months ago the eruption for which she sought relief had appeared; this consisted of a closely grouped eruption of shiny nodules covering an area on the inside of the knee and adjacent part of the right thigh of about 4 in. or 5 in. There was much itching in this position, but there was no eruption elsewhere on the body, or on the mucous membranes of the mouth or the vulva. There were no typical lichen planus papules, the lesions being much larger and more salient than usual, and the exhibitor had, in fact, thought of the possibility of the nodules being myomata, and had excised one for histological examination. The site of the eruption and the itching certainly suggested the diagnosis of lichen planus, which was probably correct. She had had ♏xv of arsenic daily for two months without the smallest influence on the eruption. A Wassermann-reaction test had proved negative.

Case of Urticaria Pigmentosa.

By G. Norman Meachen, M.D.

The patient was a boy, aged 2 years and 3 months. The first spot was noticed by the mother upon the left shoulder at the age of 3½ months. He was entirely breast-fed up to the usual period, and he was vaccinated when eight weeks old. At the time of the meeting there were numerous macular, yellowish-brown stains scattered about the trunk, limbs, and face. These were quite typical of the condition, and friction was able to restore their wheal-like character. Few, if any, fresh lesions now appeared; the old places, according to the mother, "kept on coming up again." He was a well-nourished boy, with a left congenital coloboma iridis.

Case of Xanthoma Tuberosum Multiplex.

By J. M. H. MacLeod, M.D.

The patient was a spare man, of medium height, aged 40. The disease was of wide distribution and associated with peculiar changes of an osteo-arthritic type in certain joints. He had spent most of his life as a labourer, and his florid complexion indicated much exposure to inclement weather. His family history, so far as could be ascertained, had no ætiological bearing on his present condition. At the age of 13 he had had a mild attack of small-pox; between the age of 13 and 20, when his present affection first showed itself, he had enjoyed excellent health. About 20, when he was working on a railway in Wales, he first noticed the presence of yellowish patches on the sides of his neck, which were unassociated with subjective symptoms, and about the same time he began to suffer from discomfort and slight pain in his wrist and knees. Since the disease first showed itself he had enjoyed fairly good general health, and had had no sickness, jaundice, pyrexia, or other constitutional symptoms.

The condition at present was a most striking one. There was an extensive and markedly symmetrical eruption of xanthomatous lesions, in the form of flat papules or nodules varying in size from

a lentil to a bean, and irregular patches or sheets situated on the flexors of both elbows, the axillæ and around it for 3 in. or 4 in., the neck, around the eyes and the mouth, and on the hips and over the sacrum. These lesions were yellowish or dull orange in tint, smooth on the surface, and of a tough and almost cartilaginous consistence. In addition to the patches were innumerable small, well-defined macules about the size of a lentil, and pinkish in colour; these appeared to be the initial lesions, and became gradually raised, assumed a yellowish, opaque appearance, and became transformed into the typical lesions of xanthoma. In front of the axillæ, from the movements of the joints, the patches had become broken up by deeply penetrating fissures and presented a septic papillomatous appearance, caused by the secondary inoculation of pus-organisms. The joint changes, which had progressed simultaneously with the skin-lesions, were equally remarkable. They had developed gradually and insidiously, and it was only comparatively recently that those in the wrist-joints had prevented him from doing a full day's labour. The joints most markedly involved were the elbows, knees and wrists. Clinically, they were deformed and swollen. In front of the right wrist there was a hard mass about the size of a hen's egg, which so interfered with flexion of the joint that he was no longer able to use a pick or hammer. On the ulnar side of the arm there was an oval mass about the size of a pigeon's egg which felt hard like bone. Somewhat similar growths were present in the other affected joints. Radiograms of the joints showed irregularities and erosions in the bones, with faint shadows opposite the positions of the hard nodules, indicating that they were either cartilaginous or callus in co-existence and were not completely ossified. There was no definite ankylosis of the joints, and the whole appearance was such as to suggest that the changes had begun in the cartilages and synovial membranes as well as possibly in the bone itself. A physical examination of the patient failed to reveal any general derangement which might in any way account for the condition. The liver was not enlarged, and there was no evidence or history of jaundice. The urine was examined on several occasions and contained neither albumin, sugar, nor excess of urobilin. A biopsy had been made, but the microscopical examination was not yet completed; the results of it would be reported subsequently.

Case of Canities.

By A. Winkelried Williams, M.B.

The patient was a woman, aged 29, whose hair was falling and prematurely white. It began twelve years ago as a seborrhœa oleosa with interludes of dry dandruff, lasting for three years, when the hair began to fall rapidly. The scalp was then treated for nearly three years with daily washings with soft soap and spirit dressings. Five years ago the hair turned rapidly white in the frontal area. An area of about 3 in. from the forehead consisted of white ringed hairs. There had been no neuralgia or any symptom suggesting a cause of this localization. A few grey hairs were scattered over rest of scalp. The white hairs were very loose, and showed atrophic bulbs resembling those of alopecia areata.

The point of interest was the peculiar distribution of the greyness, and the question as to whether the seborrhœa or the prolonged excessive use of alkali soap washing had any ætiological relationship with the greyness. The patient had a sister similarly affected. The patient has a rather full thyroid gland. Hairs stained by Gram's method showed practically no organisms.

Members expressed the opinion that the white patch was not connected with the seborrhœa nor with the frequent washing.

Dermatological Section.

July 21, 1910.

Dr. T. Colcott Fox, President of the Section, in the Chair.

Ringworm affecting the Upper Lip as well as the Beard Region and Hand in a Man aged 28.

By S. E. Dore, M.D.

The patient attributed the infection of the beard to being shaved at a strange barber's, in spite of the fact that the disease began on the hand and he kept several kinds of birds from which he might have become infected. He noticed the patch on the left hand three or four months before the face was attacked. When he first attended the Middlesex Hospital ringworm fungus was found in the scales from the hand and in the beard hairs. A month later a small patch appeared on the upper lip, below the left nostril, and fungus of the endothrix variety was demonstrated microscopically; cultures had not been made. The case was brought forward on account of the undoubted rarity of ringworm of the upper lip, although coccogenic sycosis was commonly found in this region.

Case of Leprosy.

By J. M. H. MacLeod, M.D.

The patient was a man, aged 38, who was born in England, of English parents, but who had just returned from China, where he had been for ten years in the police force. The case was of the maculo-anæsthetic type, the only lesion noted at the time of exhibition being a maculo-anæsthetic pink patch situated over the left jaw, but, subsequent to the meeting, definite swellings were detected in two of the nerves of the cervical plexus. The patient was a robust, well-built man of medium height, but who looked somewhat pale and anæmic from residence in the East. When he went out to China in 1890 he was in excellent health, and previous to that he had had no debilitating illness nor syphilis. His family history was negative and there was no history of any skin disease in the family. Soon after going abroad he lived at a

station a few hundred yards away from a village in which there were numerous lepers. The lesion on his face first appeared while he was in Hong Kong three years and nine months ago. It gradually spread and assumed a somewhat ringed appearance suggesting ringworm, for which he was treated for two years with no definite effect. It was examined microscopically for fungus on several occasions, but with negative results.

On examination the patch was situated near the angle of the jaw, half of it being on the cheek, while the other half spread down on to the neck. It was oval in shape, about 2 in. long by $1\frac{3}{4}$ in. broad. It was slightly irregular in outline, but well demarcated. In colour it was pinkish red and darker than the rest of the skin, and was surrounded by a slightly raised border about $\frac{1}{4}$ in. in breadth and of a slightly darker tinge. The central portion of the lesion was smooth and somewhat shiny, and the hair follicles had to a considerable extent disappeared from it. Neither in the patch nor around it was there any definite pigmentation. It felt numb to the patient, but there was no definite tingling or itching associated with it. On testing the sensations it was found that in the centre it was insensitive to heat, cold, and pain, and partially sensitive to touch, the paræsthesia being more marked in the centre than at the periphery. In addition to this the patient complained of tingling sensations in the temporal region of the same side, but there was no evidence of changes in the skin in that situation. The great auricular nerve-trunk was thickened to the size of an ordinary pencil from the posterior border of sterno-mastoid up to near the mastoid process, and the auricular branch was also thickened. The transverse cervical nerve was also thickened from the posterior border of the external mastoid for about 1 in. in a fusiform swelling. It was the latter nerve which supplied the skin of the affected patch.

Any doubt which might have existed with regard to the diagnosis from the patch alone was rendered a certainty from the condition of the nerves. A Wassermann reaction had given a doubtful result.

Case for Diagnosis.

By J. M. H. MacLeod, M.D.

The patient was a fairly healthy looking man, aged 34, who was a butler, and his skin affection consisted of a peculiar papulo-pustular eruption on the forehead, chiefly affecting the superciliary regions and spreading down on to the bridge of the nose. The patient first came under observation of the exhibitor in June, 1899, when he was sent to him from Paris by M. Sabouraud, whom he had consulted for the condition. The affection began in June, 1908, when he was in England,

on the left side of the forehead. Previous to that he had enjoyed excellent health, had never suffered from any skin eruption, and denied having had syphilis. His family history gave evidences of tuberculosis, several members on the father's side being consumptive. The father was dead, but the cause of his death was unknown to the patient. The patient had one sister who suffered from lupus.

The affection began on the forehead as a number of pinkish spots or rounded papules, about the size of a small split-pea, which were indolent in their evolution, tended to break down and form a scab, and, finally, left a depressed scar. He was treated in the first instance by a general practitioner, who burned the lesions with caustic. Under this treatment the eruption disappeared for five months, when a new group of lesions appeared on the left side of the forehead below the original group. These had not definite treatment and persisted without much change till March, 1909, when he went with his master to Paris and there consulted Sabouraud, who believed the condition to be a tuberculide.

When he first came under the observation of the exhibitor he presented about a dozen isolated papules on the left side of the forehead, which were yellowish-pink in colour, averaged in size from a hemp-seed to a split-pea, and had a somewhat translucent look. At the apex of each was a small, yellowish crust. The lesions were discrete except in one or two cases where two or three had coalesced. On examining them with a diascope the colour almost completely disappeared and no definite staining was left. At first sight the exhibitor was inclined to doubt the diagnosis of tuberculide, and thought that the affection might be syphilitic, and in consequence recommended antisyphilitic treatment with mercurial pills and iodide of potassium in a mixture. As no definite improvement appeared to result from the iodide treatment it was discontinued, but the mercury was kept until May 24, 1910, in several courses to the point of salivation. In addition to this he had had various forms of local treatment, such as exposure to a Finsen lamp, and various antiseptic ointments and lotions. Under this treatment the lesions slowly disappeared, and in February the eruption had almost gone, leaving a number of small pits about the forehead, but no pigmentation. About this time, however, a recrudescence took place in spite of the mercurial treatment, and the eruption since then had been gradually spreading. At the time of exhibition it was worse than it had been since it was first seen by the exhibitor, several groups of lesions being present over both eyebrows and at the root of the nose, spreading down as far as the bridge. These showed a tendency to coalesce into pinkish-yellow patches, which were crusted over, and near the left inner canthus had formed ulcerated fissures.

The diagnosis of the case had presented the greatest difficulty to the exhibitor. For six or eight months, practically until February, he considered it to be a tertiary syphilide, but the behaviour of the lesions since then had shaken this opinion. Unfortunately, he was unable to obtain a biopsy. A Calmette reaction had given a positive result on the right eye. (Since the case was exhibited a Wassermann reaction by Mr. Leathem at Charing Cross Hospital has also given a positive result.) The alternative diagnosis from syphilis was that it was an anomalous case of acne varioliformis, and in some respects it suggested the condition to which the name "acne agminata" had been given.

Most of the members present regarded it as a case of syphilis, and it was suggested that the patient be treated by intramuscular injections of mercury.

Case of Leucoderma Syphiliticum.

By J. E. R. McDonagh, F.R.C.S.

A GIRL, aged 22, a brunette, came up to the Lock Hospital in the middle of October, 1909, complaining of a discharge. The patient had gonorrhœa and hard chancre, a sore throat, and a very faint macular rash. The rash soon disappeared, never became papular, and as it disappeared depigmented areas were left behind. There was no doubt about it, since a gradual transition between a macule and a depigmented spot was easily discernible. No other signs of syphilis developed, but the patient remained under treatment, with the hope that the leucoderma would disappear. The leucoderma was generalized, but most marked where the skin was normally deeply pigmented—viz., neck, back, and anterior folds of both axillæ. The depigmented areas were circular, about the size of a sixpenny-piece, and discrete, none having coalesced to form a rosette, which was not uncommon in syphilis.

The case was an interesting one, in that it was all over the body, appeared about two months after infection, and clearly developed from a macular eruption.

The exhibitor mentioned that cases were not uncommon in which a spot of hyperpigmentation was to be found in the centre of a depigmented area, and that this hyperpigmentation had developed and had resulted from a papule, which had appeared in the centre of a macule. The condition was probably more common in women than men, owing to the skin being more delicate and not so red in the former, therefore the contrast was more noticeable. Treatment did not alter the condition; the lesion was a chronic one, and tended to disappear in course of time, which was often a matter of years.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

VOLUME THE THIRD

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE SESSION 1909-10

ELECTRO-THERAPEUTICAL SECTION



LONDON
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1910

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Electro-Therapeutical Section.

October 15, 1909.

Dr. SAMUEL SLOAN, President of the Section, in the Chair.

PRESIDENTIAL ADDRESS.

Electro-therapeutics in Gynæcology.

To inaugurate the work of this session, I wish to address you on a subject which has given me much thought and labour for many years. The results of this study and work must of course be considerably condensed for a single address, and may appear to you less convincing than if given in detail. This is unavoidable. What I put before you, however, will, I trust, arouse your interest in a subject which some of you may have neglected in the past.

My subject, "Electro-therapeutics in Gynæcology," I have chosen for many reasons. I have had opportunities of testing the value of this treatment in diseases of women, not by watching others or by ordering others to do it, but by doing it myself. In every case the treatment has been carried out by my own hands; the notes are my own, and I can vouch for their accuracy. Again, the subject ought to appeal to a large number of the members of the Section, for I suppose a considerable number of them are engaged in general practice, and must frequently have cases in which they can use this treatment. It requires no more knowledge of gynæcology than can easily be gained in the ordinary course of practice by a little special study, mainly from the point of view of *diagnosis*.

I hope you will excuse me for saying that as a Section there is some risk of our over-specializing. We may be drifting away from the sympathies of the mass of the profession whom it ought to be our duty

and our desire to carry with us. Our work might be made to appeal to them more, not in order to increase our membership, but to increase our influence. We should try to encourage the more general use of electricity as a remedial measure. No medical man, we ought to make it plain, can be fully equipped for the treatment of disease without an electrical installation of some sort. It need not be an expensive outfit, but it must be intelligently used. Only let him realize that it is a therapeutic power, not a something which, being ignorant about it, he does not believe in. Nor must he give it up in disgust because, perhaps owing to its laws being ignored, it refuses to produce magical results at once in cases in which everything else has failed. Let him put it fairly to the test and he will use it more, and drugs less. The days of polypharmacy are passed; the days of pharmacy as a necessary and a routine element in therapeutics are passing. Physiological and physical therapeutics are, in no small degree, taking the place of drugs, and prescribing is fast becoming a lost art. Intelligent patients no longer undervalue the advice and overestimate the value of the prescription. They are satisfied more now than formerly with a few general instructions and the administration of some medical common-sense.

Now what have drugs done for gynæcology in the past? A generation ago the therapeutical agents of gynæcologists consisted of mercury, opium, iodine, poultices, blisters, local blood-letting, applications of caustics or antiseptics to the cervix, and attempts to do so to the interior of the uterus, hot douches, medicated pessaries, &c.

Ask a gynæcologist what he now trusts to apart from surgical interference, including under this head curettage of the endometrium, and he will answer, "Douching and tampons." I have asked several gynæcologists lately, and this was the answer I got. In former days gynæcology was wholly in the hands of the physician; when a surgical operation was required, this was done by the general surgeon. At the present time the gynæcologist is essentially and almost wholly a surgeon, and so bold and successful has he become that no part of the abdomen is free from his inroads. Abdominal surgery is a field of operation fast becoming his "by right of conquest," for he it was who first proved his ability to occupy and retain it. It may well now, therefore, be asked, "Is medical gynæcology dead?" So far as carrying out systematic medical treatment in pelvic diseases nowadays is concerned, the answer may well be "Yes," for so little faith have the specialist and the general physician in this, that its measures are carried out with little expectation of benefit, and in a desultory way, and therefore

with little likelihood of profit to the patient. The physician's main treatment nowadays consists of sedatives for dysmenorrhœa, ergot for bleeding, and, wisest of all, rest and tonics to improve the general health. Now, what I am prepared to prove to you to-night is that more, much more, can be done with practically no drugs whatever, no tampons, no douches, little, if any, confinement to bed, and no "messing" by the patient herself. True, I have not dispensed wholly with the measures I have enumerated, but I am becoming less and less dependent upon them, and I frequently, with the best results, dispense with them entirely, thus putting the treatment I practise to an almost unfairly severe test. Now, if electro-therapeutics be left out in the treatment of pelvic cases, what is there left? Surgical cases, even gynæcological surgeons will admit, do not comprise more than 5 per cent. of all cases of pelvic distress in women. What about the remaining 95 per cent.? Are these to be perfunctorily treated with the pretence that the best is being done for them? I read lately a tirade by an eminent gynæcological surgeon against the medical gynæcology of the past, but what had he to offer? Mainly surgery, I find, and the treatment of the husband. Of electricity in such affections, he said, "The use of electricity in these cases is, in my opinion, one of the biggest frauds inflicted upon the confiding public." You will agree with me, I think, that the man who accuses so many of fraud convicts one—himself—of ignorance and prejudice. Think, therefore, of the temptation which the gynæcological surgeon is exposed to. Growing tired of perfunctory attempts at relieving his patient medically, he is tempted to suggest and carry out a more rapid, and certainly a more radical, method by removing the organ which he has not the patience or the skill to cure. Under these circumstances, since "there is a good deal of human nature in man," even in a gynæcological surgeon, a selfish ambition is apt to influence him. A desire to be able to reckon his ovariotomies and hysterectomies by the hundred is, I fear, sometimes a real danger to his patient, no matter how honestly he may try to rise above such a weakness. This would be less of a danger if mutilation meant always cure. Ovaries can be easily removed, but they often are so for a pain which refuses to be removed. Though for many years this has been suspected, it has of late been brought home to operators more forcibly, and a reaction is setting in in the direction of non-surgical gynæcology. Yet we have seen how hopeless its measures have been in the past.

What, then, have I to say regarding what I am proposing as a substitute for semi-profitable medical treatment, and useless as well as

dangerous operations? Can I ask you to offer hope to the woman who will rather bear the pain of her ovaries than lose them; some bleeding than the removal of her womb? Is there no substitute for mutilation but resignation on the one hand, or nerve prostration from the baneful effects of sedative drugs or so-called stimulants on the other? and the latter is too often the fate of the neurasthenic. I have no doubt it will please you if I am able to show that electro-therapeutics may prove a substitute for these; that, by its help, cure may often take the place of mutilation, and dangerous drugs may be rendered unnecessary. I do not pretend that gynæcological surgery can be very greatly reduced by electro-therapeutics, but that it will be often rendered unnecessary I am satisfied.

For the introduction of electrotherapy into the practice of gynæcology we are indebted to Apostoli, who was in some respects a great man. His work was much boomed at first, then much contemned; and I fear now that he himself is being ignored as having been of little account. Dr. Thomas Keith, the eminent pioneer in operative gynæcology, speaking of his treatment for fibroids at the close of the dedication to Apostoli of his work on "The Treatment of Uterine Tumours by Electricity," says "that in a few years you will see your treatment adopted all over the world, I have no doubt." In spite of this warm appreciation, Apostoli has gone from amongst us comparatively a disappointed man. Few are now ready to do him reverence. Of these few I am one; and I take this opportunity of paying a tribute to his memory. Had Apostoli done nothing but enforce the value of electricity in the treatment of uterine fibroids he would have done a good work, notwithstanding the condemnation of this treatment by many eminent gynæcologists of the present day — these mostly, however, belonging to the class that have not tried it. Apostoli's fame, however, does not rest only on his work on uterine tumours. Whatever ultimate decision may be arrived at regarding the effects of electrolysis, I feel sure that Dr. Apostoli has rendered a signal service to the profession by his scientific methods, which have done not a little to remove this undoubtedly valuable therapeutic agent from the hands of unqualified, though probably honest, practitioners into those of medically qualified and scientific observers. He has also contributed much valuable information, based on his own experiments and observations, on the electrical treatment of most forms of pelvic disease; though his work in this direction is either unknown or ignored by those who speak disparagingly of his better-known work. That electricity in gynæcology means mainly

electrolysis for uterine tumours seems to me to be not only the opinion of the profession in general, but even, though in a less degree, of electro-therapeutists also, with the exception of those engaged in the treatment of gynæcological cases by this means. This is evident from text-books on electro-therapeutics and on gynæcology. As an example, in one of the best of recently published works on medicine, Green's "Encyclopædia and Dictionary of Medicine and Surgery," less than one column is devoted to electricity in gynæcology, and the whole of this refers to the treatment of uterine tumours by the Apostoli method. I had the pleasure of knowing Apostoli, and have carefully studied his works; and I feel assured his observations were trustworthy and his character sterling, whilst his modesty was a silent rebuke to his detractors. He never pretended that he could by his treatment remove uterine tumours by absorption, only that he could symptomatically relieve the unfortunate possessors of those tumours; and for the proof of this statement there is abundant evidence at the present day in the records of others. My work in this direction has been very limited, but it does not take from the weight of the evidence in favour of Apostoli's views. When I have said this much, I have said practically all I mean to say on the subject of electrolysis. My endeavour to-night is to bring in review before you the whole of my work on electro-therapeutics in gynæcology.

For fifteen years past I have in most of my cases of gynæcology used electricity in some form, and the experience thus built up may be worthy of your attention. It has left upon me an impression favourable to the frequent application of electricity in this department of medicine. The picture, however, presented to me on looking back on my work with the present object in view was a vague one, which, whilst it was sufficient to influence me, was too far away, too much of an impressionist picture, to appeal forcibly to one who could not be expected to appraise its value in detail. Even to myself it was a vague picture, though one that satisfied me. When considering how to convey to you my impressions of these fifteen years' work, I felt that whilst, during the progress of the work, I was unable to see the wood for the trees, on looking back I became unable to see the individual trees for the wood. I have accordingly gone over in detail all my work during the past. That I have been able to do so with precision is due to the fact that all the cases were minutely detailed at the time. Everything has been recorded, the successes and the failures alike; not from any expectation of after-publication, but for my own guidance at the time and in the future. Sometimes the record was a despairing one, sometimes a cheering one,

but always a faithful one. In tabulating the cases I have omitted many out of a total of several hundreds. These omitted comprise the following:—

(1) Those resulting in cure where some electric treatment was applied immediately after curettage; the curetting being in my opinion the main cause of cure, and the electricity only an adjunct.

(2) Those in which all the usual forms of treatment were being employed plus some electric applications; but where there was no evidence that the latter had any material influence on the improvement obtained.

(3) All cases in which only a very few applications were made, with one exception—No. 4 on the list which I intend submitting to you—and this for a special reason which I shall afterwards give.

(4) All cases in which the electric treatment was general only, no local applications having been made, with two exceptions, Nos. 115 and 139, and for a special reason to be afterwards explained.

Although this reduces the total number very considerably, it yet leaves a sufficient number—153 cases—to judge as to the efficacy of the treatment. For a study of these I have arranged the complaints under fifteen heads. I expect to be met here with the criticism that these are labels only, not morbid entities. I frankly admit that this is so. What else, however, are our usual classifications of disease? We know diseases mainly, sometimes only by their signs and symptoms. A classification of the diseases recorded in my list of these 153 cases, to be absolutely accurate and complete, would require probably 153 different names; for no two are exactly alike. Again, a disease of the same name requires often varied treatment; first according to its degree, and next on account of the personal equation. My arrangement, however, is a convenient one. You will observe on the list that two labels are sometimes employed for the same case: for instance, septic infection and dysmenorrhœa, the one being frequently quite independent of the other, and requiring different treatment. In one instance two records are made of the same case. Here the complaints treated were quite disconnected, and were at considerable intervals of time apart.

Since this is a record of my own work only, I make no mention of the treatment of diseases which are not found in my list; nor do I refer to what others have ascertained, or have believed, to be beneficial, or the reverse, in the conditions mentioned by me. My appeal to you is a personal one; it is based solely on a review of my own work. In order to render my complete record less bulky and more capable of being used

at your leisure, as a check on my calculations and inferences therefrom, as given in the tables under different heads, I have employed the letters of the alphabet to distinguish the complaints, the kind of current employed, and the result of the treatment. The following table, which precedes the complete list of cases, will serve as a key to it :—

(1) Consecutive Numbers of the Cases.

(2) Complaints :—

a—Fibroids.
b—Hæmorrhage—apart from fibroids—menorrhagia and metrorrhagia.
*b*¹—Subinvolution without hæmorrhage.
c—Endometritis—septic ; including metritis.
d—Pelvic inflammations—cellulitis—salpingitis—ovaritis.
e—Dysmenorrhœa—various forms.
f—Pruritus—vulvar and anal.
g—Hæmorrhoids.
h—Constipation.
i—Pelvic distress—neuritis and other abnormal sensations.
j—Cystitis.
k—Irritability of the bladder.
l—Incontinence of urine.
m—Nocturnal incontinence.
s—Sterility, when a cause of mental distress to the patient.

(3) Forms of Current Employed :—

a—Unidirected—"galvanic."
b—Alternating—"faradic" and sinusoidal.
c—High-frequency.
d—Electro-chemical—"ionic."
e—Static.
f—Various kinds.

(4) Results :—

a—Practically cured.
b—Very greatly relieved.
c—Slightly or temporarily improved.
d—Unknown, or not improved.

1	2	3	4	Brief Notes of Cases	Remarks on Results
1	*a*	*a*	*b*	Married, aged 30, no children ; hæmorrhage, due to fibroid tumour of the uterus ; had been curetted	"Practically none of the hæmorrhage since this treatment, tight feeling in the abdomen gone since shortly after the electric treatment was begun, abdominal swelling decidedly less" : report one year after beginning of treatment

1	2	3	4	Brief Notes of Cases	Remarks on Results
2	*k*	*b*	*a*	Married; irritable bladder, micturition every half to one hour for one year	Passing urine only once every two hours now
3	*d*	*b—a*	*b*	Married, aged 29, one child; pelvic pain, fullness and tenderness in right and posterior fornices	"Well"
4	*b*¹	*a*	*a*	Married, aged 32, two children; leucorrhœa, uterus enlarged and cavity dilated; no improvement from previous intra-uterine applications, &c.	Pregnancy shortly after one application
5	*c*	*a*	*b*	Married, aged 25; intermenstrual yellow discharge	Almost no discharge, but what is there is still muco-purulent
6	*d*	*b*	*b*	Married, aged 30; pelvic pain, uterus drawn to left side and slightly adherent posteriorly, left and posterior fornices tender on pressure	"Kept very well for a long time, no pelvic distress now"; *per vaginam* examination shows no evidence of disease
7	*b*¹	*a*	*a*	Married, aged 29; pain when walking, uterine canal 3⅜ in., acute retroversion	Feels "so well now"; no more treatment required
8	*d*	*b*	*b*	Single, aged 32; pain in right iliac region, right ovary enlarged	"Feeling very well now, only a suspicion of pain"; tumour less in size
9	*c*	*a*	*a*	Married, aged 23; copious intermenstrual yellow vaginal discharge, a bearing-down and burning sensation	"Not only practically but actually well"
10	*c*	*a—b*	*c*	Married, aged 31; uterus to right side and somewhat fixed, canal of uterus 3½ in., muco-pus on sound after withdrawal	Curetted later; final result unknown
11	*d—e*	*b*	*a*	Married, aged 30, no children; pain in left side, prolapse of left ovary, dysmenorrhœa	"No pelvic pain at all now; last period more painless than for years"
12	*c—d*	*a*	*b*	Married, aged 25; yellow vaginal discharge; right tube is thickened to about three or four times its normal size and is firm but not sacculated	Probably gonorrhœa; note at close of treatment: "No discharge at all now; the first real influence on the discharge was the electricity"; broad ligaments now perfectly normal; was curetted by me two years later on account of some return of the discharge
13	*a*	*a*	*a*	Married, aged 38, never pregnant; fibroid in right wall of uterus, sound 4½ in. at least, dysmenorrhœa, hæmorrhage	Reduction of size of uterine canal, relief from bleeding and from pain
14	*b*¹	*a*	*a*	Married, aged 39, several children; a dragging sensation in pelvis, almost constant sickness, uterine canal 4 in.	"Very well, better than for years"
15	*d*	*a—b*	*a*	Single, aged 30; induration in left broad ligament, right ovary enlarged but movable, pain in right side	"Very well now and back to work"; no evidence now of pelvic disease *per vaginam*
16	*d*	*b*	*a*	Married, aged 26, one child; pain in left iliac region for two years, fullness in posterior and left fornices	"Very well indeed"; no evidence of disease *per vaginam*

1	2	3	4	Brief Notes of Cases	Remarks on Results
17	*i*	*b*	*d*	Single, aged 34; pelvic pain, "a gone feeling," epilepsy	Still in indifferent health in spite of all kinds of treatment and much change of residence
18	*b—i*	*a*	*a*	Married, aged 45, two children; menstrual discharge excessive and fœtid, pain, uterus fixed and tender on upward pressure	Perfectly free from the old symptoms
19	*b*[1]	*a—b*	*a*	Married, aged 25, two miscarriages; retroversion of uterus, uterine canal 3¼ in.	Length of uterine canal reduced to 3 in.
20	*l*	*b*	*b*	Married, aged 46, several children; clothing almost constantly wet with urine; when moving about urine dribbles away, even if bladder is newly emptied	"No discomfort, considerable improvement, but escape of urine if hurrying"; primary faradic current
21	*d—i*	*a*	*c*	Married, aged 29, never pregnant; "a burning heat" in the pelvis, utero-sacral ligaments tender on pressure	Very neurotic; patient lost sight of; final result unknown
22	*e—b*	*a*	*a*	Married, aged 23, never pregnant; dysmenorrhœa always, more or less; menorrhagia only occasionally	Menstruation normal; pregnancy followed, date unknown
23	*d*	*a*	*a*	Married, aged 29, one stillborn child; firm, irregular, fixed mass in posterior fornix, extending equally on each side nearly to pelvic wall	"Never was so well as now"; post-uterine thickening decidedly less than prior to electric treatment; "can now lie on my back with legs stretched out"
24	*a*	*a—b*	*d*	Single, aged 35; a myomatous uterus, hæmorrhage and pain	Final result uncertain; probably operation
25	*e—d s*	*a—b*	*a*	Married seven years, aged 27; sterile; dysmenorrhœa, uterus antiflexed, right ovary in posterior fornix, enlarged and tender, frequent turns of sickness	Menstruation much easier than formerly, sickness less; became pregnant ten months after beginning of treatment
26	*d—s*	*a*	*a*	Left tube thickened and tender, length of uterine canal 3⅝ in.; four years married, never pregnant	Pregnancy after two months' treatment
27	*i*	*b*	*a*	Single, aged 32; "a weak, smarting feeling" in the left side of the pelvis	"Hardly ever feel the pain now"
28	*d*	*a—b*	*a*	Married, aged 30, two children; fullness in both fornices, or rather a sense of resistance to the examining finger, with pain; enlargement and prolapse of right ovary	Well for two years, then probably a re-infection
29	*e*	*b*	*c*	Single, aged 40; uterus retroverted, both ovaries prolapsed and very tender to the touch; has been curetted	Very little improvement till second operation, after which patient said, "The improvement seems miraculous"
30	*a—i*	*b*	*d*	Married, aged 40, two children; pain and hæmorrhage, fibroid on anterior and right wall of uterus; sound passes in fully 5 in., epilepsy	Treatment casual; afraid of electric treatment in any form
31	*e*	*a—b*	*d*	Single, aged 30; neurasthenia, had been curetted for dysmenorrhœa	Life too strenuous mentally
32	*e*	*b*	*a*	Single, aged 25; dysmenorrhœa since first menstruation	Periods normal; "never had so little pain"

1	2	3	4	Brief Notes of Cases	Remarks on Results
33	*i—d*	*b—a*	*b*	Single, aged 35; pain in back and left side for three years, uterus drawn back by adhesions	"Longer spells of feeling well"; uterus less displaced and less tender on upward pressure
34	*e*	*a—b*	*a*	Married six years, aged 30; never pregnant; retroversion of uterus, menstrual membrane brought to me	Membrane never absent at any period till galvanic current used
35	*c—e*	*a*	*a*	Single, aged 23; endometritis, some dragging of uterus to left side, right ovary enlarged and prolapsed	"Feel better than for two years, no more need for electricity"; uterus normal in position and movable without pain, no ovary felt, broad ligaments seem normal
36	*d*	*b*	*a*	Married, aged 25; one miscarriage; uterus acutely retroverted and retroflexed, fundus fixed in left fornix	Mechanical treatment also; "Never felt so well for years as now"
37	*b*	*a*	*a*	Married, aged 34; two children and two miscarriages; menstruation too frequent, clots many and large, uterine canal fully 3½ in., cavity dilated, fullness in right fornix and some fixation of uterus, pain when attempting to push uterus up	"Menstruation has become all right, normal amount, no pain; the past has been a record year in point of health"; pregnancy eight months after beginning of electric treatment
38	*c—s*	*a*	*b*	Uterine canal 3½ in., sound causes pain, internal os contracted; ten months married, no pregnancy; aged 39	General health much improved, but no pregnancy; said to be looking "thunderingly well"; uterine canal 3¼ in.
39	*i*	*a—b*	*d*	Widow, aged 46, two children; pelvic distress, great pain, "tremblings" over abdomen, sleeplessness, extreme tenderness on pressure over bodies of lumbar vertebræ and around both ovaries	Intra-uterine medical treatment also failed
40	*i*	*b*	*b*	Relaxation of rectum, backache, general weakness	"Feel certain there is a decided improvement"
41	*j*	*b*	*a*	Married, aged 60, several children; has been in bed almost entirely for three months; pain in bladder almost constant, bladder sound causes pain and gives some sense of resistance when pushed against bladder wall	"Very little trouble from bladder, almost well"
42	*d*	*b—d*	*a*	Married, aged 43, several children; inflammatory tumour in posterior fornix, firm and tender, burning sensation in left side, spreading over hypogastrium, retroversion of uterus, pessaries could not be tolerated	No improvement till ionic medication employed, in spite of all forms of medical treatment for years, swelling in posterior fornix gradually lessened and became movable, uterus became normal in position without any pessary
43	*e—d*	*b*	*c*	Single, aged 19; severe pain in pelvis, especially at menstruation, inflammatory mass in left fornix	Continuous and varied medical treatment also; inflammatory condition removed, but pain not relieved

1	2	3	4	Brief Notes of Cases	Remarks on Results
44	*a—i*	*a*	*a*	Married, aged 41, never pregnant; subperitoneal fibroid adherent to pelvic wall, no menorrhagia, pain in left side; not worse at periods, but worse if standing much	"Quite free from pain if not overworking"
45	*c—i*	*b*	*b*	Married, aged 55, two children; yellow discharge from vagina for six years, "weak and tired feeling in lower body" since menopause seven years ago, prolapse of uterus, pain after walking	General and local treatment also, but the Faradic current required afterwards to complete the cure; "no bearing down and no pain. Can walk six miles at a stretch"
46	*e*	*a*	*a*	Married, aged 37, never pregnant; dysmenorrhœa since first menstruation, uterus retroverted, some adhesions	"Could not desire to be better"
47	*d—c*	*a—b*	*b*	Married, aged 26, never pregnant; uterus retroverted and slightly adherent, hard mass lying against left pelvic wall, tender on pressure, evidently inflammatory	"Very comfortable;" no induration left and no pain, but required curettage after for the sepsis; final result "Improved," mass described as lying against the left pelvic wall cannot now be felt
48	*c—e*	*b*	*b*	Married, aged 23, one child; pain in right side, worse just before menstruation, cervix excoriated	Required antiseptic applications to uterus also
49	*c*	*a—b*	*d*	Married twice, aged 39, never pregnant; vulva and vagina wet with muco-pus, excoriation of cervix	Antiseptic applications also; advised curettage
50	*c*	*b*	*d*	Married ten years, aged 28; never pregnant, os excoriated, right ovary prolapsed	Curetted afterwards, with but little improvement
51	*a*	*a*	*d*	Married, aged 31, never pregnant; fibroid abdominal tumour nearly 2 in. above umbilicus, steady increase of swelling, of hæmorrhage, and of pain; patient very anæmic and very weak	Cessation of growth, and temporarily of hæmorrhage; death from sudden hæmorrhage two weeks after the last application
52	*b'*	*a—b*	*a*	Married, age 37, one child and one miscarriage; pain in pelvis, enlargement of the uterus	"Quite well, unless for some bearing down"
53	*s*	*a—b*	*d*	Married, age 34, two children; sterile for three years, no evidence of pelvic disease	Result unknown
54	*c*	*a—b*	*d*	Married; endometritis, uterine canal 3½ in., cystitis	Treatment stopped after one month
55	*c—s*	*a*	*a*	Married, aged 23, one miscarriage; leucorrhœa since marriage two years ago, no pregnancy	Pregnancy
56	*f*	*a—b* *c*	*a*	Married, aged 48, several children; very severe pruritus of vulva, frequent vulvar abscesses	No other treatment of any service; all currents helped, but high-frequency most
57	*k*	*b*	*b*	Single, frequent micturition, urine normal, urethra had been dilated	"Much less bladder trouble"

1	2	3	4	Brief Notes of Cases	Remarks on Results
58	*k*	*b*	*a*	Married, aged 40, several children; irritability of bladder, micturition every five minutes	Now micturition every two to three hours; patient said current was very "soothing"
	g	*c*	*a*	Bleeding piles "for years and years"	"Got so much benefit from electric treatment"
59	*m*	*b*	*d*	Single, aged 17; nocturnal incontinence of urine all her life; a mentally defective girl	No improvement at all; dilatation of urethra with no benefit
60	*i*	*b*	*b*	Widow for eight years, aged 29; four pregnancies; burning heat in hypogastrium, orgasms about once a day, yellow vaginal discharge; had been curetted	Much less burning heat and much less discharge
61	*c—e*	*a*	*b*	Married, aged 34, never pregnant; yellow vaginal discharge for years, dysmenorrhœa	"Treatment has done much good in many ways"
62	*c*	*a*	*b*	Married eleven years, aged 36, never pregnant; menstruation scanty and fœtid, backache	Improvement in nature of menstrual discharge and in general health
63	*a*	*a—b*	*b*	Widow, aged 47, one child; fibroid tumour in anterior wall of uterus, increasing hæmorrhage and yellow discharge, great pallor and weakness, length of uterine canal 2¾ in., but enlargement of abdomen quite apparent when patient dressed	"A great deal better, energetic, and fit for work"; improvement continued after treatment ceased to be required
64	*c—d*	*b*	*c—b*	Married, aged 37, one child; copious dirty yellowish discharge round cervix, retro-displacement of uterus, tenderness on pressure in left fornix	Final result, "So much better"; uterus in normal position; curettage was required for the septic condition
65	*e—b*	*b—a*	*a*	Married one year, aged 25, no pregnancy; painful menstruation, worse since marriage, menorrhagia, uterus retroverted, with fundus adherent in left fornix, dragging pain on right side of pelvis; had been curetted with partial benefit	"Very well indeed" at close of treatment
66	*c—d*	*b*	*d—a*	Married, aged 28, two children and one miscarriage; uterus enlarged, fixed and tender, yellow discharge; very poor health	"Stronger, no pain, appetite good, doing all my own work now." But still yellow discharge
67	*i*	*a—b*	*b*	Married, aged 30, one child and several miscarriages; coccygodynia	"Less pain in coccyx than for a year and a half"
68	*a*	*a—b* *c*	*b*	Married, aged 40, never pregnant; multiple fibroids, no hæmorrhage, but frequent mild attacks of peritonitis	Decrease in bulk of abdomen, tumours became more easily differentiated after high-frequency current
69	*j*	*b*	*a*	Married, aged 25, never pregnant; pain after micturition for years, pain on pressure of sound on bladder wall, pus in urine	"No bladder trouble whatever" after treatment
70	*d*	*b*	*a*	Single, aged 20; soft elastic mass in left fornix, filling up the side of the pelvis; uterus moves with it but both are practically fixed	"Well and practically free from pain." Tumour in left fornix can hardly be felt now

1	2	3	4	Brief Notes of Cases	Remarks on Results
71	*e*	*b*	*b*	Married, aged 30, one miscarriage; pain from two to three days before onset, during the period, and till the yellow discharge ceases	"Absolutely no pain" during following menstruation, but final result unknown
72	*i*	*a*	*b*	Married, aged 40, one child and two miscarriages; a burning sensation in the pelvis, with backache	"You have hit the right treatment now"
73	*l*	*b*	*a*	Married, several children; expulsion of urine on sudden movement, urine normal, cystocele and rectocele	"Really quite well"; primary faradic current employed
74	*c—e*	*b*	*a*	Married, aged 32, four pregnancies; constant yellow discharge, pain lasting nearly all the time of menstruation; has been three times curetted	"Very well now"
75	*i*	*b*	*a*	Married fourteen years, aged 39, never pregnant; an open feeling in the vagina	"Feeling firmer and better"; primary faradic current used
76	*d*	*b*	*a*	Married seven years, aged 28; never pregnant, right tubo-ovarian inflammation, intense pain on touching the right ovary	Uterus smaller, right ovary not now enlarged, left indurated mass smaller; general health good
77	*l*	*b*	*a*	Married, aged 35, one child; incontinence of urine since confinement two-and-a-half years ago, vulva scalded by the urine	"Can now dance with no discomfort whatever"; primary faradic current
78	*l*	*b*	*a*	Married, one child; urine comes away when lifting anything or laughing	"Bladder now almost no trouble"; primary faradic current
79	*i*	*b*	*a*	Single, aged 26; a "sick pain" in the pelvis; was curetted with no relief	"Better than for years"
80	*i*	*b*	*a*	Married, aged 34, six children; pelvic neuritis; curettage was of no avail	Her medical attendant says: "Completely better of the neuritis"
81	*i*	*b*	*b*	Single, aged 35; pain in the left side of pelvis, uterus fixed in retroversion, ventrofixation was afterwards performed; backache less after the operation, but the pain in the left side was worse	"Walking much more and with less pain"; had dorso-abdominal current also
82	*j*	*d*	*a*	Married, aged 48, several children; bacteriuria, attacks of great sickness and intestinal colic	"The following winter was the first for years requiring practically no confinement to bed"
83	*i*	*b*	*c*	Single; vague pains in vulva, bladder and pelvic cavity; had had cystitis	Think there was afterwards an operation for "ulceration of the bladder"
84	*f*	*c*	*a*	Married, aged 40; several children; "Eczema inwardly, intense and almost like to take my reason away;" had been curetted for endometritis	"Very well now in every way"; electricity of little avail till second curettage; pruritus cured only after resuming the high-frequency
85	*i*	*f*	*c*	Single, aged 35; frequent intense orgasms; had been curetted and had an ovary removed with no benefit	More relief from the electrical than from any other treatment, but not much satisfaction from any form of treatment

1	2	3	4	Brief Notes of Cases	Remarks on Results
86	*a*	*b*	*b*	Single, aged 40; increasing hæmorrhage from fibroid which reaches to near the umbilicus and occupies nearly the whole of the pelvic cavity	Diminished hæmorrhage, but continued growth of tumour; primary faradic current employed
87	*h*	*e*	*a*	Single, aged 27; constipation absolute, following operation for piles	"I can honestly say I never felt better than I do now"
88	*g*	*c*	*a*	Married, aged 32, three children; large painful pile, partly external and partly internal	Practically well, no hæmorrhoids now. Fissure of the anus also, which required zinc ionization to cure it
89	*l*	*b*	*a*	Married, aged 50, several children; prolapse of anterior wall of vagina, with incontinence of urine, watch-spring pessary gives only slight relief	"The electricity has made a great difference in me." Primary faradic current employed
90	*i*	*b*	*a*	Married, aged 25, three children; pain in upper left iliac region, disappearing on resting, throbbing pain in mid hypogastric region during the night, both pains since first confinement five years previously, except during six months of pregnancy; pain on pressure in neighbourhood of left utero-sacral ligament	"Feeling awfully well since the internal electrical applications"
91	*c*	*d*	*a*	Married, aged 32, two children; very copious purulent discharge, fœtid and "soaking everything"	Discharge now simply a milky mucus, her doctor says. "There is a tremendous change. The discharge is about a twentieth part of what it was"
92	*e*	*b*	*c*	Single, aged 40; dysmenorrhœa for years, pain during two days preceding flow, "a sort of suction pain," uterus displaced to left side and flexed, uterine canal 3½ in.; uterus was dilated and curetted; pain "terrible" some months after the operation	Improved but relapsed on account of fatiguing occupation: was a school teacher
93	*d*	*c*	*a*	Married, aged 47, never pregnant; chronic fixation of lower segment of uterus, constant pelvic trouble for twelve years	"Feel now as if the pain never would come back"
94	*g*	*c*	*a*	Hæmorrhoids for one year, pain and copious bleeding, ordinary treatment of no avail	"No pain, no bleeding, only a fullness"
95	*g*	*c*	*a*	Married, aged 40, two children; pain and hæmorrhage from piles for one year	"Practically no trouble from the piles now"
96	*d*	*c*	*b*	Married, aged 40, several children; tenderness and thickening of left broad ligament, a dragging pain in left iliac region	Very much improved; pain in pelvis almost gone, but left broad ligament still thickened and tender; no return of the pain in the side six months after

1	2	3	4	Brief Notes of Cases	Remarks on Results
97	*d*	*c*	*a*	Single, aged 30; uterus retroverted, retro-uterine inflammatory mass, adherent ovary, dysmenorrhœa and intermenstrual pain	Her doctor says "improvement has continued since you saw her; she can go about now as an ordinary human being; was quite unfit for anything before"
98	*f*	*c*	*a*	Single, aged 26; "fearful itching" at vulva, in no way improved by ordinary treatment	Remained practically free from pruritus for six years after treatment
99	*h*	*e*	*a*	Married; "no rectal expulsive power"	"Very small amount of aperient required now"
100	*i*	*c*	*a*	Single, aged 26; weakness and pain in pelvis; had been successfully treated for septic endometritis by curettage	"Very much better in every way, no pain and no weakness now"
101	*f*	*c*	*a*	Married, aged 40, two children; pruritus of the perineum, eczema of anus; no relief from local medicinal applications	"Last night was the first whole undisturbed night for six years"; final report, "practically free for past two years"
102	*h*	*e*	*a*	Married, aged 66, several children; atony of rectum, constipation sometimes absolute	"Had practically no trouble for six years after"
103	*h*	*e*	*a*	Married, aged 30; chronic obstinate constipation	"I do not think you realize the good you have done me"
104	*e—s*	*c—e*	*a*	Married two and a half years, aged 27, never pregnant; dysmenorrhœa, sometimes severe, dyspareunia all through married life	Pregnancy followed
105	*h*	*c—e*	*a*	Married, aged 36, several children; chronic severe constipation	"Bowels move naturally now"
106	*m*	*b—c*	*b*	Single, aged 14; nocturnal incontinence, vulva scalded, meatus and vaginal orifice congested	"So much better now"; improvement became more apparent a few weeks after cessation of treatment
107	*d*	*c*	*a*	Married, aged 40, two children; pain in right iliac region, especially if standing much or over-working; some fixation of uterus, inflammation in right broad ligament	"Pain has not returned"; induration in pelvis less
108	*e*	*d*	*a*	Married, aged 35, one child and one miscarriage; membranous dysmenorrhœa, curetted four and a half years ago without any relief	Her doctor says: "Membrane feathery now, no pain; pregnancy followed"
109	*c*	*d*	*a*	Single, aged 21; purulent endometritis, erosion of cervix, anæmia	General condition much better, practically no discharge now
110	*j*	*c*	*a*	Married; prolapse of anterior wall of the vagina, frequent painful micturition, pus in urine, a burning sensation in urethra when urine is being passed	"Improvement all along the line; can retain urine from three to five hours"
111	*c—i*	*d*	*a*	Married recently, aged 38; purulent endometritis, dysmenorrhœa, a burning sensation in the pelvis, curettage seemed to give no relief	Vaginal discharge small in amount and white

1	2	3	4	Brief Notes of Cases	Remarks on Results
112	*c*	*c*	*c*	Married, aged 30, one miscarriage; twice curetted after abortion; said to have had peritonitis, ten weeks in bed; present complaint hypogastric pain, never free from this for more than one day at a time; some fullness and tenderness in the left fornix, prolapse of ovary, some muco-pus around cervix	Vaginal monopolar application only, still pus around cervix, but no evidence of pelvic inflammation *per vaginam*; curettage after, result unknown
113	*i*	*c*	*d*	Single, aged 30, pelvic neuritis; sciatica, pain, sickness and headache during menstruation	High-frequency applications externally only, curettage advised; latest report from her medical attendant: "Condition remains still unchanged since she was curetted; there is still pain and weakness"
114	*b*	*d*	*a*	Married, aged 42, four children; metrorrhagia, practically continuous; curettage and intra-uterine monopolar high-frequency applications failed to arrest the bleeding	Bleeding stopped after two intra-uterine applications of copper electrode, and has not returned
115	*b*	*b*	*a*	Single, aged 35; great nerve prostration, uterine hæmorrhage twice each month, sleepless and "nervous;" has just had a month's holiday with no benefit	Dorso-abdominal sinusoidal current; great improvement in general health; sister says the improvement is "miraculous"
116	*c*	*d*	*a*	Married, aged 45, five children; copious yellow discharge from vagina, in spite of long-continued treatment, including curettage on two occasions, left pyosalpinx which fills and empties itself into the uterus and the vagina at regular short intervals; tongue always foul	Discharge now slight and white only; freer from pain than for eight or nine years; tongue is at last perfectly clean
117	*c*	*d*	*a*	Single, aged 32; copious purulent discharge from the vagina, in spite of curettage four months ago, always yellow fur on tongue; carbolic-acid applications of no service	Report one year after close of treatment: "Never so well as now since the discharge started; no syringing required"
118	*f*	*c*	*a*	Married, aged 70, several children; diabetic pruritus vulvæ	No pruritus now unless gross error of diet
119	*b*	*d*	*a*	Married twice, aged 48, several children; metrorrhagia, bleeding comes in gushes, intermenstrual yellow discharge, severe abdominal pain daily in the early morning, lasting for about one hour, uninfluenced by diet, evidently toxæmic, frequent attacks of sickness	Report ten months after end of ionic treatment: "Periods normal, but still some yellow discharge; the abdominal pain has practically gone since the new treatment began"; had only four intra-uterine copper electrode applications
120	*c*	*d*	*a*	Married, aged 35, one child; thick muco-pus around a ragged cervix	Absolutely no discharge around cervix two years after; had had four ionic applications only
121	*c*	*d*	*a*	Married, aged 32, one child; mass of muco-pus firm and dry on account of alum douches, has had yellow discharge more or less for years, was two or three times curetted	Her doctor reports two years after treatment: "Has remained absolutely well ever since the treatment"

1	2	3	4	Brief Notes of Cases	Remarks on Results
122	*b*	*d*	*b*	Married, aged 37, never pregnant; hæmorrhage more or less ever since aged 17, intermenstrual pain in both iliac regions for years; length of uterine canal 2½ in., though by digital examination body of uterus seems to be larger than this would indicate	Report three months after: "Able to do what before seemed impossible, better than for years:" report by her husband nine months after close of treatment; "There is no doubt the treatment did her good." Bleeding returned, and she was arranging to return from Ireland for treatment when she died suddenly from heart failure
123	*c*	*d*	*a*	Single, age 29; copious discharge of muco-pus from the vagina, which has lasted for several months, dysmenorrhœa; was curetted some years ago	Now no vaginal discharge, practically no dysmenorrhœa since the ionic treatment nine months ago
124	*c—b*	*d—c e*	*a*	Married, aged 28, one child; intermenstrual pain, yellow discharge since confinement, prolapse of anterior wall of vagina, uterine cavity dilated, length of canal 3¼ in., menorrhagia, excoriation of cervix	Very well, quite firm and practically no discharge now; "husband says she is a new woman," September, 1909
125	*c*	*d*	*b*	Married, aged 35; walls of vagina bathed with pus in spite of systematic and frequent douching; was curetted five years ago	"Practically no yellow discharge, never got so much benefit from any treatment," report some months after cessation of treatment
126	*c*	*d*	*a*	Married, aged 35, several children; toxæmia, copious muco-pus around cervix, cervical erosion about the size of a shilling, which bleeds when touched with a cotton-covered probe	Report at end of treatment: "Absolutely no muco-pus around cervix; no erosion now"
127	*j*	*d*	*b*	Married, aged 53; urethritis and cystitis, chronic	"General uneasiness much less"
128	*c*	*d*	*a*	Single, aged 35; erosion of cervix, which is covered with muco-pus, pruritus of vulva and anus, anæmia	Discharge now simply pure mucus; general health improved
129	*c*	*d*	*a*	Married, aged 33, four children; "continuous white-yellowish discharge between times"; general health poor, pain on upward pressure of uterus, and in posterior fornix, broad ligaments normal, cervix injected, but no erosion, pus in lumen of speculum, uterine canal 3¼ in.; anxious to avoid operation	At close of treatment only slight mucus around cervix, no pain on upward pressure of uterus or in posterior fornix; "all right in every way;" letter six months after cessation of treatment
130	*c*	*d*	*a*	Married, aged 32, four children; yellow discharge for years, severe laceration of left wall of cervix, erosion fully the size of a florin around os	Cervical erosion healed, general health much improved; a possible tendency to yellowness in discharge one year later

1	2	3	4	Brief Notes of Cases	Remarks on Results
131	*d*	*a*	*a*	Married, aged 31, several children; pain in right iliac region, yellowish discharge since first confinement, uterine canal dilated, right ovary tender but not enlarged, right tube thickened and tender, dysmenorrhœa, chronic diarrhœa, general health very poor; local and general treatment for three months before and for five months after curettage, with practically no benefit	Thickening in right tube removed under galvanic current, chronic diarrhœa cured by vacuum electrode in rectum
132	*c*	*d*	*b*	Married four years, aged 30, never pregnant; intermenstrual yellow discharge, large yellowish cheesy mass over and around cervix, cervix around os red, uterine canal 3¼ in., "generally ill" since marriage, probably gonorrhœa	"No discharge now." Greatly improved in every way, but still copious white muco-pus around cervix; treatment discontinued
133	*c*	*d*	*a*	Married, aged 38, one pregnancy; a yellow discharge lasting for two or three days at middle of the intermenstrual period, preceded by pain and uneasiness, which are relieved after discharge has passed	Intermenstrual purulent discharge cured after three months' treatment
134	*c—s*	*d—c*	*a*	Married, aged 29, two children; toxæmia, weakness and pain in pelvis, intermenstrual yellow discharge, sterility	"No weakness and no pain, discharge slight and pure mucus." Became pregnant five weeks after cessation of treatment
135	*c*	*d—c*	*a*	Married four years, aged 40, never pregnant; has been advised dilatation and curettage by her medical attendant for septic endometritis and general weakness	"No discharge since; very well"
136	*c*	*d*	*a*	Single, aged 35; pallor and emaciation, muco-pus around cervix, os raw looking; melancholia	Cervix normal, os perfectly clean and healthy looking, no improvement in the melancholia
137	*i*	*b*	*a*	Married; urethritis and cystitis, pyuria, micturition required about every half-hour, bladder sound 4 in., slight pain at fundus, compared to the pain of passing the sound along the urethra	"No bladder trouble now." Drugs and special diet had proved of no service
138	*h*	*c—e*	*a*	Married, aged 42; great depression of spirits, alternate constipation and diarrhœa, has had operation for hæmorrhoids, followed by frequent dilatation of anal canal for stricture	More power of expulsion now and of retention
	b	*d*	*a*	Profuse menorrhagia	"Much less discharge monthly than formerly"
139	*e*	*c*	*b*	Aged 32, single; dysmenorrhœa, "pain comes and goes as the discharge stops and flows"; neurasthenia, uterus infantile and flattened	A neurotic girl, no local treatment employed; "much improved in every way"

1	2	3	4	Brief Notes of Cases	Remarks on Results
140	*c*	*d*	*b*	Aged 23, single; dysmenorrhœa for four years, pain lasting for twelve hours from onset of menstrual flow, internal pain at mid-time of a shooting nature, intermenstrual yellow discharge for a year; was curetted five months ago, pain just as severe and continuous since, and yellow discharge increased; sent to me for ionic treatment	Much improvement in local symptoms since electric treatment, but unable to continue it
141	*c*	*d*	*c*	Aged 40, married; septic discharge from vulva, vagina and uterus, said to have been continuous and yellow for two or three years; has been curetted for this three times; vulva now bathed with "laudable pus" in spite of regular douching	Discharge became mucous only, and of small amount, but again became more purulent and the treatment was discontinued; her medical attendant, in reply to my inquiries, says she is "disappointed"
142	*i*	*d—c*	*b*	Single, aged 25; aching sensation at vulva when standing (no cause discoverable), abrasion of lips of os about 1 in. in diameter, small tumour in posterior wall of cervix	Great improvement, but still some wearied sensation
143	*c—d*	*d—c*	*a*	Single, aged 28; dysmenorrhœa, with almost constant sickness, intermenstrual discharge constant, very copious and fœtid, erosion, size of florin, round os, ovaritis	"Practically quite well now"
144	*i*	*c*	*a*	Aged 40, married, two children; occasional spasms in hypogastrium, some fixation of uterus with pain on upward pressure, pain at internal os from passage of sound	"Practically none of the pelvic discomfort since"
145	*g*	*c*	*a*	Married, aged 65, several children; protrusion of large mass of bleeding piles	No bleeding, and piles much reduced in size
146	*c*	*d*	*a*	Married, aged 32, two children; pain in lumbar and left iliac regions since second confinement, six months ago, increased by upward pressure of uterus, intermenstrual discharge	Pure, clear mucus only, erosion healed
147	*g*	*c*	*a*	Married, aged 54, two children; hæmorrhoids	"No bleeding or distress of any kind now, in spite of fatigue"
148	*b*	*a*	*a*	Widow, aged 45, two children; menorrhagia, uterine canal 4 in., cavity roomy, walls unduly hard, great exhaustion, pallor and emaciation; "fibrosis uteri"	Improving steadily, "never had so satisfactory a period in my life as last one: feel as if I were being remade"; treatment not yet finished
149	*c—e*	*d*	*a—b*	Married, aged 35, never pregnant; dysmenorrhœa and septic endometritis; was curetted nine years ago	Letter 27/9/09: "Much better and able to face any trouble, but only if I take sufficient rest; there is sometimes a slight milky discharge, and still some pain during menstruation"

1	2	3	4	Brief Notes on Cases	Remarks on Results
150	*d*	*d*—*a* *c*	*a*	Married, aged 44, three children; pain and burning heat in right iliac region, more or less always, but worse during the past few weeks, discharge slightly yellow for two months, discharge around cervix mainly mucous, some thickening of right tube, cervix severely lacerated	Just finished treatment; practically well
151	*c*	*d*	*a*	Single, aged 30; purulent discharge from the vagina, coagulated mucopus around eroded cervix	Almost perfectly well in every way, but treatment not quite finished
152	*c*	*d*	*a*	Single, aged 45; "worn out," "nervous and sleepless," septic endometritis, dysmenorrhœa	"Almost quite well; very great improvement in general health"
153	*c*	*d*		Single, aged 32; discharge of pus from the vagina, excoriation around os, about ¾ in. in diameter	Improving, but treatment only recently begun

Some important omissions may be noted from the above list. All purely and indisputably surgical cases are excluded. Displacements also are omitted; although it will be seen from the notes that a large number of the cases in the list had retro-displacement. This, though sometimes of considerable importance, as evidenced by the great comfort derived from a properly fitting pessary, does not of itself require mention here; since, when uncomplicated by inflammatory condition of the pelvis or by enlargement of the uterus, it cannot be considered a pathological condition. Cases requiring the galvanic cautery are also omitted since the effect is caustic and not electric. In tabulating the results under the different items I have reckoned *a* and *b* amongst the benefited and *c* and *d* amongst those not benefited, although some of the latter were partially improved.

The first in the list is fibroids. No case of fibroid tumour was treated by electricity unless for some symptomatic reason, usually bleeding or pain. The following are the results:—

TABLE *a*.—FIBROIDS.

			Improved	Not improved
Treated by *a* alone	...	4	3	1
,, *b* ,,	...	2	1	1
,, *a* and *b*	...	2	1	1
,, *a*, *b* and *c*	...	1	1	0
Totals	...	9	6	3

This record, though a small one, is not unfavourable to the employment of electricity in cases of fibroids, where these are causing bleeding or distress. Out of a total of nine, six cases showed sufficient improvement. Of the unsuccessful cases, one—No. 30—suffered mainly from pain. She was an epileptic, and was so frightened by the current that the treatment had little chance. In another case—No. 24—where there was pain and hæmorrhage, absolutely no good resulted from the electricity or from a course of treatment which she had obtained in a hospital. Regarding the cause of death from hæmorrhage, the report, one month before the fatal hæmorrhage, is: "Perhaps fully more than half as much blood lost now as was the case a year ago; the general health, compared to one year ago, is now decidedly better." Of the successful cases, the result was in some instances most gratifying. As an example, in Case 13, two months after treatment commenced, the length of the uterine canal fell from "4½ in. at least" to 4 in.; whilst two months after the cessation of the treatment, the patient's husband wrote to me thus: "My wife has been improving so rapidly that time has passed unknown to me. I am sure you would be quite proud if you could see how well she looks. She is quite well after her period is over, and the pain is now gone." The limited time at my disposal to-night prevents me from entering into the details of the cases. I would state it as my opinion, however, that, from my own somewhat limited experience, electricity should always be tried in the treatment of bleeding fibroids before operation is resorted to. To explain why in some cases the relief from the hæmorrhage is marked and prompt, whilst in other instances the treatment is less successful or fails, I would remind you that the bleeding is not from the tumour but from the endometrium. In Roberts's "Outlines of Gynæcological Pathology," p. 173, the following remarks occur: "Sometimes the most remarkable changes are found in the endometrium. The whole endometrium is involved in an overgrowth of its elements, wrongly called 'endometritis,' but it is not, as such, an 'itis' at all. The condition is really adenoma of the body of the uterus; vessels, glands, and stroma share in the general overgrowth, and the mucosa may sometimes be half an inch thick; or pedunculated adenomata (mucous polypi) are found. This increase in thickness is not so in every case, but appears to occur mostly in uteri with large submucous fibroids; with subperitoneal growths the endometrium may not be affected at all, or may even be atrophied. Atrophy of the mucosa in uteri containing fibroids which project into its cavity is also seen. On the surface of such a tumour there is no endometrium, or only a few

atrophied remains. In the depression into which such a tumour fits in the opposite wall a similar atrophy of the endometrium is found—it is due to pressure—while around the tumour is a distinct collar or corona of vascular endometrium, and at its very edge many of the delicate vessels of the endometrium are ruptured. Exactly the same condition occurs in the opposite pit into which the fibroid fits: deep in the hollow no endometrium, at the edge of the cup thick endometrium and a vascular corona of ruptured vessels."

The effect of the applications will thus depend on the parts of the uterine cavity in which the electrode is lying, and of this the operator must remain ignorant unless from information obtained from the results. The treatment by the galvanic current ought, with care, to be perfectly safe. In evidence of this I refer you to Cases 4 and 26, in which pregnancy followed immediately after the applications of moderately large doses to the interior of the uterus—in the former case after one dose of 50 ma. for seven minutes, in the latter immediately after the fifth application, where the average dose had been about 60 ma. In this latter case the lady had been previously under skilled medical treatment, and had been four years married without having become pregnant.

Regarding hæmorrhages apart from fibroids I can speak with no small satisfaction. The following table shows that there were in this group no failures, no case requiring after-curettage:—

TABLE *b*.—MENORRHAGIA AND METRORRHAGIA.

			Benefited
Treated by *a* alone	...	4	... 4
,, *b* ,,	...	1	... 1
,, *a* and *b*	...	1	... 1
,, *d* alone	...	5	... 5
Totals	...	11	... 11

Of these, one—No. 115—was not treated locally, but with dorso-abdominal applications of the alternating current, the benefit derived being probably due to an improvement in the tone of the abdominal circulation.

In Class b^1—subinvolution—there are five cases. I have made this into a separate class because the cases in it were unaccompanied with menorrhagia or metrorrhagia or with septic infection of the endometrium. The patients usually complained of pain or other pelvic distress, and their general health was poor. The enlargement of

the uterus probably arises in such cases from an inflammation of its substance.

TABLE *b*[1].—SUBINVOLUTION.

			Benefited
Treated by *a* alone	...	3	3
,, *a* and *b*	...	2	2
Totals	...	5	5

As might have been expected, the following class, *c*—septic infections of the endometrium and the uterine walls—is by far the largest number, being in all forty-five. The septic fiend is woman's greatest enemy, and is the despair of her physician. I have referred in a previous communication to you to the hopelessness of ordinary medicaments in such cases.

Before I began to treat these by electro-chemical methods I had treated all cases of sepsis by the curette, and, in nearly all my cases, with great benefit. Some further treatment was usually required, however, to complete the cure. I have practically abandoned this operation now, however, as a routine treatment, and my results are better than in the days of universal curettage. There may still be exceptional cases in which it may be advisable to perform this operation, but this will usually be as a preliminary to, not as a substitute for, ionic treatment. As an object-lesson in this connexion I may mention that in seven of the septic cases in the following table curettage had been previously performed without relief, and in two of these cases this had been done on three separate occasions. Notwithstanding what I have said and inferred regarding curettage, I am ready to admit that I know of few forms of treatment to compare with skilfully executed curetting in suitable cases. That it can, however, often be superseded by electro-chemical treatment I know from a considerable experience of both.

TABLE *c*.—SEPTIC ENDOMETRITIS.

			Benefited	Not benefited
Treated by *a* alone	...	8	8	0
,, *b* ,,	...	6	3	3
,, *a* and *b*	...	4	1	3
,, *c* alone	...	1	0	1
,, *d* ,,	...	26	25	1
Totals	...	45	37	8

Several deductions may be drawn from the above table, and some questions suggest themselves. That all these for which *a* alone were employed were successful is not to be wondered at, for the action of the

intra-uterine electrode is antiseptic, if not caustic also. That *a* and *b* cases have fewer successful than unsuccessful results evidently arises from the fact that *a* alone having failed, *b* could only act by improving the tone of the circulation. That *b* alone should have succeeded at all requires some explanation. I find that in one of the cases—No. 45—the septic condition was cured by local antiseptic applications and douches; but, in spite of this improvement and of the benefit derived from general treatment, the faradic current was required for the pelvic pain: so that in this case little, if any, credit may be due to it for the removal of the sepsis. Case 48 was also treated by antiseptics, with much the same results as in Case 45, the faradic current acting mainly as a necessary pelvic tonic. Case 50 derived little benefit from any treatment, including curettage. Case 64 was also curetted on account of the continuance of the septic discharge. The faradic current, however, sufficed to materially reduce the pelvic inflammation. Case 66 received decided benefit generally, but there was no improvement in the septic condition. In Case 74 the current was employed after a third curettage, and was necessary for the treatment of the dysmenorrhœa. It is therefore obvious that the faradic current is of little, if any, service in cases of uncomplicated sepsis. This might quite have been inferred, but these cases attest the fact. All the cases treated by the ionic method have done well with one exception. In this case the lady was sent to me after three operations for curettage had failed. Pus was welling out of the vagina, and there seemed no hope but from hysterectomy, the septic process having penetrated deeply into the walls of the uterus. After two months' treatment the discharge became scanty and was composed of pure mucus only. The patient now, however, began to fret at the treatment, owing to arriving in a state of fatigue, in consequence of a long railway journey in cold and wet weather. The treatment, therefore, was less thoroughly applied and the discharge gradually returned. I advised either thorough treatment or none, and the patient is at present considering the matter. Thus, out of twenty-six cases treated by electro-chemical methods, twenty-five have been successful; whilst the remaining one promised to be so, and under suitable circumstances probably would have been a success also. Many of these cases had had curettage performed previously without a cure, and, whereas in all other forms of treatment for endometritis recurrences were quite to be looked for, I am observing that, once this treatment is allowed to take full effect, the result is practically a complete cure, there being no return of the patient for other treatment; whilst the

continued improvement in the general health vouches for the completeness of the cure. No douching is permitted or required during the treatment, and there is no necessity for any change in the patient's habits; moreover, the duration of the treatment is less than that of antiseptic and caustic applications to the uterine canal in the usual manner. That the case treated by high-frequency currents alone—No. 112—failed is probably due to the fact that the applications were vaginal only, not intra-uterine.

The affections included under the next head—*d*—vary considerably in their nature and in the degree of their severity. Whilst cases of sepsis may be obstinate, those arising from pelvic inflammations, including parametritis, perimetritis, salpingitis and ovaritis, singly or in combination, are often quite hopeless by the ordinary method. Treatment seems to relieve, but only till a little chill or a little fatigue reminds the patient that the cause of her aching pain is still there. Displacements of the womb complicate matters, pessaries being often tried, only to find that they make matters worse. These affections are the consequence of sepsis, though there may be no vaginal discharge observed. The following table indicates the results of the electric methods of treating them :—

TABLE *d*.—PELVIC INFLAMMATIONS.

			Benefited		Not benefited
Treated by *a* alone	...	5	... 4	...	1
,, *b* ,,	...	10	... 9	...	1
,, *c* ,,	...	4	... 4	...	0
,, *a* and *b*	...	6	... 6	...	0
,, *d*, *a* and *c*	...	1	... 1	...	0
,, *d* and *b*	...	1	... 1	...	0
,, *d* and *c*	...	1	... 1	...	0
Totals	...	28	... 26	...	2

This seems at first sight incredible even to myself. I have gone over every case, however, more than once, and I see no reason to change the figures. True, in most, if not in all, of them the ordinary therapeutic measures were employed to assist, whenever they seemed to me serviceable; but from my previous experience of these I feel convinced that the good results are in a significant degree due to the electric measures. Indeed, I do not believe that such results could be obtained without the help of electro-therapeutics. Case 43 is put down as unsuccessful, and yet the last record says: "There is still great pain, but a careful examination can detect nothing except tenderness on pressure of the uterus, the broad ligaments, and the pelvic wall at the sides and posteriorly. There is great tenderness also over the lumbar vertebræ."

A gynæcological surgeon in Glasgow was then appealed to. His diagnosis was "neuralgia," and the treatment he advised was "cycling." There had been an inflammatory mass in the left fornix. This case has been put down as a failure on account of the continuance of the pain, though the inflammatory condition had disappeared—at least there was no evidence of its presence. Of course, as may be found by a reference to the total list of cases, several of those noted in the previous table as benefited are marked *b* on the list; that is, there was in these instances great improvement only. However, these so marked constitute eight only of the twenty-six benefited, leaving eighteen of "practical cures." It is interesting to note that, of these eight, half of them are found to be amongst the first twelve cases of the list. A natural deduction from this is that the less favourable results at the beginning may have been due to want of experience of the treatment, the cases in the list being arranged as nearly as possible in the order of the time of their treatment. Some of the cases in Table *d* were long under treatment, but this was compensated for by the fact that in no case was an operation required for the condition which I had treated electrically. One of them—Case 42—had unavailing treatment, including various forms of electric applications by myself for years, a practical cure having been effected only after ionic medication was commenced, the result then having been, in the language of the patient, "marvellous." This patient has just a few days ago replied to my inquiry as to the final result. Her letter is a most grateful one and fully confirms the truth of the remark recorded in my notes at her last visit nine months ago, that "improvement will probably continue without further treatment."

The next table refers to cases of dysmenorrhœa simply as a symptom, no attempt being made to differentiate between various forms of this affection. In some of the cases there was associated septic endometritis, and in others inflammatory affections of the pelvis; but as these may be present with no dysmenorrhœa properly so-called, I am dealing purposely with painful menstruation only, in so far as I have treated it, specially, by electric methods.

TABLE *e*.—DYSMENORRHŒA.

		Benefited	Not benefited
Treated by *a* alone	4	4	0
,, *b* ,,	8	5	3
,, *a* and *b*	4	3	1
,, *c* alone	1	1	0
,, *d* ,,	2	2	0
,, *c* and *e*	1	1	0
Totals	20	16	4

In a large number of cases of dysmenorrhœa general electric treatment is more suitable than local. In purely neurotic cases it is likely to be of more service. Case 139 was distinctly one for general treatment, and no local applications were made.

Table *f* refers to a most distressing complaint, frequently very chronic and very resistant to treatment—pruritus of the vulva. In all the cases of this which I have treated high-frequency currents have been employed by means of the effluve or vacuum electrode, and, looking back on some cases in my practice before I knew of electric methods of treating disease, I often wish I had then had such effective means for their cure. No case has resisted this treatment. One—Case 56—had been temporarily benefited by galvanism and by the faradic current, but the pruritus disappeared for good only after high-frequency currents were employed. Even a case of diabetes—No. 118—yielded to this treatment. Though not absolutely cured, the trouble is now very slight, and is easily kept under by a very moderate attention to diet.

TABLE *f*.—PRURITUS—ANAL AND VULVAR.

			Benefited
Treated by *a*, *b* and *c* ...	1	...	1
„ *c* alone ...	4	...	4
Totals ...	5	...	5

Only otherwise incurable cases have been so treated. A creolin lotion and some ammoniated mercury ointment will often suffice in mild cases.

The next table refers to a complaint common in men also, but in my experience vastly more common in women. I refer to hæmorrhoids, and so I give it a separate table :—

TABLE *g*.—HÆMORRHOIDS.

			Benefited
Treated by *c* alone ...	6	...	6

In few of these cases was a return necessary. They were all severe, and some of them would certainly have required operation. The result of this mode of treatment in my hands has been so good, even in the worst cases, that I would not advise operation, even in apparently hopeless ones, till high-frequency applications had been tried. In one of the cases zinc electrolysis was required for a fissure of the anus, two applications sufficing to effect a cure. The improvement in the general health due to the treatment is often most marked.

The next affection on the list is constipation. In no cases in all my practice have I seen this affection so obstinate in men as in some of these I have treated electrically in women. In two of them the constipation was absolute. Aperients had no effect, enemata had no effect; only by resorting to mechanical measures could the rectum be emptied.

TABLE *h*.—CONSTIPATION.

		Benefited
Treated by static induced current ...	6 ...	6

The result of the treatment in these cases was a revelation to some of the patients. In one of them, the worst I have ever known, the complaint had followed an operation for piles. In a letter received from this patient ten months after the cessation of the treatment, she says, "I can honestly say I never felt better than I do now. Do you think it would be wise for me to get married?" Some additional cases of severe constipation, which I am not recording in this table, were also treated by the static induced current, but only for a few times. In these cases the improvement derived was in proportion to the number of applications made. From six to twelve applications generally suffice, and I endeavour to make the intervals of muscular contraction eighty or ninety per minute. A metallic electrode is inserted into the rectum and a clay pad placed on the abdomen, the length of the spark-gap being about half a centimetre. The rhythmic contractions of the levator ani can be seen and felt, and their force can be regulated by the length of the spark-gap, whilst the relative force of the rectal and abdominal contractions can be altered to suit the patients' sensations by varying the amount of the clay electrode which is allowed to rest on the abdomen.

In the next table I have placed all cases of indefinite sensations of the pelvis, including pain unaccountable unless from a neuritis. These abnormal sensations—neuroses of the pelvis—may be defined by the patient as "a bearing-down sensation," "a fullness," "a feeling of looseness," "a wearied sensation," "an aching feeling at the vulva," &c. Two were cases of severe orgasms, causing great distress and consequent nerve prostration. In the cases of neuritis, there was generally a tenderness on gentle pressure over the whole of the pelvic walls and over the bodies of the lumbar vertebræ. I believe that in these cases the cause is mainly a constitutional one. Probably the affections in this table test the value of electrotherapy more than any other complaints. They are generally in no way benefited by any other

form of treatment. As a rule, no sufficient cause can be found to account for them.

TABLE *i*.—PELVIC DISTRESS.

			Benefited	Not benefited
Treated by *a* alone	...	4	3	1
,, *b* ,,	...	12	9	3
,, *c* ,,	...	3	2	1
,, *d* ,,	...	1	1	0
,, *a* and *b*	...	3	2	1
,, *d* and *e*	...	1	1	0
,, *f*	...	1	0	1
Totals	...	25	18	7

The proportion here benefited is considerable. In my experience treatment in these cases is usually unsatisfactory. As I have said, there is often present a constitutional neurotic taint. Two of them were epileptics. These, as might have been expected, appear amongst the six unbenefited.

Those belonging to the next group—cystitis—constitute a troublesome class under ordinary modes of treatment. I have not found them so, however, when treated by faradism. My list contains six. One of them was more a case of bacteriuria than of ordinary cystitis. It proved, however, the most obstinate of them all, though it was finally practically, if not absolutely, cured by ionic treatment. Another—Case 127—was more urethritis than cystitis, and it also was practically cured by ionic medication. The remaining four were cases of cystitis, with the usual symptoms—namely, frequent micturition, pain, and pyuria. The treatment in all the cases was effective.

TABLE *j*.—CYSTITIS.

			Benefited
Treated by *b* alone	...	3	3
,, *c* ,,	...	1	1
,, *d* ,,	...	2	2
Totals	...	6	6

In Case 137 there was great nervousness due to pain and want of sleep, and the urethra especially was exquisitely tender; so that it was with difficulty the bipolar electrode could be introduced. The treatment, however, at once began to tell. After three applications of the faradic current, the electrode could be passed in without pain, ordinary diet could be taken, and the urine became perfectly clear. Ten applications in all were given; and what impressed the lady most was

her increase in strength and her difficulty in keeping awake. The current evidently acts as a nerve sedative and a muscular tonic. It is very soothing; but only when from a fairly large secondary coil and with rapid vibrations.

Cases of irritability of the bladder are usually difficult to cure, though dilation of the urethra sometimes suffices. The condition is fully described by Matthews Duncan in his classical work on "Clinical Lectures on Diseases of Women." He calls it "The Irritable Bladder." Its diagnosis is made when the following conditions exist: frequent micturition, pain, normal urine, bladder walls painless and elastic when the sound is passed. The employment of faradism in these cases gives most satisfactory results, as seen in the following table:—

TABLE *k*.—IRRITABILITY OF THE BLADDER.

			Benefited
Treated by *b* alone	... 3	...	3

Cases of incontinence of urine are merely disagreeable, but they are sufficiently annoying to make the patient grateful for their cure. My list contains five of these. They were all treated by the primary faradic current and were all benefited. All of them occurred in married women. In three of them the cause was evidently due to parturition; in the remaining two it had followed chronic cystitis:—

TABLE *l*.—INCONTINENCE OF URINE.

			Benefited
Treated by primary faradic current	... 5	...	5

In these cases the urine would escape when the patient was hurrying, golfing, laughing, lifting a weight, &c.; so that the ability to dance "with no discomfort whatever," as one of them expressed it at the close of the treatment, must have been a delightful change.

I have treated two cases of nocturnal incontinence only thus; one by the faradic current alone, but in this instance with no benefit. The girl was mentally defective, and the trouble had lasted nearly all her life. Faradism was applied locally by me, and for a long time the gavanic current was applied to the spine by the girl's mother at home. As might have been expected, however, no benefit resulted. The other case was practically cured by applications of faradism to the spine, followed by some monopolar high-frequency treatment to the sacral and hypogastric regions.

Several cases of sterility are recorded in the list; but, for this,

treatment was mainly directed towards the removal of all existing morbid conditions and improving the tone of the pelvic organs. When this was successful, pregnancy frequently followed; but I have little, if anything, to say as to what effect electricity may have upon sterility *per se*. Of course the cause may be irremovable. As bearing on this matter of sterility, I may relate a recent interesting case, No. 134. The lady came to me in May of last year. She suffered from toxæmia due to septic endometritis, and complained of general weakness and pelvic pain, but especially of sterility. She was aged 29, and her previous confinement had been three years before. She came to me with the expectation that I would curette her, but I asked her to permit me to treat her without operation. After eleven applications of ionic medication her general health was completely restored, and she had lost her pelvic distress, whilst the uterus and cervix had become perfectly aseptic. She returned a few months after still perfectly well in every way. I then administered on five occasions intra-uterine and monopolar high-frequency treatment as a uterine tonic, after which I dismissed her, stating that she required no further treatment. Six weeks later she became pregnant. Writing to inform me of this, she adds: "I am very well indeed, and have felt every so much better, ever since the first course of electricity I had."

You will have observed that some of the cases had several forms of electric treatment; but if, at your leisure, you glance over the complete list, you will notice that, for some time, treatment was largely confined to high-frequency applications, and afterwards mainly to electro-chemical methods. This was only natural, since new methods excite more enthusiasm and are usually employed on all possible occasions, in order that one may test by knowledge and experience the theory upon which the employment of the treatment is based, and thus be able to correctly estimate their value and judge for what cases they are best suited.

This tendency to rush anything new had once an amusing result in the practice of an eminent Scottish gynæcologist of a generation ago. During a busy afternoon on one occasion the prescriptions of two of the cases became mixed, Mrs. Smith getting Mrs. Thomson's prescription and Mrs. Thomson getting Mrs. Smith's. One of these ladies on discovering the mistake hurried back in great distress, and, handing her prescription to the man-servant, pointed out the error. He looked at it, told her it was all right, she was just to take what was written on her prescription; adding, with a smile, "They're a' getting that the day."

As an illustration of a case requiring more than one form of electric energy in the course of the treatment, let me relate a recent one. This lady complained that, since her confinement, "the womb came out of the passage" as she expressed it, there was a general sense of pelvic discomfort, the menstrual periods were too frequent, and the flow was excessive; whilst there was a constant discharge of yellow matter between the menstrual periods. On making an examination I found the following: The anterior wall of the vagina was prolapsed, the uterus was enlarged and its cavity was dilated throughout, the cervix was gaping, and the os was excoriated. She had at first vaginal and intra-uterine ionic treatment, then vagino-hypogastric static-induced applications, and finally—since, though the discharge had ceased to be yellow, there was an excess of mucus from the vagina—some intra-uterine monopolar high-frequency applications. Under the last form of treatment—the high frequency—the mucous discharge from the vagina became only a quarter of what it had previously been. Throughout the whole of the treatment no douching was done, no extra rest ordered, and, with the exception of an occasional pellet of styptol on account of some bleeding following the intra-uterine ionic medications, no drugs were ordered or taken. Eight months after the close of the treatment she reported to me as follows: "I am still very well, feel quite firm, and have practically no discharge of any sort." He husband added, "She is practically a new woman."

I should have liked to discuss the methods of application of the electricity under different conditions. I have no time for this, however, on the present occasion, nor for any reference to the physiological *modus operandi* of the treatment. This latter is largely theory. I have confined myself to stating facts, and I thank you for your patience in listening to them.

I cannot, however, bring my remarks to a close without emphasizing the importance of general electrization in cases accompanied with low vitality, since whatever tends to improve the general tone of the patient is, of course, a great help in curing her of disease. Now, that general electrization can do this, when drugs, rest, change of air, &c., have failed, I feel as sure as I do of anything in medical treatment; and the high-frequency mode of doing this is both effective and convenient. It must, however, be employed in suitable cases only, and with the skill which is obtained from experience.

Having been requested by several members of the Section to supplement my address by giving some details as to technique, I trust the following remarks on this subject will prove satisfactory :—

Regarding the unidirectional or "galvanic" current, as it is commonly called by electro-therapeutists, I obtain my supply from a battery of thirty to forty Leclanché cells, which gives a voltage of over 50. I prefer this to a shunt current off the main, since the voltage of this in my house is 250, and accidents might happen with such a voltage. For intra-uterine electrolysis I almost invariably apply the positive pole internally, the electrode being the usual platinum one recommended by Apostoli, the vaginal portion of which is insulated by means of a vulcanite tube. At first 50 ma. is an ample dose. More than this may be dangerous as a start. In the case with fibrosis uteri, No. 148, at the close of a seven minutes' application of this strength the patient became alarmingly faint, probably in part due to the fatigue of the preliminary examination. This was her first visit, and she was, from long-continued bleeding, very weak. I have given as high a dose as 200 ma., but only in my early practice. In one case—No. 25—when a current of 95 ma. had been applied for three minutes the patient had an epileptic fit. I then learned that she had been subject to such attacks. When applying this form of treatment for parametritis I wrap a small round leather-covered carbon electrode with a large mass of cotton wool soaked with warm saline solution. This is inserted by means of a perineal retractor, and is placed against the part requiring treatment. In this manner I find a dose of from 50 ma. to 120 ma. can be borne with no discomfort and with no damage to the wall of the vagina against which the electrode is placed. This is due to the fact that the surface of the electrode is proportioned to the quantity of the current. As in the application of the intra-uterine electrode, the metal stem of the carbon electrode is covered by a vulcanite tube to protect the vagina from the bare metal. The negative electrode is usually placed over the abdomen, and for currents of over 100 ma. requires to have a surface of about 50 sq. in. The one used by me is made of sculptor's clay spread on copper gauze and covered in by one or two folds of lint. It can be kept warm for an afternoon by being placed on the top of a closed copper pan filled with boiling water. It must be kept constantly moist or it will crack and lose its flexibility, besides ceasing to act as a conductor.

For ionic medication the same voltage is used, but I rarely exceed 20 ma., and for such doses one or both hands can be placed on the clay electrode, thus rendering unnecessary the undressing of the patient. Of

course rings should be removed. If the speculum is a small one 10 ma. to 15 ma. is as large a dose as the cervix can bear without suffering on the part of the patient; and where, in my haste to obtain rapid results, I have applied larger doses, I have found at the next visit that around the os there had been some superficial necrosis, causing a copious amount of muco-pus. This discharge, however, is in great part aseptic, having arisen from an electro-chemical destruction of tissue. At the next visit there will probably be found pure mucus only. With the solid metal electrode inserted into the cervix, 20 ma. is an average dose; and when the electrode is a copper one, three or four minutes will be required with a reversal of the current in order to loosen the electrode, first bringing the current slowly back to zero, and then, as when first applying it, gradually bringing it up to its maximum by any convenient and reliable rheostat. A zinc electrode requires no reversing. I have used the latter metal mercurialized, but if this is done more than once and there is much difficulty in introducing the electrode past the internal os, the mercury may render the electrode dangerously brittle. Before electric applications to the interior of the uterus, the vagina and cervix must be cleansed either by a douche or a dry cleaning through the speculum. The strength of the solution I use is 1 per cent. of Cu Cl_2 or the same of Zn Cl_2. When there is an erosion of the cervix, and the copper solution is used, there will be found at the end of the séance a green deposit of oxychloride of copper covering the eroded part, and difficult to remove by means of a cotton-covered probe, should an attempt be made to do so. As the erosion heals, however, this is at first easily removed by the probe, and finally ceases to form, except just at the edge of the os, when the erosion has completely healed. When the galvanic current is employed either for electrolysis or for interpolar action the sensations of the patient serve as a useful guide as to dose and time. Regarding the latter, I find from seven to fifteen minutes the average required—the former for initial intra-uterine doses, and the latter for ordinary ionic applications.

In the case of the alternating current, the faradic form of this is obtained from a secondary sledge coil, having 8,000 to 9,000 turns of fine wire and capable of being tapped for a varying number of windings, the larger number for sedative and the smaller for stimulative purposes. The break is the usual vibrating hammer type. The current is measured by my faradimeter, which I consider as essential in the use of this current as a galvanometer is admitted to be in the case of the galvanic. An illustration of this instrument will be found at p. 68 of Dr. Lewis Jones's

"Medical Electricity," fourth edition (1906); whilst the construction of the instrument and the principles of its calibration are described in *The Transactions of the Glasgow Philosophical Society*, 1897-98. The average current for internal applications and for bladder cases is from 3 ma. to 5 ma., whilst when used with the double vaginal electrode a current of 10 ma. or more may be applied. When applied monopolar to any special part of the vagina, the carbon electrode I have referred to can be used. These currents are for the more sedative form; if, from a small number of turns, as a stimulant, less current must be used, or irritation and an increase of the pain will result. Thus harm or good may be done by these currents from the secondary coil. The primary induced current may be applied monopolar or bipolar to the interior of the uterus or of the bladder. The impulses should be from 50 to 100 per minute, and the amount of current as measured by the faradimeter—not by the galvanometer—should vary from 1 ma. to 10 ma. A small amount of current only can be tolerated if the application is vagino-abdominal, or if the electrode is introduced into the urethra only. If bipolar intra-uterine, or bipolar and well into the cavity of the bladder, then a larger current can be borne. The amount of the current can be increased without distress if the rate of impulse is increased.

The sinusoidal current used by me is taken from a magneto-electric apparatus driven by means of an electric motor. I sometimes employ it as a substitute for the faradic in dorso-abdominal applications, using two clay pads for the purpose with a quantity of current of 3 ma. to 8 ma., as measured by my faradimeter. This I find an admirable tonic, acting, as I believe it does, on the abdominal sympathetic nervous system.

In the cases recorded by me as treated by high-frequency currents I have employed the auto-condensation method occasionally as a general tonic. Locally this current has been used by means of the effluve connected directly with the top of the solenoid, with the movable contact attached to the highest ring of the lower solenoid. When the vacuum electrode is required it is also connected directly to the top of the solenoid, but in this case the movable contact is attached to the sixth or seventh ring of the lower solenoid. In each case the lowest part of the solenoid is connected to the condenser plate under the couch. A conical vacuum electrode serves for the vagina and for the rectum, as in cases of pruritus. When employing the monopolar form the arrangement is as in the case of the effluve; but instead of a brush discharge there is a metal contact, my own vaginal one, Apostoli's intra-uterine, or my special rectal electrode, as the case may be; the dose varying from

100 ma. to 250 ma. and the time about fifteen minutes. The break from which I obtain most satisfaction is the Gaiffe mercury jet interrupter.

The "static induced current" is what is known as the Morton current. When speaking of this current in the treatment of constipation I have given sufficient details of my method. The duration of the application in this case also is fifteen minutes. The Leyden jars I use are the pint size, and, in order to keep down the rate of the muscular contractions to about 80 or 90 per minute, it may be necessary, if the machine is in good working order, to permit only about half the number of plates to revolve.

Electro-Therapeutical Section.

November 19, 1909.

Dr. SAMUEL SLOAN, President of the Section, in the Chair.

The Treatment of Nævi by Electricity, Cautery, and Refrigeration.

By EDWARD REGINALD MORTON, M.D.

AFTER the most excellent paper we had towards the end of last session on the treatment of nævi from Dr. Lewis Jones, it must seem to most of you here present that it is an impertinence for me to come forward at this stage with a treatise on the same subject. I can assure you I feel this myself—probably more than any of you feel it—and I would not do so were it not for the fact that I intend to bring before you a method of treatment which is, in this country, more or less novel and not without value. As this method is not an electrical one, perhaps some apology is necessary for bringing it forward in this, the Electrical Section of the Royal Society of Medicine. At the same time it involves the use of a physical agent, and I think you will agree that no Section has a stronger claim to the place for advancing the cause of physical therapeutics than this.

Ever since I took charge of the Electrical Department of the London Hospital the treatment of nævi has been a very important feature of the work, and the number of cases to be treated has steadily and rapidly increased. In a period of six years some 2,000 cases have been treated by myself, and during that time the methods employed were electrolysis, puncture with a fine galvano-cautery, and high-frequency spark. In one case involving the left mamma of a female child, I used pieces of sterilized magnesium ribbon, plunged into the growth and left there. I hoped by this method to avoid destroying the

mammary gland, and, so far as one could judge, the result was satisfactory; but of course it is impossible to be sure of this until the child reaches puberty. I am convinced this means of treating nævoid growths has a definite value, but I am also of the opinion that for routine hospital practice it is not suitable.

Electrolysis.

While I used this agent very freely at first, latterly I have reserved it for the treatment of the cavernous type of nævus, for which I find it the most satisfactory of all that have been proposed. While excision is an ideal method from a theoretical point of view, yet I am sure even surgeons will admit that it is frequently followed by recurrence—at least I often have cases referred to me for electrolysis, in which this has followed excision of the original growth. Possibly surgeons have had to excise a recurrence following electrolysis by myself, but, if so, they have been polite enough not to tell me of it.

In this, as indeed in all methods of treating nævi, the secret of success is to know when to stop. Rather do less than enough, and finish on a subsequent occasion, than risk an unsightly scar through endeavouring to make one operation do all that may be necessary. It is, no doubt, good policy—even advisable under certain circumstances—to make one operation suffice; but when this is done, even with the greatest care, there is no little risk of the resulting scar being less elastic and more noticeable. On the other hand, by adopting a less vigorous plan of campaign and giving full time between the operations for the effects of each to be developed, I have had the most satisfactory results in cases of such severity that the outlook seemed wellnigh hopeless. One case in particular: A deep, cavernous nævus, involving the left side of the face from orbit to chin, was treated by electrolysis, piecemeal, for nearly two years, during which period the child was anæsthetized no fewer than sixteen times. The result was better than I dared to expect. The face was very nearly symmetrical; and while the affected side was mostly scar-tissue, it was soft, pliable, and elastic, and every trace of nævoid tissue gone so far as one could tell. For this method of treatment I always use the bipolar needle designed by Dr. Lewis Jones. Mine has fairly stout platinum needles, which are heated to redness in the Bunsen flame before and after each insertion. They are allowed to cool before being inserted, except when dealing with scar-tissue, which is usually very tough and resistant.

Here much time is saved if they are pushed in while still quite hot. I have never seen any bad results from this, and what would otherwise be a matter of considerable difficulty becomes quite easy.

I generally use a potential of ten volts—corresponding to six to eight Leclanché cells—and this is turned on to the needles before being placed in position, the skin itself forming an excellent rheostat.

Cautery Puncture.

The majority of cases met with in my experience can be satisfactorily dealt with by means of a fine galvano-cautery. It has the merits of efficiency, simplicity, and economy of time. Its chief value is in the treatment of capillary and stellate nævi, even cavernous nævi so long as they are small in size and do not extend too deeply. Here, again, I strongly advise against attempting to do too much at one sitting. Each puncture will give rise to an approximately circular area of inflammation—of which the puncture is the centre—and this may be from $\frac{1}{8}$ in. to $\frac{1}{4}$ in. in diameter. Obviously, to get the best result, our aim must be to destroy as little normal tissue as possible. All we require is to set up a simple fibro-plastic inflammation which will cause a clotting of the blood in the nævoid vessels, and these in turn will become obliterated by the resulting new-formed fibrous tissue and its subsequent contraction.

In treating a capillary nævus with the fine cautery, I first ascertain by inquiry, as well as from the appearance of the edge, if the growth is spreading, and, if it is, my first efforts are directed towards preventing this progressive increase of size. This I do by making a series of punctures about $\frac{1}{8}$ in. apart along the nævo-cutaneous margin, so that the influence is exerted as much in the healthy skin as in the nævus itself. If the nævus is a small one—say, less than $\frac{3}{4}$ in. in diameter—probably nothing more will be required, and except in the large ones this is a very convenient point to stop for the first application. The growth is safe against spreading, and it sometimes happens that the vessels feeding it are so seriously interfered with by what has been done that after a period of two or four weeks the growth has undergone a reduction in size as well as in depth of colour. In such a case it is obvious that there will be less to do to finish it off, and in this way we conform to the principle of destroying as little of the original tissue as possible. With the larger capillary nævi, it is, of course, advisable to go over the surface as well as doing the margin. The punctures should be made from $\frac{1}{8}$ in. to

¼ in. apart, and should, in my opinion, go right through the whole thickness of the skin. In nearly all cases the cautery should be quickly withdrawn the moment the sense of resistance which the skin gives is lost. This prevents the action of the cautery extending too far. My reasons for insisting on going right through the skin are two. In the first place, it is otherwise impossible to be sure that the nævus will be affected through its entire thickness; and secondly, because it is much less painful afterwards. You can easily understand that if you are making a lot of punctures into, but not through the skin, a number of them will be burns of the third degree, which are intensely painful, as you well know.

After a nævus has been treated in this way, the immediate result is a pink scar with a slightly pitted surface, the pits corresponding to the individual punctures. As time goes on, the surface becomes paler and more uniformly smooth. In the case of children seen about twelve months afterwards, the area is smooth, soft, and elastic; and, as regards colour, scarcely to be distinguished from the surrounding skin. The pitting left by the punctures is still present, but not very noticeable. It tends to become less perceptible as time goes on, but slight traces can be found years afterwards.

The High-frequency Spark.

The class of nævi which is most favourably influenced by this method is that comprising what is generally called the "port-wine stain." This is another way of saying that it is only applicable where the growth involves the superficial layers of the skin. The method of procedure is akin to that described as "fulguration," as practised on the Continent for malignant disease, but any ordinary high-frequency apparatus is sufficiently powerful for our purpose. I have now treated about twenty cases by this method, some of which have had several applications. The milder cases have been cured to all intents and purposes; the more severe ones have been improved to an extent which is, roughly, inversely proportional to the severity of the original lesion. The method of making the application is familiar to everyone here, and so need not detain us. From my own experience I am of the opinion that the narrower the spark-gap and the closer the metal-point electrode to the surface of the skin, the more superficial is the effect produced, the more does the thermic effect predominate over the electrical effect, and the changes are produced more rapidly. I am also of the opinion

that under such conditions we are more frequently troubled with keloid changes in the scar. This I attribute to the thermic effect, because I feel sure that the scars following heat-burns are more prone to keloid changes than those from other kinds of injury. Probably some will disagree with me in this. It is merely a record of my own personal observations. I find, however, that any high-frequency application made with the electrode close to the skin surface, and long enough to produce at least some destruction of tissue, is very likely to be followed by keloid changes. The minimum distance of the electrode from the skin should be about $\frac{3}{4}$ in. It takes a longer time to get the desired reaction; but the effect penetrates more deeply, and is less likely to be followed by keloid changes in the scar. With such an interval between the electrode and the skin the application is so painful that a general anæsthetic is almost always necessary, and the troubles of the anæsthetist are not made lighter by the objectionable noise which is an unavoidable part of the procedure. Ether must not be used on account of the risk of its vapour catching fire from the spark. This has happened once, to my personal knowledge, owing to an assistant accidentally filling the usual chloroform bottle with ether.

While I consider the high-frequency spark a valuable means of treating port-wine stains, I have almost entirely discarded it for the new method which I now propose to show you. As a result of my experience with the foregoing methods I look upon no case as too severe to make some serious effort, especially in female children, to remove an unsightly birth-mark. Practically every case can be vastly improved, while the less severe ones can be cured to all intents and purposes. With care, and not attempting to do too much at one time, unsightly scarring is avoided and deformity from cicatricial contraction kept within the narrowest possible limits.

Refrigeration.

The methods so far described have been those employed by me almost exclusively up to midsummer of the present year. At that time Dr. A. C. Geyser, of New York, called upon me and asked me to try a method employed by him for nævi and various other superficial conditions which in his hands had been remarkably successful. This I put into practice forthwith, and the method is so free from the objections common to most others, and the results so satisfactory, that I now treat more cases by it than by all the other methods taken together. The

process is one of refrigeration, and is carried out by means of the local application of a solid crayon of carbon dioxide. This gas is obtainable in iron cylinders containing some 20 lb. to 30 lb. of the liquefied gas at a pressure of over 20 atmospheres. It is obtained from the combustion of coke, and finds its chief use in the mineral-water industry. When the liquefied gas is allowed to escape into the atmosphere the sudden expansion causes a prodigious lowering of its temperature, and it appears in the form of a white semi-solid snow. By collecting this in a suitable receptacle it can be formed at once into a comparatively hard stick, not unlike an ordinary candle in appearance. It can also be compressed into a mould of any desired shape and size, and subsequently shaped at will with a knife. The temperature of the frozen gas is −79° C. (−110° F.). Of course, making use of low temperatures in treatment is by no means new. On the Continent cases of lupus vulgaris have been successfully treated by ethyl-chloride spray. Later, liquid air was tried, and I understand that a few months ago a demonstration was given in this room on the use of liquid air in dermatology by the late Dr. Radcliffe Crocker, whose recent demise we all lament. While liquid air possesses the quality of intense cold to a degree far surpassing anything else we know of, it is extremely difficult, even impossible, to use it in an efficient manner. Its liquid form is responsible for this, making it difficult to control and impossible to make use of that most important adjunct—pressure. Whatever be the agent we employ for the freezing process, the principle involved is of course the same; but that is not to say that the different agents are equally advantageous in practical use. The point I wish to emphasize about solid carbon dioxide is that it is a solid, and yet sufficiently soft to be moulded or cut to any convenient size and shape. Also because it is a solid it enables us to use as much pressure as we wish, and thus freeze the tissues to any desired depth, within reasonable limits. A further point to be remembered is that our freezing agent maintains a constant temperature. As the tissues give up their heat to the crayon the surface of the latter becomes gaseous and escapes into the atmosphere. Carbon dioxide at −79° C. is a solid; at −78° C. it is a gas at ordinary atmospheric pressure. The surface in contact with the part under treatment is thus being constantly renewed. To say that the crayon is "in contact" is probably not literally correct. It is more likely that they are kept apart by a thin layer of gas. For this reason it is possible to hold a small piece loosely in the hand without discomfort. Also, if a flat disc of solid CO_2 be placed upon a level sheet of plate-glass slightly warmed, the disk can be seen to be in constant motion, and if one end of the glass

be raised the disk slides downhill with a rapidity out of all proportion to the degree of inclination. It will be seen that some pressure is necessary to get any effect at all, but owing to its rigidity we can make as much pressure as may be necessary. It is this matter of pressure that gives CO_2 such a great advantage over other refrigerants. The solid gas evaporates very slowly. A crayon 5 in. long and 1 in. in diameter will last from one to two hours if left exposed in an ordinary room. Wrapped in cotton-wool, it loses about half its weight in a period of four hours. With such a crayon I have made over thirty applications, a piece about $1\frac{1}{2}$ in. by $\frac{1}{2}$ in. remaining.

Dosage.

As with any other new agent, so with this—we have much to learn. There is no reason, however, why the question of dosage cannot be reduced to mathematical accuracy here. As we have seen, the temperature of our crayon is a constant one. The only two variable factors in dosage are time and pressure. There is no difficulty about the measurement of time, and a holder with a spring inside which is adjustable would solve the latter point. I doubt, however, if this will be necessary, but it is easy to see how the depth and intensity of the subsequent reaction could be regulated with almost absolute certainty. I am always averse to the use of any appliances that tend to efface the personal equation entirely, and in this case no very great experience is required to make the necessary degree of pressure oneself.

To come to practical details. Take the case of an ordinary capillary nævus of moderate size—less than 1 in. across in any direction. For this I trim the crayon roughly to the shape of the nævus, and I prefer it to be slightly larger.

It is now applied to the nævus and firmly pressed down so as to drive the blood out of the part, or at least arrest the circulation. The average duration of the application will be about forty seconds. If there is bone immediately beneath, a shorter application will do. If on the thigh or abdomen, it should be a little longer. A "port-wine stain" is best treated with a moderate degree of pressure, depending on the depth of the lesion. For a cavernous nævus the end of the stick may be the same size as, or slightly smaller than, the area of the growth, and a long application with deep pressure should effectually freeze the whole mass. In any given case the time and pressure of the application will be decided by the conditions present.

The diseases curable by this agent are numerous, and I fancy will be of great interest to dermatologists. As I have said, I now use it for nævi more often than all the other methods taken together. It is very successful for moles and other blemishes; it seems to have a favourable influence on lupus erythematosus, and I have successfully cured patches of lupus vulgaris; it answers excellently for warts. In the treatment of the latter, a long application is necessary on account of the poorly-conducting properties of the growth. I do not time the application. After trimming the stick to the size of the growth, I apply it with firm pressure until a narrow zone of healthy tissue around the base is frozen. If we stop before this, the result will not be satisfactory. In the keratosis accompanying chronic X-ray dermatitis, brief applications answer exceedingly well. The list of diseases to which this agent is applicable will probably be extended very greatly; no doubt within the next year or so dermatologists will have much to tell us in this respect.

The Preparation of the Solid CO_2.

The first requirement is obviously a cylinder of the compressed and liquefied gas; a large cylinder, such as I have here, is much the more satisfactory. This should be mounted on a stand at a convenient height, and with the stop-cock on a lower level than the opposite end, so that the liquid gas covers the inner orifice of the valve. The next procedure will depend on whether we have a large or a small number of applications to make. In any case, we require an ordinary cylindrical ruler about 1 in. in diameter and a hand-towel. The latter is folded into three lengthwise and rolled round the ruler as tightly as possible. The ruler being withdrawn, we have a porous, hollow cylinder, in one end of which we place a short, loosely-fitting cork. If we have a large number of applications to make, we take an ordinary bandage, bind round the towel-cylinder a few turns, and apply its open end over the cylinder outlet. The bandaging is now continued in such a way as to secure the towel to the valve and to make as tight a joint round the latter as is possible with bandaging. I find two ordinary bandages sufficient. The cylinder is now turned so that the valve is pointing downwards; the valve is then opened, allowing the space within the towel to become filled with the soft CO_2 snow, which later becomes solidified, and this is continued until it is packed firmly. The fixing of the towel and the management of the valve require a certain amount of practice, but it is the easiest

and best method of getting a large, firm crayon. After the valve is closed the bandages are removed, the towel unrolled, and the crayon secured with a turn or two of lint. It is now ready for use. If we require enough for one or two applications only, instead of binding the towel to the valve, we hold it in place with one hand while we open the valve with the other. The towel now becomes filled with the soft snow, and we cut off the gas as soon as it begins to blow out round the tap. We now place this snow in a metal or vulcanite tube of the desired size and shape and pack it down hard with a suitable rammer, using a mallet if a particularly hard stick is wanted. No matter how hard we pack it, it at once frees itself and can be pushed out quite easily. The tubes for moulding can be of any shape or size; a square one is used for doing large areas without leaving intervening spaces untreated, as would be the case if a round applicator were used.

Effects of the Application.

On removing the crayon after an application the depression made by it remains: the frozen surface is pure white and hard, like a piece of china. The process of thawing occupies about the same length of time as the application, and the depressed tissue gradually returns to its normal place and colour and is found to be covered with moisture of condensation. Reaction sets in immediately. Within a few seconds the treated area is of an appreciably firmer, brawny consistency, and in two or three minutes perceptibly swollen. A wheal develops, with acute hyperæmia, within half an hour, and a vesicle frequently within an hour; but this may take longer, or not develop at all in the case of a very short application. An application of thirty seconds or longer will almost certainly be followed by scarring. A very intense superficial destruction is obtained by a second application immediately after the tissues have thawed out from the first one.

With regard to after-treatment, this is very simple. Boracic-acid ointment is what I use almost invariably. If a blister forms the fluid is carefully let out, and this may have to be done a second, or even a third, time. Within a few days a crust forms, which should be allowed to come off of its own accord. The treated area is completely recovered in about a fortnight. The surface is then perfectly smooth and of a delicate pink colour, which gets paler as time goes on. The ultimate result is a pale, soft, pliable and elastic scar, which is as good as any produced by any other caustic agent, or even radium.

Like radium and unlike the cautery or electrolysis, there is no immediate destruction of tissue. The results are achieved by a reaction set up in the tissue itself, and we have a complete range of effects from a simple, mild hyperæmia, which clears up in a few hours, to a complete necrosis. Unlike radium, it is cheap, and instead of waiting days or weeks for a reaction, we get this within a few hours. I predict that it will displace radium for many of the purposes for which the latter is now used. I show some results obtained. In a photograph they do not differ greatly from those treated by other methods, and no good purpose would be served by showing a number; these are typical of the results.

In the majority of cases a single application, lasting from thirty to sixty seconds, is all that is necessary. If the cure is incomplete, a second one can be made as easily as the first. The application is practically painless and no anæsthetic is ever necessary. I have had children who never moved or cried during the application. The majority do make a noise, but this is more because they are held still than because of the pain. It is a curious fact that many adults voluntarily say "it is beginning to hurt now" at about the twenty-fifth second. The thawing out is more uncomfortable than the freezing.

With regard to time, two or three cases can be finished off while one is being anæsthetized for electrolysis or cautery; I have treated thirty cases within an hour and a half. I have not yet seen any keloid in the resulting scars, nor have I had an opportunity of examining the changes that take place in the frozen tissue. It is quite possible that at this very low temperature changes other than those following ordinary frost-bite would be found. Ordinary frost-bite, lasting no more than sixty seconds, does not produce anything like the same degree of reaction we get with solid CO_2. With the fixation and rigidity which occur just below freezing-point, followed by the expansion of the frozen liquid part of the tissues which must take place with this extreme lowering of temperature, it is conceivable that the cells are mechanically injured —the reticulum broken down and the cell-wall burst—just as a bottle full of water is fractured after being left out on a frosty night. This would at least account for the immediate and profound reaction that occurs after each application. This, however, is conjecture, and I shall be glad to have the views of others regarding this question.

To say that solid CO_2 is merely a caustic, as some have asserted, is not quite correct. All caustics, strictly so called, destroy tissue immediately—e.g., cautery, chemical substances, &c.—but CO_2 does not do

so. After the part is thawed out, the whole of the treated area is still living, as evidenced by the intense reaction which sets in. Local death may occur later owing to inflammatory stasis, but this is the result of a vital reaction, and not an immediate result of the application of the irritating substance.

In conclusion, I wish to apologize for such a lengthy paper, but I am sure there will be agreement as to the value and importance of this interesting and fascinating substance as a therapeutic agent. Personally, I look upon solid CO_2 as the most important addition to the list of physical agents employed in therapeutics since that of radium, and, as I have already said, I predict that it will be found to compete successfully with radium in the treatment of many superficial lesions.

DISCUSSION.

The PRESIDENT (Dr. Sloan) said he had much pleasure in asking the Section to accord a very hearty vote of thanks to Dr. Morton for his most interesting and able paper, a great deal of which must be new to most members. With regard to the electrical part, he agreed with Dr. Morton's advice that too little should be done rather than too much, especially on the face of a nice-looking girl, where it was a serious matter to produce a permanent blemish. He asked what Dr. Morton's experience was with regard to fulguration of moles—whether he had seen keloid resulting from it? Dr. Morton need not have apologized for having brought forward the paper with regard to the solid carbon dioxide. The Section was the Electro-Therapeutical, but this included physical therapeutics, he thought. If anyone had had experience of the method, or wished to put questions to the lecturer, the meeting would be glad to hear them. He asked Dr. Lewis Jones to give his views in regard to keloid resulting from the high-frequency method, seeing that he had had so much experience of this mode of treatment.

Dr. LEWIS JONES, in answer to the President's question, said that, so far, he had never been troubled by the development of keloid after the treatment of a nævus; and, speaking on the subject of the paper before them, he had made a few experiments with the solid carbon-dioxide method, because Dr. Morton very kindly gave him a private lesson in the art of preparing and using the material in September. The procedure he followed had been identical with that which had been demonstrated on that occasion—i.e., the method of the rolled-up towel. But he was glad to have seen it done again that day, because so far he had not been quite successful in his attempts to form a good solid stick. And almost every time he had tried it the towel had been torn by

the escape of gas; and when the towel tore, as it did, in its inner layer, its cavity lost its proper cylindrical shape, and the solid CO_2 tended to collect in irregular pieces. He had treated several cases of nævus, but it was yet too soon for him to speak of final results. Mr. Moore, the apothecary at St. Bartholomew's, had taken weighings of the cylinder before and after the preparation of the solid CO_2, and the figures were: of gas exploded, 8 oz.; and of cost 1d. Thus, so far as the cost went, the use of solid CO_2 was quite cheap.

Dr. FINZI asked whether Dr. Morton knew anything of the late results of the treatment. He thought it possible there might be some contraction of the scar-tissue afterwards, which might spoil the excellent early results.

Dr. HORACE MANDERS expressed his gratitude for the paper, which had created quite an enthusiasm in the Section. The method had been used in America for some time past. He first read of it in January, 1909, in the German *Centralblatt für Chirurgie*, in which Sauerbruch said he had heard of it from Chicago, where two Americans—A. J. and E. Oxner—had used it to destroy nævi and angiomata, as well as lupus and rodent ulcer. But their technique was very inferior to Dr. Morton's. They allow the carbon dioxide to solidify upon cotton wool, and then they press small pieces of that wool on to the nævi. The application was not one of few seconds, but of many. He was surprised to hear Dr. Morton say he found fulguration good only for port-wine stain; he also mentioned something about keloids. He (Dr. Manders) had used fulguration very largely for nævi, but had never yet seen keloid result; still, he knew such were produced, and he knew how. Very great care was wanted in fulguration, and what Dr. Morton said about the closeness of the spark causing thermic action over the electrical had much to do with the matter. Dr. Branth, of New York, had told him that in treating warts or epitheliomata, the moment any moisture was seen one ought to cease, because, if persisted in, keloid would result. That took only about three seconds. In nævi one did not see the moisture. He regarded $\frac{3}{4}$ in. as rather too long a spark; but the frequency of the spark had much to do with the matter. If one wished to destroy growths by fulguration it was better to have a considerable coil between the condensers, and the condensers not so big. The frequency should run into something like three or four millions per second. One advantage which this method had over fulguration or any other electrical treatment was that it was quite painless. He had seen half-a-dozen cases treated by it, and no one seemed to object at all. The fact that at the end of twenty-five seconds the patients said the process began to hurt he regarded as a mental phenomenon. The patient had made up his mind to go through with a certain thing, and at the end of twenty-five seconds he gave out, just as, when having a tooth extracted without gas, if there was delay, the courage of the sufferer oozed out. He supposed the amount of pressure was just enough to squeeze out that layer of gaseous carbon dioxide which was forming so as to keep the actual solid always against the diseased part.

Mr. W. DEANE BUTCHER said that perhaps the Section might be interested in hearing a few words from the patient's point of view. Dr. Morton was good enough to treat a small angioma on his (Mr. Butcher's) eyebrow, and with the very best results. It had already been twice treated by electrolysis, and the process caused considerable pain, as it was near the supra-orbital nerve. The process of freezing was almost painless, but there was a good deal of pain when it was thawing out. The result was quite perfect. A sort of crater was made, which reached down to the bone. The reaction was very considerable and the healing very slow, but there was no trace whatever of a scar. The process should not be undertaken lightly, as one or two cases had already appeared in literature in which a certain amount of sloughing had occurred.

Dr. MCCULLOCH said he had been much edified by the paper, but was surprised to hear that Dr. Morton confined the high-frequency-spark treatment to port-wine stain. Recently he (Dr. McCulloch) had a case of fungating angeioma on the back in a man whom he had treated with the high-frequency spark, with alternations of X-ray irradiations fortnightly, in anticipation of anything like keloid formation, which would thus be prevented. The lesion was about the size of half a crown, but it had now disappeared, and the surface of about the area of a shilling looked like a faint port-wine stain. He hoped to show the case at an early meeting of the Section. The carbon-dioxide treatment seemed to him to be analogous to the first degree of frost-bite, the subsequent necrosis of which was clinically very like that produced in diabetes. He once had 100 cases of frost-bite among people composing a convoy in the Himalayas, and many were bad cases, fingers and toes being lost. That brought one very near the condition of X-ray necrosis. In this case there was a denervation of the tissues—i.e., a destruction of the trophic peripheral nerves. That view was supported by the well-known period of latency preceding the change. Dr. Morton stated that it took three or four days or two or three applications before the visible result of the "crayon" application manifested itself. If this corresponded to a brief period of latency, might it not be that the freezing action on the myelin of the trophic nerves accounted for the change? He desired to recall the observations made by him at a meeting of this Section last year on the histopathology of these conditions.[1]

Dr. G. B. BATTEN thanked Dr. Morton for his valuable paper. He asked Dr. Manders whether the high-frequency he spoke of was with an ordinary resonator or with the one he specialized in, with a constant high-frequency current? He also would like to hear from any dermatologists whether they thought the process would be justifiable in chronic eczemas with thickened skins. Was there danger of setting up acute eczema in the surrounding parts? He knew one patient—a lady—who had a very big area in the popliteal space, which caused considerable pain, and he knew only one method of treatment which seemed likely to do good—namely, X-rays; but it was not always convenient to apply them to that region, and it would be interesting to know whether this carbon-dioxide treatment would be beneficial.

[1] *Proc. Roy. Soc. Med.*, 1909, ii (Electro-Therap. Sect.), p. 28.

Dr. GOFFE asked whether Dr. Morton had used solid CO_2 for the removal of hairs from the face and other parts of the body?

Dr. N. E. ALDRIDGE asked Dr. Morton if he had used carbon dioxide for the treatment of purely subcutaneous nævi, or whether the treatment had been confined to those only in which the skin was involved.

Dr. HORACE MANDERS, in further remarks, said he did not think that anywhere in Europe there was submission to anything like a temperature of -79° C. Messrs. Oxner were wrong about their temperature, as they gave it as -70° C. There was only 1° between solidity and aeration. Dr. Batten's question to him seemed scarcely in the scope of the present discussion. Sufficient time had not yet elapsed to know whether there would be keloid or not. In the little he had seen of it there had not yet been any appearance of keloid. He used the current from two capacities and a selenoid between them, and one tapped from that selenoid on to a resonator. But he used a particular resonator, which was partly a broomstick and partly a garden hose with the iron-wire core taken out. Upon that was wound 110 yards of bell wire, which formed a flexible resonator. It was easy to tune, and the point of discharge could be brought right up to the patient. Only a lead of 6 in. or so was wanted, and therefore the tuning was always correct. If one used a flexible resonator that had a period of its own, one had to tune that; so that as the surges came down again they got another elevation. Once in tune, by bringing the flexible resonator right up to the patient, one was always in tune. But if one had a long lead from the end of a resonator, and there was the slightest spark, the whole thing was altered, and a bad result in the matter of keloid was more likely to occur.

Dr. MORTON, in reply, said he was much gratified by the kindly way in which his effort had been appreciated. He had only had keloid from such cases as were treated by high frequency, and only in such cases where he, either by design or by accident, had allowed the point of the electrode to get too close to the skin. To get through 2,000 cases of nævi in four or five years with only one day a week meant some strenuous days' work; and it had always happened that the fulguration cases had to be done last, and they had to be done in a small room; and with the noise going on, and with the patient under the anæsthetic, one was tempted to let the point go a little closer to the skin, as the reaction was obtained in the skin much more quickly. It was in such cases that he noticed the thermic effect predominating over the electrical, and it was frequently followed by keloid. He had treated twenty cases by high frequency, and keloid had occurred in four. With regard to the preparation of the stick, he tried the scheme of opening the valve freely and letting it blow in quickly, but it was no advantage: one did not get it hard enough, and it filled the cylinder too rapidly; he found that the formation took place from the periphery inwards. But if it were not let in at a certain pace, it would escape by evaporation. There was always a fair amount of waste. It was essential to have waste, as one depended on expansion. That was why a porous towel

was necessary. His colleague at the London Hospital tried a nickel-plated tube fixed up by clamps. But in this arrangement there could be no expansion; it was a case of either blowing the thing to pieces or shutting up the valve, as the pressure was 600 to the square inch. With regard to the late results, he was assured by Dr. Geyser that the results were better than by other methods, as there had been no gross destruction of tissue. By this method one could get just what reaction one required, and there was no necrosis, unless the application extended over too long a time. He had not yet tried the treatment for rodent ulcer: he was waiting until he got a rodent not too near the nose, mouth, or eye. If the ulcer were close to the nose, it was difficult to get pressure on to it evenly. He had not used it for subcutaneous nævus, for the reason which was sufficiently obvious: to affect such nævus enough to cause its obliteration he would be sure to cause sloughing of the overlying skin, and that would be worse than putting in a Lewis-Jones bipolar needle. The same remark applied to the nævi, which were very soft, like a cirsoid aneurism. To meet the wishes of Dr. Abraham at the West London Hospital, he made an application in a case of this kind, but it had no influence at all. He had only used fulguration for port-wine stain with an ordinary high-frequency apparatus, and Dr. McCulloch was surprised that he had not used it for other kinds of nævi. In busy hospital practice one chose the method which gave the best results in a reasonable time, and the majority of cases of nævi he treated with the fine galvano-cautery. The result, especially if too much was not attempted, was splendid. He did not agree with Dr. Manders that the feeling of pain at the end of twenty-five seconds was purely a mental phenomenon, as it occurred at the same time in neurotic and in phlegmatic people, even when the latter were engaged in a conversation. The pressure varied according to the depth of the action. In the case of a capillary or cavernous nævus, there must be sufficient pressure to arrest the circulation, otherwise the deeper parts of the nævus would not be frozen. It was true that no such temperature as the crayon gave was experienced in Europe, but Peary, who claimed to have reached the Pole, found there a temperature of -80° C. In Mr. Butcher's case the lesion was very close to the trunk of the supra-orbital nerve, and then, of course, there was pain. No nerve would stand freezing without resenting it, but in ordinary cases the application was to all intents and purposes a painless one. With regard to the sloughing which sometimes took place after the applications, he had noticed that in the case of a raised capillary nævus it practically always sloughed off. He had had a great experience of frost-bites in Canada, and the main difference between the reaction so produced and that by carbon dioxide was due to the length of time the freezing was continued; also in ordinary frost-bite the tissues did not get reduced to anything like the temperature of carbon dioxide. Probably the temperature was not reduced much below zero C. With such a very low temperature as that now used, there was bound to be a different set of conditions. There might be seen, microscopically, lacerations in the walls of the cells. In the ordinary nævi he had found it necessary to make only one

application. The port-wine stain was essentially superficial; his aim was therefore to make moderate pressure for a long time, and get a very intense but very superficial reaction. In one case, after a first application, there was not much improvement in three or four weeks, but after a second application it was blanched out in a most pleasing manner. The first application seemed to prepare the ground. In chronic eczema he would much sooner apply carbon dioxide than X-rays. He was himself a little dubious about the ultimate effects of X-rays; he believed a time would come when some people would wish they had used them less. The position was different when treating malignant disease, in which case anything was better than the disease. He would prefer not to use X-rays in a case of erythema; some cases returned years afterwards with telangiectases in the most unexpected places. In the case of chronic eczema the treatment now demonstrated ought to be good, as it enabled one to set up a healthy activity in place of a pathological one. He had treated hairy moles by the method, and the hair had disappeared; and where the mole had been above the level of the skin the result was to make it lose its elevated aspect. By applying ethyl-chloride spray one did not have pressure; he doubted whether the tissue was reduced much below the freezing point, and after the superficial part was frozen it would be difficult to freeze the underlying part.

Electro-Therapeutical Section.

December 17, 1909.

Dr. Samuel Sloan, President of the Section, in the Chair.

Radium in the Treatment of Malignant Growths.

By N. S. Finzi, M.B.

The introduction of certain special methods of using radium has provided us with a very powerful agent for the removal of neoplasms —one which will act when other means are of no avail. It is no universal panacea and will not cure every type of malignant disease, but as cases can be cured by it which are inaccessible to any other method —even surgery—it is bound in future to take a very prominent place in the treatment of this terrible complaint. It will not displace surgery in the treatment of many growths, though it will in some, but it will often be employed in conjunction with an operation in order to destroy neoplastic cells which may have escaped the attention of the operator. In fact, I hope some day to show that every operation for cancer should have prophylactic radium treatment as a routine measure. It is as ridiculous to expect to cure every case as it is to suppose that an operation will never be followed by recurrence, but I am confident that we shall later on be able to cure 80 or 90 per cent. of certain types of growth. There is a tendency among some people to condemn the treatment on account of some particular case in which it has failed—possibly a case quite unsuitable. This is most unreasonable, but happens to every new treatment. If, out of a number of cases which were

inoperable, one could cure only 1 per cent., I say one would be justified —nay, more, compelled to use it in every such case, providing it did no harm. But I claim that it will cure a far larger percentage than that, and, further, where it does not cure, that it will often relieve. It must, however, on no account be used indiscriminately, as some growths are not improved, at any rate by the quantities of radium we can use at present, and it is even possible that in some we may do harm.

To start with, it is necessary to say something about the physics, but only the most essential points, for it is a very complicated subject.

Radium is a metal which has never yet been isolated and which is of the same family as calcium, barium, and strontium. The bromide is the salt generally used in medicine. This substance and its transformation products, which I will describe to you in a moment, give off rays that are of three kinds—α, β, and γ—and each of these has very different properties. The α-rays are minute particles, about the size of atoms, which have very little penetrating power and are stopped by a sheet of paper or a few centimetres of air. They are deflected by a magnet, but only in a field which is very intense. The β-rays are very much smaller particles—electrons, charged with negative electricity—which are of very varying penetrative power; some will penetrate little more than the α-rays, while others will go through 2 or 3 mm. of lead; between these are rays of all grades of penetration. These β-rays are strongly deflected by a magnet in the opposite direction to the α-rays. Lastly, there are the γ-rays, which are not particles, but ether vibrations of very extreme penetration, few only being stopped by 5 mm. of lead, a fair proportion going through 2 or 3 cm. They are not deflected by the most intense magnetic field. They have been said to be very penetrating X-rays, but I find that they do not, even after three weeks' exposure, change the colour of a Sabouraud pastille, on which the most penetrating X-rays have an especially energetic action.

Radium itself only gives off α-rays. It is slowly and continuously being transformed into a gas known as the emanation, which also gives off α-rays, and gradually disappears and forms a deposit known as radium A. This too gives off α-rays only and changes into radium B, which gives off slow β-rays, and itself changes into radium C, which gives off all three kinds of rays.

The next product is radium D, which is again inactive. From this we get radium E^1, inactive; next, radium E^2, which gives off β- and probably γ-rays; and, last, radium F, which gives off α-rays only and changes into a rayless product. I show you on the screen a table

of these products and the time each takes to fall is half its original activity[1]:—

	Time to fall to half value	Nature of rays
Radium ...	1300 years	α
Emanation ...	3·75 days	α
Radium A ...	5 minutes	α
,, B ...	26 ,,	Slow β
,, C ...	19 ,,	α, β, and γ
,, D ...	40 years	No rays
,, E^1 ...	6 days	No rays
,, E^2 ...	4·8 ,,	β
,, F ...	143 ,,	α

I shall endeavour to demonstrate that the rays useful in this work are the γ, and perhaps the most penetrating of the β, so that the only products of use to us are radium C, radium E, and possibly radium B. The emanation, by its decomposition, provides all the products one wants, but it would be necessary to collect it continuously for over a month to get the maximum of radium C, for if one stopped collecting it the supply of radium C would diminish and the β- and the γ-rays would consequently gradually decrease in quantity, so that what one does is to seal up the radium itself, its emanation and resulting products, and then, after about a month, the amount of radium C formed is exactly equal to that decomposing, with the result that we get radiations of a constant strength. As radium D, E, and F change very slowly, there is a constant growth of rays from radium E^2 for a period, estimated by Rutherford at about 220 years.

As with light, X-rays and other forms of ether vibration, the strength of the radiations varies inversely as the square of the distance of the source from the object, provided that that source be approximately a point; for example, if we have an object 1 cm. from the radium and another 5 cm., the latter will receive a radiation only one-twenty-fifth the strength of the former. This is a great disadvantage in treating malignant disease, and an attempt is made to counteract it in various ways. If the radium were an infinite distance from the skin, this and the deep parts would be irradiated to an exactly equal extent, but the amount of rays would be infinitely small. However, if one places it at 5 cm. from the skin, the tissues 5 cm. below the surface receive exactly a quarter of the radiation which the surface receives, provided that none is absorbed. We are, however, faced with the difficulty that the intensity is very greatly diminished by this method, and we shall see that this decreases the efficiency. By the use of very much larger quantities of γ-ray producers we shall in future be able to overcome this difficulty.

[1] Makower, "The Radio-active Substances," Lond., 1908.

Owing to the different powers of penetration of the rays you will see that it is possible, by filtering them through metal, to alter the proportions of the different radiations or to suppress the α and the β entirely. A screen of 8 mm. of lead will allow only γ-rays to pass, but the proportion of β-rays passed by 3 or 4 mm. of lead is so infinitesimal that for medical purposes it need not be considered. Filtration through 1 mm. of lead or more will give a great preponderance of γ-rays.

The absorption of the α- and γ-rays is in proportion to the density and thickness of the metal used, and in most cases the same is true for β-rays; but tin and lead are exceptions, lead absorbing nearly double what its density would make one expect, and tin more than double. These metals, then, are particularly suitable for excluding β-rays.

Now, of course, if you filter the rays through, say, 1 mm. of lead, you will only get a small proportion of the original radiation, though its character is very much changed. Very little is absorbed by the skin; nevertheless its action on the skin, if placed near to it, is much more intense than on the deeper parts, owing to the diminution in intensity of the rays in proportion to the square of the distance. However, the filter, by preventing the skin from absorbing a disproportionate amount of rays, allows the time of the application to be much longer, and thus the deep parts obtain a much greater dose than when no filter is used. If left to act long enough these penetrating rays will produce a burn, the first stage of which is an erythema and the second a painless blister, either of which occurs after a latent period of about five weeks' time. The longer the dose the earlier the reaction and the shorter the latent period, but of the more severe reactions with the use of thick filters I know nothing.

It has been proved by using rays through 5 mm. of lead, which filters out almost entirely the β-rays, that the γ-rays have a curative action on certain malignant growths.

Action.

Radium rays exert a selective action on the cells of some tumours, and it is found that this selective action is greatly increased by using lead filters at least 0·5 mm. thick. I believe that by the use of screens 2 or 3 mm. thick, with the consequent longer exposure which is possible, we can cure growths that cannot be cured when thinner screens are used. By selective action I mean that the tumour cells are destroyed while the cells of the normal tissue remain uninjured, and the whole

treatment hinges on this. Different growths vary in the extent of this selective action, but in some it is indeed enormous, and I have seen growths shrink away, parts of which must have received one-twentieth or less of the amount received by the skin or mucous membrane beneath which they lay, and which itself was unharmed.

There is another factor influencing the selective action, and that is the intensity of the radiation which with the same filter can only be increased by using more radium, so that when one has only a limited amount there will be a certain filter which will give the optimum effect, and probably this will vary in thickness for different tumours. Increase in the amount of radium, on the other hand, will increase the efficiency of the action on every tumour, and will bring within one's power those growths which have proved refractory to smaller quantities. This has been definitely proved by Dominici in a tumour which resisted the action of 2 cg. for a long exposure, but yielded when treated with 5 cg. for a considerably shorter period. Another factor is that of distance. The further off the radium is, the less will be the difference between the dose received by the surface and the deep parts of the growth, but then, even though one might prolong the time of the dose considerably, the intensity of radiation might be diminished beyond the point at which it would affect the growth at all. The action of these so-called ultra-penetrating rays is much more selective than that of X-rays, and this considerably enhances their scope. The possibility of introducing tubes containing radium into cavities and into the tissues still further increases their utility. Though the filtered radiations are a depressant of the activity of the cells of almost all malignant growths, there is probably a point, before or long before the full dose has been given, when the action, if stopped, would be a stimulant to their activity, as with most other cell depressants. I have mentioned that in some cases one-twentieth of the full dose will be sufficient to cause regression, but there are other growths which are only affected by half the full dose. It is in these latter where the stimulating effect is to be feared if an insufficient dose is given.

The histological changes one sees from very penetrating rays are firstly an invasion of the growth by leucocytes without any cell change that cannot normally occur but with an accentuation of these changes. In a later stage a large number of the cells have disappeared and their fibrous stroma is left and finally only fibrous tissue remains. The changes, when only 0·5 mm. of silver is used, include degeneration of the cells as well, especially in the superficial parts, and a great

proliferation of the endothelium of the blood-vessels. There seems also to be new formation of fibrous tissue. In some cases the improvement is apparent after forty-eight hours, especially in the relief of pain, but more usually not much alteration in the size of the growth is seen for two or three weeks, and this goes on for six or more weeks altogether.

Apparatus.

I now show you a photograph (fig. 1) of the apparatus I use. It consists, as you see, of a glass tube containing 50 mg. of pure French radium bromide, of a silver tube 0·5 mm. thick, a lead tube 1 mm.

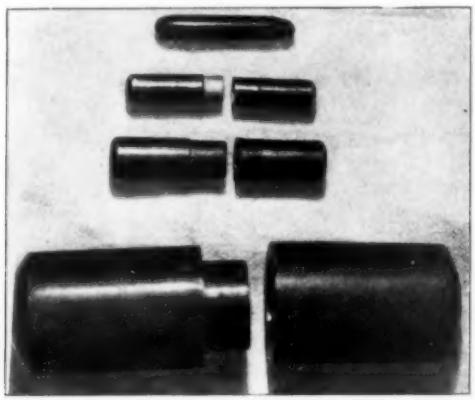


Fig. 1.

thick, and, lastly, of a thick lead case to carry it in and to act as a partial protection. I have also lead tubes 1½ and 2 mm. thick. The radium bromide when shaken to one end of the tube only fills it for about ¼ in. It is better to have the radium in a sealed metal tube of absolutely uniform thickness without any glass tube. The apparatus is then more portable and convenient, and the next apparatus I am getting is in that form. For introduction into cavities the metal tube is tied up inside a rubber tube, and either a long silk thread left at one end to come to the surface (fig. 2), or the rubber is tied at one end only and a bougie introduced at the other.

I show you also some of the tubes used in France, and you will see

that they are thinner and more convenient for use in the tissues and in the œsophagus. They have the disadvantage that their thickness is often not uniform. They should be made of platinum, too, and not of silver, as 0·5 mm. of platinum or lead is sufficient filtration, while 0·5 mm. of silver, which corresponds to 0·3 mm. of lead, is not. I also show you models of radium spread, and this leads me to the question which I am always asked, Why do you use a tube instead of having it spread? My answer is: For dermatological treatment, nævi, lupus, eczema, &c., where an absolutely even action is required over a large surface, use the spread radium. For treating tumours it has no advantage over the tube and has serious disadvantages: for instance, the impossibility of treating cavities, the œsophagus, rectum, vagina, and of introducing it into the tissues. The only advantage—that of acting

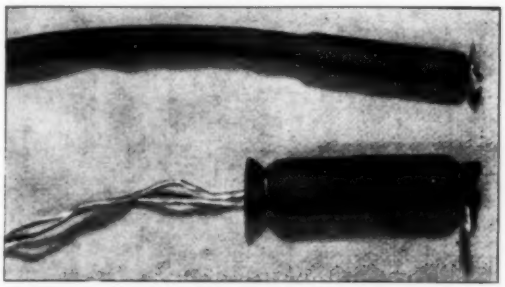


Fig. 2.

over a large area of skin—can be obtained with the tubes by altering their position from time to time.

Filtration was introduced into France by Dr. Wickham, and the use of thick filters for malignant tumours was worked out by Dominici in a most thorough and scientific manner.

Technique.

For External Applications.

The apparatus, in a suitable metal shield, is separated from the skin by a thickness of lint, paper, or indiarubber, to a distance which will, to a certain point, be more the greater the depth of the growth. Supposing

a dose be ten hours with the radium at a certain distance, the dose will be forty hours, approximately, if this distance be doubled, for the radiation falls off as the square of the distance. A further use of the lint, &c., is to cut off secondary radiations of feeble penetration which are formed by the γ-rays striking the metal. I find that the maximum doses of my apparatus on sound skin are as follows if the radium is shaken down to one end :—

Ra. Br.	Silver	Lead	Lint	Indiarubber	Dose
50 mg.	0·5 mm.	—	—	1 mm.	1½-3 hours
50 ,,	0·5 ,,	1·0 mm.	—	2 ,,	12-15 ,,
50 ,,	0·5 ,,	1·0 ,,	8 layers	—	18-20 ,,
50 ,,	0·5 ,,	2·0 ,,	—	2 mm.	17-18 ,,
50 ,,	0·5 ,,	2·0 ,,	8 layers	—	26 hours (about)

These doses in the skin will, in some people, produce a slight erythema in about four or five weeks, in others none, and their action on the mucous membrane is, if anything, slightly greater. Skin containing carcinoma will support a larger dose than healthy skin, even when not ulcerated. In treating a tumour a full dose is given to as many parts of it as one can get skin for, and if one has several tubes one uses them from different sides at the same time, and so, from the crossing of the rays, we get a fair dose in the middle of the tumour. The applications have to be repeated in five or six weeks and one or two prophylactic applications given after the disappearance of the growth. The number of applications depends upon the size of the tumour and the readiness with which it reacts.

Application to Internal Surfaces.

In this case the radium in its silver or lead tube is placed in an india-rubber tube and either tied in at both ends with the silk at one end left long, or else tied in at one end and a bougie or silver wire inserted into the rubber tube which has been left long at the other end (fig. 2). My experience extends to the following cases :—

Nose.—The tube is passed into the anterior nares and the patient placed in such a position that the radium falls towards the growth.

Pharynx.—If the growth is high up, the tube is held by a piece of stiff bent wire at the back of the throat, which has been well cocainized. In these cases a small hypodermic of morphia and atropine is useful to limit secretion. If, however, the growth extends below the level of the epiglottis, the best method of application is through an œsophagoscope

which is left in place, the heavy handle, with the illumination on it, being removed as soon as the radium is in position.

Œsophagus.—My tube is passed down on to the stricture or into it by means of an œsophagoscope. On the methods of doing this, Dr. Hill and I hope shortly to publish a communication. If the tube is not tightly gripped, the X-rays are very useful for localizing it. I show you a skiagram (fig. 3) of an impermeable stricture of the œsophagus with the radium in its rubber tube being pressed down into it by a bougie. This is the reason of the inclination of the radium tube. I am



Fig. 3.

happy to say that this stricture is now permeable. In these cases a narrower tube is often useful. In the treatment of mediastinal growths the cartridge (fig. 2) is attached to a strong thread of floss silk and placed at the back of the throat when the patient can swallow it quite easily.

Rectum.—If the tube can be introduced right into a carcinomatous stricture this is done, and a much larger dose than usual can be given. Otherwise it is made to lie on the growth. It may be introduced from above by a colotomy opening and the patient placed in a suitable

position. The rectum is also a convenient channel for treating the prostate.

Vagina.—In the treatment of cervical carcinoma the tube is placed into the middle of the growth, which will generally grip it and hold it. In other cases it must be packed in place. The vagina is also useful for treating carcinoma at the base of the bladder.

I have no experience of internal applications to the larynx, to the urethra, or to the bladder (by a cystotomy opening).

Introduction into the Tissues.—This has the advantage, often obtained also in the treatment of internal surfaces, œsophagus, rectum, &c., that the rays in all directions are utilized. Personally, I have not yet employed this method, though I highly approve of it. The radium should not be introduced into the centre of the tumour, but towards its margin in several places. *Treat the growing edge and leave the centre to take care of itself.*

General Considerations.

It is just as important to treat the outlying glands, whether they are obviously involved or not, as it is to do radical operations in surgery. Filtration is essential to secure the selective action. The thicker and denser the filter the more selective the action. A large quantity of radium will cure a tumour which a small quantity will not. A sufficient dose must be given to all parts of the tumour and glands. The cardinal principles then are:—

(1) Treat the outlying glands and the whole tumour.
(2) Filter the rays.
(3) Use plenty of radium.
(4) Give large doses.

The next question which arises is that of the supremacy of the one-dose method and the frequency of its repetition. Some claim that by dividing up the dose the action on the skin is lessened. But, surely, it is only reasonable to suppose that the action on the tumour is also decreased. Then, apart from the inconvenience to the patient and the operator, it is very difficult to gauge the dose by this method. I find that my dose can safely be repeated in six weeks, but probably experience will teach that this repetition can be made even before the latent reaction from the first dose appears, and I hope some day to increase the efficiency by making the second application in three weeks. At present I go cautiously.

Class of Case.

Now it is most important to remember that the selective action of the filtered rays varies enormously in different growths; that even two growths of the same kind may behave differently in different patients, and that a similar type of growth in different situations will not react in the same way. To give an instance: an epithelioma of the tongue is usually unsuitable for treatment; an epithelioma of the floor of the mouth will occasionally respond to large doses, while an epithelioma of the lip will often react to ordinary doses. The behaviour of a growth seems, to a certain extent, to depend on its histological structure, and we may be able to make more of this presently when we have more experience and data to go upon. The size and situation of the growth is of much less importance than this, but under certain circumstances may render it unsuitable for treatment by making it inaccessible. It stands to reason, however, that, in a suitable case, the earlier in the course of the disease it is treated the better, and this is the basis of treatment to prevent recurrence.

Epithelioma.

Taking, first of all, rodent ulcer, the cure of this, when it can be reached at all, is as certain as anything in medicine, and anyone can improve their statistics by including this class of case.

With regard to squamous epithelioma, that occurring on the vulva is, in my experience, quite unsuitable for treatment; in fact, I am not certain that the growth is not sometimes accelerated.

Epithelioma of the tongue is only curable in a very early stage, when it ought to be operated on, and as soon as it has begun to infiltrate the muscles it is useless to treat it by radium. This fact is interesting because, though in the early stages it will almost invariably be wiser to adopt surgical measures, it shows the possibility of dealing with early stages of growths which are later refractory, and so with remnants left by the surgeon, by prophylactic applications, and also the possibility of radium supplanting surgery in the early stages of some forms of the disease.

Epitheliomata of the floor of the mouth will sometimes respond to large doses. Here, again, I could never recommend the treatment of an operable case, and I expect we shall get better results from an alliance of the radiumologist with the surgeon in both operable and inoperable

cases. This disease is treated both from inside the mouth and outside, under the jaw.

Epitheliomata of the lips, buccal mucous membrane, palate, pharynx, and nose are suitable for radium treatment. In these cases the tube is held against the growth by various devices, a piece of stiff flexible copper wire being very useful. In the case of pharyngeal growths low down, an œsophagoscope tube is employed. The desirability of treating operable growths must be decided on the merits of each case, and the rate of growth and histological structure will have a great influence on the decision. At any rate, disfiguring radical operations may be avoided by suitable radium treatment. Do not forget to treat the glands, even if not enlarged, in these cases.

Epithelioma of the larynx ought to be treated by the filtered radiations in every case. In the intrinsic form the delay of a month or six weeks, which is needed to see if there is improvement, will do no harm, as the disease spreads very slowly, while, in the extrinsic form, anything ought to be tried to avoid the operation of laryngectomy, which leaves the patient considerably crippled. This disease is treated from outside, and, except in very stout patients, one can get within a few millimetres of the growth. Until recently I have only had one case of this disease; a patient in whom a tracheotomy had been performed about a year before, and in whom the growth was so large that it was pressing on the œsophagus, so that he could only take food through a nasal tube. Within forty-eight hours he was able to swallow. He died of toxæmia a month after, but the power of swallowing remained until the end.

Epithelioma of the skin is suitable for treatment, even if rapidly growing. If ulcerated, one can give very large doses to the ulcerated portion without fear of damage: for instance, using my apparatus with 1 mm. of lead, ½ mm. of silver, and 2 mm. of indiarubber, I should not hesitate to give a dose of forty-eight hours to an ulcerated epithelioma, though the dose to healthy skin is only thirteen to fifteen hours.

Carcinoma.

To deal with the most common form—carcinoma of the breast; an inoperable case of this is often suitable for treatment. I have seen disappearance and diminution of nodules of this nature, and have seen a case where pain from pressure on the nerves supplying the arm was completely relieved after forty-eight hours from the first treatment. Owing to the frequency of mediastinal involvement, it is necessary to

obtain instantaneous radiograms to show the extent of this. If one decides to treat such a case with radium, œsophageal applications are made (using 1 mm. of lead at least), the tube which the patient is made to swallow being got into position by paying out a silk thread to which it is attached, while she is examined with the X-rays until the active part of the tube, the lower end, comes into the centre of the mediastinal mass. If there are only discrete glands which cannot be seen on the screen, the distance is determined on the skiagram in relation to the aortic arch, and the radium then passed to the correct distance. The very slow growing mammary carcinomata may sometimes be treated by the radiations without operation. Yet they will not always respond, and occasionally the rapidly-growing ones will. I cannot yet understand the reason of this. After every operation for carcinoma of the breast, several radium tubes, each containing 5 cg., and encased in 1 or 2 mm. of lead, should be left in the wound in various parts, to remove any cells the surgeon has left, and the mediastinum should be treated from the œsophagus, whether obviously involved or not.

Carcinoma of the œsophagus is a disease in which this is the only method which holds out any hope. It practically always relieves, and we hope in time to cure some of the cases. They are often very advanced when we first see them. I have worked at this question with Dr. William Hill, and the only early case we have had is very much improved. The applications are usually internal, but when the growth is just below the cricoid cartilage, applications outside the neck can be made as well. We have seen great relief from only external applications in one case, but he, unfortunately, had extensive disease below the strictured part. An X-ray examination must always be made in these cases, and it greatly helps one in the application.

Carcinoma of the rectum has shown itself particularly responsive to treatment in most of my cases. If possible, the tube is placed in the stricture and left there for considerably more than an ordinary dose; but if this cannot be done, it is applied to the surface of the growth or inserted into its substance.

In the one case of carcinoma of the prostate which I have had, the result was extraordinary, the whole tumour having completely disappeared.

Carcinoma of the stomach and intestines would probably react, but I have no experience of them. The latter will generally be better treated by operation.

Carcinoma of the cervix reacts only with difficulty. Dominici has

employed it first of all to render operable a growth which was originally inoperable, and then, after removal of the growth, to apply it to the scar, and he seems to have had successful results from this. Personally, I should be inclined to leave a tube *in situ* at the time of the operation and give one or two further prophylactic doses subsequently.

Carcinoma of the body of the uterus ought to be most favourable, but I have had no case until a few days ago.

Carcinoma of the penis, again, has been very successfully treated in Paris, but I have had no case of it.

Sarcoma.

Of this disease I have little experience. Successes have been obtained by other observers, but many forms must be unsuitable, on account of the rapid metastases. In one case I treated there was no improvement. Some of my successful cases of what most pathologists would have termed round-celled carcinoma have been classed as endothelial sarcoma, but I am bound to accept the more generally used description, and describe them as carcinomata. True endothelioma appears to react well.

To sum up, then, it is the slowly-growing epitheliomata, a few rapidly-growing ones, many types of carcinoma, especially the small round-celled variety, and the adeno-carcinomata of the rectum and the endotheliomata which react best.

Relation to Surgery and X-rays.

I have shown that the desirability of a partial or total operation must be decided in each case on its merits, and that several factors must influence this decision; there remains to be considered the question of treatment by X-rays, especially with certain modern improvements in the method of their employment.

The disadvantages of X-rays are: (1) The difficulty in securing sufficient penetration; (2) The lessened selective action on the cells of most tumours as compared with radium; (3) The painful reaction if the dose is pushed; (4) The difficulty or impossibility of applications to the internal surfaces in sufficient doses. Both (1) and (2) may be improved by filtration, but neither penetration nor selective action can be made equal to that of radium.

The advantages of X-rays are: (1) The possibility of giving doses with the tube at a fair distance from the skin and filtering through

metal, so that a reasonable dose is obtained at a depth; (2) The ability to treat large areas at the same time; (3) The diminished expense.

In the treatment of rodent ulcers, failure with X-rays is rare, though the scar left is more obvious than after radium treatment. But one often needs to push the dose to a painful reaction, and seldom can get a cure with a single dose, as with radium.

In treating recurrences after operation for mammary carcinoma, I often prefer X-rays, owing to the widespread involvement of the mediastinum in many of them.

My experience extends, excluding rodent ulcers, to forty-eight cases, many of them of the most hopeless description, for I often undertake a case to relieve pain only. I can honestly say that I have obtained relief in more than 70 per cent. of all cases; that is, either relief from pain or diminution of the tumour. I have two inoperable cases apparently cured, and several others well on the way to a cure. As only two of all the cases are operable, you must agree that the primary results are superior to any other method; and, even if they are not lasting, the relief and respite given by this painless treatment marks a great advance in our therapeutics.

That our results will improve with improved technique and larger quantities of radium is unquestionable. The lines along which improvements will take place are indicated by the cardinal principles: (1) Treat the outlying glands and the whole tumour; (2) Filter the rays; (3) Use plenty of radium; (4) Give large doses.

In some cases radium will not cure on account of unsuitability of the type of growth, and in others the situation or the generalization of the disease prevent the desired result. Whether this, when obtained, will be permanent, we cannot say for years, of course, though both in France and America they have had cases which have remained free from recurrence for over four years. Of one thing we can be quite certain: that in suitable cases it will prolong life, relieve suffering, and cause the disappearance of the growth. The treatment is absolutely painless if the doses are not too large, and a recurrence would, in all probability, react in the same way as the primary growth.

The treatment is undergoing a boom at present and is being used for many unsuitable cases. There is bound to come a reaction, but it will not be a severe one—let us say a healthy reaction, and not a necrosis. My paper is, I know, in the nature of a preliminary communication, but I have tried to place my results fairly before you without exaggeration. I trust that I have succeeded.

DISCUSSION.

The PRESIDENT (Dr. Sloan) said he was sure all present would be grateful to Dr. Finzi for his admirable and just record of his work. The question to consider was not what others had found, but what the author had himself found. Some people were apt to condemn radium and other means of treatment because in certain cases they had done harm. He usually found that an agent which was incapable of doing harm was also incapable of doing good. Dr. Finzi had emphasized the importance of dosage; and, if this were borne in mind, he (the President) had no doubt there was a future for the treatment. He was sorry he had not personally had any experience of radium treatment.

Mr. DEANE BUTCHER said that the treatment by radium had hitherto been too much neglected in this country. It had a very important future, but one which was perhaps more limited than many imagined. There was a disadvantage in the use of the bromide of radium, in that it was changeable in composition and soluble. The sulphate or carbonate of radium was insoluble, and therefore better fitted for therapeutic purposes. The dosage of radium was a matter of very great importance. The Röntgen Society had prepared a standard of radium and deposited it at the National Physical Laboratory, where specimens of radium might be standardized. Radium should be prescribed in terms of the metallic radium contained in the salt. To say that a given specimen contains 20 mg. of a salt of one million uranies activity gives one but a vague idea of the quantity of radium one is dealing with, whereas a radium applicator or plaster is absolutely defined by saying it is of 4 sq. cm. area and contains 5 mg. of pure radium per square centimetre. The rationale of radium treatment was a matter of very great interest. The primary action of radium irradiation seemed to be an interference with the nerve supply of the part, as shown by the immediate relief of pain, and especially of itching. The selective action of the radium on the tissues was probably due to the alteration in the electric conductivity of the skin and subjacent parts which undoubtedly took place during radium irradiation. The question of filtration required a good deal of study. He was not satisfied with Dr. Wickham's explanation; he (Mr. Butcher) thought filtration merely protected the skin and superficial parts from injury by holding back the softer β-rays, which are particularly inimical to the nervous and vascular mechanism of the skin. One of the most interesting points he had noticed was the proliferation of the epithelium at the edges of a varicose ulcer or even an X-ray ulcer. In the treatment of cancer also there was a marked stimulation with growth of fibrous tissue which seemed to embrace and strangulate the neoplastic cells. Dr. Finzi had alluded to a latent period of five weeks. In his own experience with but slightly filtered rays it was much less than that—often as little as five to ten days. It might be that the filtered rays had a much longer period of latency than the unfiltered radiations. For intense irradiation it might

be possible to use a screen perforated with small holes like a colander, so that the destruction of the skin should take place only in isolated spots, which, being surrounded by uninjured tissue, would speedily heal, just as by the actual cautery one made little dots, so that the whole surface of the skin would not be destroyed. With regard to the enlarged glands, he preferred to treat them with X-rays, as it was difficult to treat any large surface by radium alone. As to results, his experience was limited to cutaneous cases. He had seen cases of epithelium of the face and other parts which had been absolutely cured, as far as he knew, by radium; and when recurrences had taken place, a repetition of the radium treatment had been quite successful.

Mr. F. C. WALLIS said the Section was more scientific than clinical, but still the clinical facts he could give in connexion with three of the cases which Dr. Finzi had mentioned might be of interest. One of the cases was that of a young officer with an enlarged prostate—an athletic and capable man. He was sent to him by a physician with the statement that he had a tumour which could be felt *per rectum*, on which he would like Mr. Wallis's opinion. Feeling anxious about the nature of the swelling, he got another surgeon to see it, and they agreed, from clinical experience, that it was an inoperable malignant growth. Dr. Finzi was asked to try what radium would do for him. It was six months since he was treated, and the tumour had absolutely disappeared. After the first dose of radium the mucous membrane over it was found to be very lax and the tumour somewhat diminished. Though the growth had disappeared, one more dose would be given for prophylaxis, and the patient would then go back to the Service. In another case he had already operated for cancer of the rectum, on a woman, the growth having been found 5 in. from the anus. The growth was removed by a trans-sacral operation, the mucous membrane above the growth being brought down and stitched to the anal margin. She made an excellent recovery, but had not been seen for eighteen months, when she returned with a recurrence as large as a hazel-nut at the edge of the mucous membrane which had been brought down. Examination showed that an operation would be an extensive affair, and control of the sphincter could not be insured. He asked Dr. Finzi to see her with him, and after two applications of radium there was great improvement. Sections of the growth showed it to be adeno-carcinoma. The mucous membrane over it felt exactly as in the officer's case. Another case was one in which he had to do a colotomy for bad rectal carcinoma, which became fixed afterwards. The radium was gradually lessening the growth, and the calibre of the bowel was increasing in size.

Mr. THURSTAN HOLLAND remarked that it had been said that stricture of the œsophagus could only be affected by radium treatment. It was impossible to argue from one case, but a very interesting case had come under his own notice, that of a gentleman who was sent to him by a surgeon in Liverpool with all the classical symptoms of malignant stricture of the œsophagus. The patient could not swallow, and it was suggested that X-rays should be tried.

Bismuth food was observed to stop at the level of the third dorsal vertebra, and soft bread and milk containing bismuth was vomited. He could only keep down small quantities of liquid. Since he had had X-ray treatment he had gained weight, and could eat any kind of solid food easily, and bismuth food was seen on the screen to go down without any hitch.

Dr. McCULLOCH said he desired to support the view of Dr. Thurstan Holland. In 1908 he contributed an article to the *British Medical Journal*,[1] describing a similar case, one of four, treated by X-rays. It was œsophageal malignant stricture, and the man lived for eleven months after the date fixed for gastrostomy. He was able to swallow only liquid food with difficulty, till X-ray irradiations were begun, when he returned to ordinary food. With regard to certain types of tissue being influenced by radium, he had no experience of the effect of radium on mucous membranes affected by cancer; but he believed that at St. Mary's Hospital they were using neoformans vaccine in epithelioma, and when the growth did not retrogress, a secondary infection was generally suspected and identified. In such cases the coli bacillus had been discovered, and the vaccine against that being given, there was marked reduction of the growth and relief of symptoms. It occurred to him that where radium was supposed to act on certain types of growth, the actual condition was one of some secondary organism being concerned and overlooked, whether in the rectum, or mouth, or œsophagus. In his opinion if the opsonic index were taken, and the vaccine given accordingly, it would aid the treatment by the radium or X-rays in malignant disease.

Dr. HARRISON ORTON said that although he had not had much experience with radium, except with small quantities for superficial lesions, such as rodent ulcers, yet the case of Dr. Thurstan Holland interested him very much, as five years ago he had, in private, a case of malignant disease of the œsophagus, in which the X-rays were applied from the exterior. At that time the man was able to swallow only small quantities of liquid, and was getting very thin and miserable. Three weeks after X-rays were given the swallowing began to improve, and continued to do so rapidly, and six weeks after the treatment was commenced he was able to swallow solids as well as he had done formerly. But about three months later the condition relapsed, and the patient died. In the year in which the British Medical Association met at Leicester (1905), a man came to St. Bartholomew's Hospital with a large tumour at the angle of the jaw, which had been twice removed at the Great Northern Hospital. It was reported to be a small round-celled sarcoma. After a few exposures to the X-rays the tumour disappeared, and the patient was shown at Leicester as cured. Five months later the disease recurred in the jaw as a myeloid sarcoma. Half the jaw was removed, and again it recurred in the scar. X-ray treatment again caused the disappearance in as remarkable a manner as at first. He remained well for another six months, when the disease recurred in the liver. Very distinct masses could be felt. X-rays were applied and the lumps again dis-

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appeared, and then the patient died. He had been much struck with the similarity of the results obtained by radium to those obtained by X-rays. One could get at certain regions with radium which could not be well reached by X-rays, and in these it no doubt had advantages. Bordier had given very interesting results lately in the treatment of uterine fibroids, in which he had induced an artificial menopause and disappearance of the tumour by filtering hard rays through aluminium. Satisfactory filtering was likely to open up a new field in X-ray therapeutics. He had treated altogether over 100 cases of (recurrent) carcinoma of the breast with X-rays, and in only five of them could one say there was an apparent cure. It was now five years since the rays were stopped, and there had been no recurrence. One lady had nineteen recurrences in the scar. Of the rest, although one had relieved the symptoms—especially pain—and in many cases removed the primary evidences of growth, they had eventually relapsed and got recurrences elsewhere. Was not this the usual course with radium? He did not know whether Dr. Finzi could produce any evidence of absolute cure other than in superficial cancer. Did he know of any case (excluding these superficial cases, which could be cured by other means) in which, in his experience or in that of other workers, a proved malignant mass had been cured by radium?

Dr. FINZI, in reply, said it was impossible to say yet whether there were any permanent cures or not. It was too early to say whether the disease in the prostate case which Mr. Wallis mentioned would recur. In this no microscopic section was obtainable, but he had a case of recurrent glands in the neck after removal of carcinoma of the lip; an attempt was made at extirpation, but some of the disease was left behind. That disappeared under radium applications. He had seen a case well five years after being treated with X-rays; this was sarcoma of the mediastinum and glands of the neck. Another case of inoperable sarcoma of the ilium disappeared under treatment with X-rays, but recurred five years afterwards; it was then again treated, and was now disappearing. He agreed with Mr. Butcher that the only way of standardizing radium was on the amount of the metal. When buying radium, one would be told there was so much radium bromide in it, but often it was found not to be so when compared with a standard. There should be a Government standard, with which it could be compared. The pastille experiments were made in the dark. When using β-rays through a thin silver filter, it gave a rough indication of the dose; but when the γ-rays were used through half a centimetre of silver there was no change in the pastille in three weeks, although it was kept in the dark all the time, in a safe and in a box. If a large dose were given, the latent period would come on in forty-eight hours. With the ordinary dose he found slight erythema four or five weeks after. There was more idiosyncrasy with radium than with X-rays. He had seen lymphatic glands disappear under it which were known to be infected with carcinoma. All the cases he had treated for Dr. William Hill had been instances of advanced œsophageal carcinoma. Possibly there might be cures of such cases with X-rays, but he did not think

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it so likely as with radium, because of the greater facility of application and the greater selective action of the latter. The reason he thought the result of treatment of recurrent breast cases was so bad was because of the early recurrences in the mediastinum, as shown by instantaneous radiograms. So far, he had had no experience of German radium, but he had ordered 60 mg., and later on would probably give his experience of it. A knowledge of dosage was gained by experience, and the dose was measured by time. The factors to be considered were the distance of the radium from the growth, the thickness of the filter, and the time of application. The relief of pain was due to the selective action on the growth and the diminution of the tumour, and consequently of pressure. Another cause of benefit was by reducing the infiltration of the tissue around the growth. He understood from Sir William Ramsay that the atomic weight was not definitely known. He could not tell Dr. Pirie much about the difference between filtered γ-rays and filtered X-rays, but there was a physical difference, for there were cases which would respond to filtered radium rays which would not respond to filtered γ-rays. The effect on the Sabouraud pastille was also different, as his experiments showed.

Electro-Therapeutical Section.

January 21, 1910.

Dr. Samuel Sloan, President of the Section, in the Chair.

On the Therapeutical Use of Static Electricity.

By J. A. Voorthuis (Holland).

Much honoured by the amiable invitation, I accede with great pleasure to the desire of your vice-president, Dr. Morton, to read a paper at this scientific meeting on some new applications of static electricity, especially the spark and flame discharges for therapeutical purposes. I have tried to overcome the difficulties of expressing myself in the English language, of which I have only a little experience. I invoke your indulgence with regard to my boldness in this matter. Occupied by the application of Finsen's method in a number of cases of lupus that were under my care some three years ago, I was much disconcerted regarding the great amount of money, time, and perseverance demanded both from the patients and the attendants by this method. The results obtained by the Finsen method are so splendid and so well known that I need not praise them; so it was only by the desire to meet with those drawbacks that, at the moment Dr. Suchier's method came to my notice, I felt myself obliged to give it a trial. Being in possession of a small influence machine of the Wimshurst type, I arranged it according to the prescriptions of Suchier to make some preliminary experiments, the results of which were so encouraging that I decided to study the method at the source—Dr. Suchier's clinic at Freiburg, in Baden. Three big influence machines of the Wimshurst type, each with twelve ebonite plates, with inlaid tin sectors, turning at a speed of 800 or 1,000 revolutions per minute, were at work all day and all the year round.

Even the most desperate cases of lupus were welcome here, and I was quite astonished at seeing the beautiful results, of which some photos I can show you speak volumes. Only a few months were required for this case, for which the Finsen treatment would certainly have taken two years. Not only lupus, but other skin diseases were treated in Suchier's clinic. Even cancer of the skin was not beyond his reach; innumerable were the diversity of nerve disturbances that were under static treatment.

I left Freiburg with many new suggestions and with the knowledge that the static current can be as easily managed as the galvanic or faradaic currents. I ordered a big Suchier machine, which enabled me a few months afterwards to commence the new treatment. A great drawback in the use of influence machines is their proverbial unreliability in wet weather and certain atmospheric circumstances. My machine has at this moment been in use during two years. It never failed even on the wettest days of autumn, and this in my notoriously wet country. The secret of the reliability of Suchier's machines lies in their being built within a strong enveloping case and the special means to keep the machine clean, dry and warm, and to avoid direct sunlight—conditions under which only the instrument will at any moment be at our disposal. Of course, a machine which is to be in action every working day for several hours ought to be correspondingly strong. The only failure which I have to refer to in Suchier's machines is the use of leather belts for driving the ebonite plates. These leather belts, owing to their extensibility, are the cause of a certain irregularity in the turning of the plates and consequently of an irregularity of the current. In future these belts must be eliminated. This can be arranged by substituting the set of counter-turning plates by fixed ebonite or glass ones; the turning set can then be fastened all on one turning axle. In this way a higher speed, and consequently a stronger current, can be obtained. Dr. Hulst, of Chicago, has realized this idea in a vertically-standing machine of fifty plates turning at a speed of 1500 per minute. For our purposes, however, we do not want these excessive measures.

It is not my intention to theorize on static electricity. The origin of electricity by friction, although the oldest form in which electricity is known to us, is still very obscure. We may suppose, according to the latest views of science, to which Fournier d'Albe has given a popular introduction in his book "The Electron Theory," that there are at the surface of every uncharged body a number of electrons ready to part company with the positive atoms to which they are attached. The effect

of rubbing two bodies—of which one is rich, the other poor—in such loose electrons gives the loose electrons an opportunity to pass, and so we get a charged body. Once we have a charged body, it is easy to obtain other charged bodies by influence. The ideal electro-static machine is one in which electrons can be separated from positive atoms, with an expenditure of work which can be entirely recovered in allowing the two electricities to recombine. A limit is set to the amount of electricity separated and stored in condensers by the dielectric strength of the medium. When the air or other dielectric between the coatings of a condensor is subject to a certain limiting stress, an electron breaks out from the negative coating and rushes across towards the positive coating. In doing so, it collides with a number of neutral atoms and breaks them up into ions of opposite signs. These ions practically reduce the distance between the coatings, and thereby increase the stress. This, again, leads to a rush of further electrons and positive atoms towards each other, and we have a whirl and turmoil of movements, collisions, separations and recombinations—we have the electrical discharge.

The discharges that will occupy us in this meeting are the point, the brush, and the glow discharge, by all of which the gaseous molecules they pass are split up into ions. I can show you these discharges at the machine which Dr. Morton has been so kind as to get ready for this demonstration. The first thing we want to manage the current is a metallic switch between the two poles of the machine. By flexible metallic wires, connected with isolating glass rods, we can conduct the current to the metallic electrodes, each provided with a glass or ebonite handle, allowing us to direct the spark on the spot where we want it. For the point and flame discharges our electrodes are provided with a platinum point, in order to avoid oxidation. Our electrodes can be easily sterilized in a gas or alcohol flame. Now, when the machine is working and the switch closed, we bring our electrodes in contact. By opening the switch the current passes through our wires and our electrodes, but, as the circuit still rests closed, the moving ions and electrons can recombine within the circuit. We do not perceive anything of what is happening, but as soon as we remove our electrodes from each other an air-gap is formed and a spark appears; enlarging this air-gap, our spark changes into flashes. Increasing the distance, these flashes disappear, the brush and flame discharges enter on the scene, both accompanied by a beautiful violet light. Further and further removing the electrodes, those flame discharges vanish in their turn, and the silent discharge, known as the electrical wind, occurs. In all these different discharges,

condensation of the air takes place. The silent discharge just mentioned is accompanied by the formation of ozone, the presence of which can be easily detected by a sheet of paper soaked in a solution of iodide of potassium in company with a starch solution. When we expose our paper to the electric wind, we see the paper in a few moments taking a well-known blue colour, signalling the iodine set free by the ozone with the following chemical reaction :—

$$2\,KI + O_3 + H_2O = 2\,KOH + I_2 + O_2$$

In a room in which a static machine is at work the presence of ozone is easily perceived. The aspiration of it, if the proportion is not too high, is rather agreeable and refreshing, and, however true it may be that ozone is easily decomposed when in connexion with organic substances, it certainly must be a good supply of oxygen; and some cases of phthisis incipiens being at that time under my care, I proposed some experimental inhalations. As none of the patients were in the least inconvenienced by these inhalations, I continued the experiment for months at sittings of one hour every day. All agreed they were much refreshed by it. The number of the respirations diminished very sensibly, whilst each respiration became more intensive; the aeration of the lungs seemed to be amplified. All were undoubtedly benefited by it, and in one case the accompanying fever soon disappeared. By measuring the average quantity of the ozone thus supplied, I determined a rate of about 28 mg. per cubic metre, a rate far higher than that of the air of Arcachon, the healthy effect of which on lung patients is ascribed by prominent French authors to the presence of ozone.

Another use of this silent discharge is its application as an electrical douche for different nerve disturbances, especially headache. By applying the electric "spider" as a douche on the head in a darkened room, we see the electrical discharge leave the scalp along the hairs, accompanied by minuscule sparks and flimsy violet flames; and by this observation an effect on scalp diseases really is suggested. Although I had no occasion to examine the effect on scalp diseases—e.g., tinea favus and trichophytea—yet in one case under treatment for nerve disturbance I saw a bald spot, which had existed for years, completely disappear under the influence of the static douche.

Turning towards the application of the flame discharge from a metallic point, the first thing that strikes us, when approaching so close that it comes within the reach of the respiratory organs, is an extraordinary penetrating odour, quite different from the refreshing and

agreeable odour of ozone; if we happen to inhale it, it provokes an irresistible cough. Apparently we have to do here with the production of nitrous and sub-nitrous acid resulting from the splitting up and following recombination of ions of the air components. Therefore the application of this flame to the face requires special care. Applying this violet flame to the skin of our hand, we perceive it as a cooling wind. That in reality heat is excited can easily be proved by directing the flame on a piece of paraffin or wax. Under the influence of the flame it melts. The effect on the uninjured skin, after an application of a few minutes, is a certain degree of hyperæmia, lasting longer according to the duration of the application. Continuing it, some swelling of the skin appears, and the irritation can go so far as to produce exudation of a clear serum. This irritative effect has led to its application to atonic sores and ulcers on which the disinfectant effect of the flame is soon appreciable, and which often heal after some sessions, each lasting about one hour. Naturally, this process of healing can be much accelerated by preparatory surgical treatment—curettement.

Suchier obtained very good results in the treatment of tuberculous and scrofulous keratitis. Here the violet flame is directed straight to the eyeball, at first only for a few seconds; afterwards, when the patients are accustomed to the irritation, for one or two minutes. Suchier reports even the disappearance of maculæ corneæ under its influence. Suchier also praises the treatment of eczema in its various forms by the flame; even in psoriasis he had good results. In the few cases that came under my care I could state the same favourable effect. In one case of chronic eczema in the left elbow, which had resisted for years all different kinds of treatment, four sittings were sufficient for a radical cure. In neuralgic pains the violet flame has a real sedative effect. This led Suchier to its application in cases of tabes and other nerve diseases. His results, which have not yet been published, are, according to a communication by letter recently received, rather astonishing: returns of knee-jerks, recovery of serious motor and sensory symptoms could be stated. The flame discharge in these cases particularly seems to affect those paths which, although attacked by the infection, are not completely degenerated or destroyed. In one case of Friedreich's disease in a female patient, aged about 40, very remarkable improvements were obtained. In primary lateral sclerosis the success was less brilliant, but even here improvement could be stated. Diseases of the grey substance of brain and cord seem to resist the treatment. Neuralgic pains are very successfully influenced by the violet flame.

Before passing to the use of the spark, I must mention a very curious effect of the flame on the Röntgen screen. Directing the flame to the sensitive coating of the screen, we see the same phosphorescence as when touched by X-rays. The direct collision of the charged ions with the sensitive coating renders it luminant. But now turning the flame to the reverse, we see the radiation penetrating the whole thickness of the screen and the sensitive coat again illuminating. Photographic paper and plates are in the same way affected by these rays, as I can show you by these specimens. Whether we have to do here with X-rays, kathode rays, or perhaps only charged ions passing the pores of the screen, is another question to be solved by physical research. Professor Lorentz, of Leyden, the famous promoter of the electron theory, to whom I showed this beautiful experiment, did not reject the idea of the presence of X-rays. He suggested some physical experiments to solve this problem, which, however, lie out of the sphere of the medical practitioner.

Coming now to the application of the static spark, it seems that the method meets with some difficulty in spreading through the therapeutical world. Probably the cause must be sought in the fact that the management of static machines is still very uncertain. Even the machines built according to the suggestions of Suchier have many important faults, but, as soon as we are in possession of perfect, strong and reliable instruments, the method will undoubtedly take its place in daily practice. Suchier has described the mode of treatment in several papers published in the *Wiener Klinik*,[1] the *Dermatologische Zeitschrift*,[2] and in the *Annales d'Electrobiologie et de Radiologie*. Dr. Jesionek, of Giessen, professor of dermatology, has investigated and practised the method, of which he gave an account in the *Münchener medizinische Wochenschrift*.[3] His results are in all respects equal to those of Suchier.

For the electrostatic treatment of lupus the process begins with thorough cleansing and disinfection of the parts to be treated. Under local or general anæsthesia the tuberculous granulations are removed by small but strong and sharp curettes, a work which, under unusual circumstances, may prove very difficult, as, for instance, in old, indurated cicatricial tissue. After the bleeding has ceased and the wound has become almost dry, the patient is connected with a metallic electrode in connexion with the negative pole of the machine. A

[1] 1904, xxx, p. 269.
[2] 1905, xii, p. 731.
[3] 1908, lv, pp. 1270, 1342.

pointed electrode connected with the positive pole is now placed on the healthy skin in the neighbourhood of the wound to be treated. The circuit is opened. Lifting the positive electrode from the skin at the distance of about 1 mm., we see the spark appear. The glass handle of the pointed electrode enables us to direct this spark on every part of the wound. In about five or ten minutes the wound gets covered by a dark brownish crust, surrounded by an anæmic ring of the healthy skin. As a rule, a little swelling of the surroundings of the wound follows the application, but it disappears within a few hours. Also some serum is often secreted, which should be absorbed with an aseptic tampon. As the wound is now completely dry, no bandage is required. The patient is ordered to leave the wound untouched under any circumstances. Usually within a few weeks the crust is loosened and rejected, meanwhile epidermization has taken place under it, and a flat, pink scar is left which, by the thickening of the epidermis layer in another few weeks, becomes entirely white. The application is not very painful, and, once set going, a certain amount of anæsthesia is produced. Even the weakest and most sensitive patients learn to stand it during the first sitting. Especially in scrofulodermia I found it to be of high value.

As to the duration of this sideration, as the process can be called, it is found by practice that a spot of the extension of about a shilling takes nearly half an hour. It is easily perceived that this cure takes much less time than Finsen's treatment; the difficulty is to bring the granulations within the reach of the curette, after which healing is almost certain. Not only to the skin is our treatment restricted. It is possible to use the spark also inside the cavities of the nose and mouth; therefore glass tubes, provided with a small copper wire, must be introduced. As the glass tubes, through the respiratory air-current, soon become damp and wet, we have to avoid their becoming conductors and to prevent the production of undesired sparks; this is of no difficulty within the mouth, but gives much trouble when operating within the nose. Occlusion of the nose by tamponnade during treatment is then necessary. Asking what may be the direct cause of the curative effect of the spark, it is obvious that the principal force in action must be the heat production. Take a piece of tinder or cork and treat it by the spark; smoke is immediately seen, indicating that it is burning. Even during the application on dry crusts of the skin, this combustion is revealed by smoke and by smell. Contrary to other heat applications, in our method the heat is continually created by means of the proximity of wound and electrode, which can be continued *ad infinitum* without any damage to the patient. Whether,

again, the speed of the ions, the stress of the current, and the light concur in the curative effect, I must leave as a question unsolved. Certainly in many cases of lupus Suchier's method equals Finsen's light treatment, often surpassing it in rapidity. Without doubt it must be regarded as valuable in our struggle against the terrible disease as our modern surgical, light, and Röngten methods, all of which perhaps in future may be superseded by the ingenious method of your illustrious compatriot Wright—I mean opsonotherapy.

To prevent the long flashes which may occur with inexperienced assistants or anxious and fidgety patients, I have constructed a kind of safety-valve by placing between the conducting wires an auxiliary variable air-gap. As soon as the distance of the electrodes from the patient surpasses that of the air-gap just mentioned, the spark is compelled to pass in this air-gap. To one particular application of the spark I must draw your attention—that is, the treatment of teleangectatic nævi of the face. Very fine results can be obtained especially in the so-called port-wine stains. A somewhat atrophic but normal-coloured skin is the only sign of the former state after treatment in this way.

It is possible to use the static current for the stimulation of the nervous system and the muscles, just in the same way as we use galvanic or faradaic currents. Here we have to state that the static current is not a constant one, like the galvanic, but must be considered as a continuous, pulsating, non-intermittent current. If we place two wet electrodes, as used for galvanization, on the skin, and open the circuit, the current is forced to pass through the body. The only sign by which this current is revealed is a slight reddening of the skin under the positive electrode. This current seems to be of no importance. Perhaps in very strong and heavy machines, as Dr. Hulst's, this effect might be more perceptible. If we put a pair of Leyden jars in the current, undulating contractions in the muscles are seen; but as experiments with these arrangements are very difficult, and unexpected discharges rather painful and terrifying, they are out of consideration. By supplying a variable air-gap to one of the wires we can produce an intermittent current, the effect of which is excitation of the nerves as well as of the muscles. The narrower the air-gap, the greater the number of interruptions; the wider the air-gap, the lower the number, but the stronger and more painful the contractions. By using, again, Leyden jars of a convenient size the effect can be strengthened up to a certain limit, beyond which the pain cannot be borne.

Although my experience is not very large in this matter, I have observed good effects in some cases of spinal and peripheral paralysis. One case of writer's cramp, existing for over four years, which had resisted several methods of treatment, was almost completely cured under electrostatic treatment of the medulla cervicalis, plexus bronchialis, and muscles of arm and fingers.

To close the enumeration of the different ways by which we can profit from static electricity, I draw your attention to it as a source of Röntgen rays. By connecting our negative pole to the kathode of the tube and the positive to the antikathode or anode, and opening the circuit, a bright and steady Röntgen light appears, supposing that a sufficient current is produced. However, one condition must necessarily be fulfilled: the tubes must be absolutely dry. It is easily understood that when the tube is damp or wet the glass will take on the functions of a conductor and allow the discharge to pass over, instead of through, the tube; by warming the tubes before use this can be easily obviated. As our current is characterized by very high tension, estimated to vary between thirty thousand and several hundred thousands of volts, and very low amperage, the quantity of Röntgen rays produced is much less than that produced by a big coil. I think this to be a special advantage for the use of X-rays in the treatment of skin diseases, the over-dosing being more readily avoided. For the treatment of deep-seated lesions our method is insufficient; also for radiography, for which a too-long exposure would be required. However, Dr. Hulst has been able to produce instantaneous radiographs of the chest by his gigantic installation.

For the application of X-rays to the face I place my tubes inside a lead-sheathed removable case, a hole in the front of which, provided with a set of diaphragms, allows the radiation to escape. By pressing the diseased spot to this diaphragm the patient takes the radiation. In some particular cases of lupus situated in the superficial layers of the skin, very good results were obtained in this way. Also cases of rodent ulcer in the face gave way under this treatment. One case of molluscum contagiosum is still under treatment, but distinctly improving. From absolute lack of personal experience I must leave the so-called Morton currents, induced static currents, out of discussion.

In closing my remarks, I hope to have convinced my audience that it is possible to utilize the static current in widely different conditions as easily as any other current. As the method, however, is still in its infancy, a fair trial in one of the great medical centres is highly desirable. To induce such a trial has been one of the motives of my presence here.

DISCUSSION.

The PRESIDENT (Dr. Sloan) said that, whilst there was a sort of boom in the matter of static electricity some years ago, it was now being almost universally neglected in this country. He still used static electricity, but only in the form of the static induced current. The results of his previous trial of other forms of it were disappointing. Probably that was the case with most of the members, and for that very reason they welcomed the paper. He believed that after the demonstration they would not have the same idea as they had previously entertained. He hoped to hear a good discussion; and perhaps those who had not had experience of the static treatment would give their experience, if any, of high-frequency treatment; because the latter seemed to act much in the same way as had been described by the reader of the paper, in the case of skin diseases. Since Dr. Reginald Morton recommended it to him three or four years ago, he had used high frequency very successfully for very obstinate skin diseases—cases for which the usual local applications did no good. When the brush was used, the current ought to be sufficiently strong to cause some pain and a slight dermatitis. In the case of boils and carbuncles, he had no doubt, from experience, that by applying a pointed vacuum electrode with a strong current the disease could be arrested. In one or two cases in which opening of a superficial abscess seemed to be called for, he applied the vacuum electrode, and in two or three days the abscess had disappeared. As to the *modus operandi*, was it a local bactericidal action, or was it due to auto-inoculation, brought about by the flushing of the part with blood, which brought the antitoxins to the seat of the disease, taking away also the toxins into the blood current, to be subsequently destroyed there? This state of active congestion permitted the phagocytes to get nearer to the seat of the disease, and enabled the blood fluids so to modify the bacteria as to render them an easier prey to the phagocytes. He believed that to be so in acne and eczema. From one case he had he thought the condition in the skin was sometimes an angeioneurosis, and that the current acted as a tonic on the vasomotor nerves. He had an interesting case of that in a lady, aged 30, who had had, since an attack of measles at six years of age, most annoying flushings of the face; the knowledge that she was to meet somebody would cause her to flush badly, as would also the taking of tea or coffee or the exposure to cold air. There was no acne, nor permanent erythema, nor dry eczema. He applied the vacuum electrode to the face twelve times. Three years afterward she saw the lady again, and she was strikingly improved in appearance. She admitted that all her trouble had gone since the electric applications. For producing contractions of muscles he attached great value to the static induced current. The contractions were painless. He had made recto-abdominal applications for obstinate constipation, with great benefit. The lecturer spoke of producing a favourable effect upon psoriasis. One of his (Dr. Sloan's) patients told him that he had had his

psoriasis treated in that way in Paris, but that the improvement was only moderate; and he would be glad if the reader of the paper would say precisely what the effect was on that disease in his experience.

Dr. CURTIS WEBB said that the method had obtained very little hold in this country, but since 1904 he had used it very extensively in his general practice, and on cases which had been sent to him by others. He had not had a great experience of the treatment in skin diseases, but in three or four cases of psoriasis he had had improvement, though they relapsed after a month or six weeks. Perhaps one of the reasons why static electricity had not taken a hold in this country was that the particular type of machine had too great drawbacks. His own machine was of the American type, enclosed in a glass case; since 1904 the glass case had not been opened more than once a year, for cleaning purposes, and he had not had any trouble with it. Our climate was unsuited to the open kind of machine. Even though the present machine shown was installed only that afternoon, and with great care, there was some delay before it began to work. The American enclosed type of machine seemed to give much better results. Ozonization had been mentioned, especially in connexion with phthisis. Dr. Muthu, of the Mendip Sanatorium, who had devoted considerable attention to the matter, had had excellent results from it. The proportion of ozone generated from the electrical breeze could scarcely be fairly compared with the open-air effect, because the air was breathed at all hours of the day, whereas the machine was applied only an hour or two per day. A particular field of usefulness was the Morton wave-current, which was by far the most important static modality. Some time ago he published in the *Lancet*[1] a paper on cases of chronic sciatica which had lasted eighteen months and which had been cured by that method, and these cases had resisted all other forms of treatment. Since that time he had accumulated over sixty cases of sciatica and other forms of neuritis, and by means of the Morton wave-current, applied with the insulated platform, very good results had ensued. One could not hope for the cure of toxæmia by it, but for the perineuritis the Morton wave-current was one of the most powerful means in the physician's armamentarium. He had been round several of the London hospitals recently; in some he had seen static machines, in others none. Some of those he saw had been allowed to fall into disuse, and evidently they were not much used. The author had spoken of the direct spark and brush. He (Dr. Curtis Webb) had always used the indirect, the patient being treated from the ungrounded pole of the machine. That produced excellent results, and the spark could be regulated to a nicety. After what he had seen that evening, he would certainly try the direct.

Dr. LEWIS JONES said that it was interesting to hear a paper on statistical methods, as of late these had rather fallen into disuse in this country. The difficulty was to obtain precise information as to the cure of disease by statical treatment. The chief need of electro-therapeutics was that it should

[1] *Lancet*, 1907, i, p. 85.

find a field in the treatment of serious or definite disease, and should not be so much confined to the production of general tonic effects, and so on, in neurasthenics or in those suffering from simple debility. For his part, he found that field of electro-therapeutics to be monotonous and unsatisfying. He was glad therefore to hear the writer of the paper deal with the treatment of lupus by the static brush-discharge. There was no room to doubt that this constituted a valuable mode of treatment, and it was possible that the traces of nitric acid and the ozone formed were the active agents in bringing about a good result. It had been shown by Foulerton and Kellas that these were the probable agents of bactericidal effects in the similar brush-discharges of the high-frequency apparatus.

Dr. F. H. HUMPHRIS, remarking on the difficulty of using the static machine, said that a properly manufactured machine in a good case should not need to be opened more than once or twice a year, and it should be as easy to turn on the current as in an ordinary faradaic battery. It was useful in very many skin diseases. His own experience was that cases of acne, old ulcers, chronic eczema and such conditions, could be relieved almost immediately, and good results were seen in a week. He agreed with Dr. Curtis Webb that for neuritis, especially sciatica, the Morton wave-current was good, if the case had not gone as far as the formation of adhesions, for which surgery was the only relief. The pain could be relieved by this form of electricity, and in most cases cured. For lumbago and other myalgias it was almost a specific, giving relief within twenty minutes. For recent sprains and fracture, the brush-discharge was a valuable therapeutic measure, the pain practically disappearing in ten minutes, while the swelling and blue discoloration due to the extravasated blood would gradually disappear. It sounded a brave thing to talk about relieving locomotor ataxy, but he had no doubt that static electricity would give relief to it and improve the walking, for this he had himself done, though admittedly nothing would restore cells which had once been destroyed. Still, the method relieved deep-seated congestion and prevented the destruction of further cells; moreover, he suggested that there might be other cells in the spinal cord ready to take on the function of those which had been destroyed, if the destroying agent—i.e., the congestion—were removed. The President had asked for the *modus operandi* of the method (the direct spark). He (the speaker) thought it was more than heat and more than nitric acid. First, there was a profound contraction of the part to which the spark was applied, and there occurred not only a contraction which could be seen, but a protoplasmic contraction. In the case of lupus the cells were forced to the surface, and when they got there the formation of nitric acid, and probably of nascent ozone, occurred, and that was antiseptic. He thought the reason that many men had not found the method good was that they turned on the static machine and expected certain results to happen, without taking into account the kind of case or the voltage or the amperage. Only in America, probably, was there an apparatus for registering the voltage of a static machine. A competent static machine should give out not less than

90,000 volts, and should be capable of 750,000 volts, and should show on a properly constructed milliampermeter ½ ma. It would not serve as well as a coil for taking radiographs, but the man who went in for electro-therapy had enough to do to deal with treatment.

Dr. PIRIE said that eight years ago, when he went over to America, there was a boom in regard to static machines there, and again six years ago he noticed they retained their reputation. But when he was there in the year just ended, there did not seem to be anything like the same enthusiasm. He went to five hospitals, in none of which was there a static machine; the coil was the only form which seemed to be in use. Static machines seemed to have taken a back seat.

Dr. HUMPHRIS rose again because his experience in America had been so utterly opposed to Dr. Pirie's, and he thought he had had a better opportunity than that gentleman for judging, since he had lived ten years in the country. He thought the use of such machines in America had grown 50 per cent. in the last five or six years. The Electro-Therapeutical Society of the United States, of which he was a Fellow, published a journal in which to-day articles on static electricity formed the most important part. The manufacturers were now turning out hundreds of machines where they were formerly turning out tens, and almost each month saw a new form of machine put on the market or an old form improved. Next time Dr. Pirie went to America he would give that gentleman some addresses, and he would be able to satisfy himself on the point. Dr. Humphris much regretted at having to rise to contradict especially a gentleman whose work was so well known as Dr. Pirie's, but he felt it was not fair to the Society to carry away views upon the subject of static electricity in the United States which he believed were erroneous.

Dr. REGINALD MORTON replied, at the request of Dr. Voorthuis, as his knowledge of English did not, in that gentleman's opinion, enable him to do justice to the discussion. He (Dr. Morton) would take the occasion to say a little on his own account. He was not a specialist in skin diseases, but, like other electro-therapeutists, he occasionally had such cases sent to him, and he had found that the vacuum electrode was one of the most useful and successful things which he had ever employed. That applied especially to such a disease as acne vulgaris. The first case of the kind he had had was on the face of a lady who had been troubled with the condition from childhood, and in her case, though she was otherwise a handsome woman, it was complicated by pitted scars. The first time he used the treatment he did so in desperation, after telling the patient frankly it was in the nature of an experiment. The condition began to clear up almost instantaneously, and in a fortnight one would scarcely have known there had been anything the matter except for the old scars. Another case was that of an attendant at a bath who was in danger of being discharged because he had the disease on the shoulders and it was unsightly. He treated that patient in the same way, and in a fortnight the condition had vastly improved, yet it was of fifteen months' duration. His aim was to

produce a mild yet definite hyperæmia over the area. If a spot was pustular he sparked it, and on the next visit the lesion was found to have withered away. In reply to the discussion, Dr. Voorthuis said the action he got with his flame-discharge was of the same kind as with the high frequency and the vacuum electrode. Also, he thought the action was a local bactericidal one. With regard to psoriasis, he believed the method described was no more efficacious than the X-rays, but he thought the effect produced lasted longer. The author had had no experience of the Morton current, but hoped soon to have. He had found the method valuable in the cure of acne, and for the relief of pain but had not tried it in locomotor ataxy. The lecturer did not understand what Dr. Humphris meant by protoplasmic contraction. His own view—and he did not mean any offence in saying it—was that it existed only in imagination. Dr. Voorthuis wished to thank the Section very heartily for its kindness and for the discussion.

Electro-Therapeutical Section.

February 18, 1910.

Dr. Samuel Sloan, President of the Section, in the Chair.

Points in the Diagnosis of Ureteral Calculi.

By C. Thurstan Holland.

I feel that, to a certain extent, an apology is due to you for venturing to read even a short note at this Society on such a worn and threadbare subject as that of calculi. But certain interesting cases, in which mistakes were narrowly avoided, have influenced me to hope that their short narration and a few remarks based upon them may not be altogether out of place. I cannot claim that there is anything really new about these cases. Probably all X-ray men with any large experience have met with others exactly like them, but, even taking this into consideration, I am quite sure that enough emphasis has not been laid upon certain pitfalls into which one may fall in the X-ray diagnosis of calculi.

Case I.—The first case is that of a woman, E. A., aged 45, who for six years had had acute attacks of pain in the right loin, together with backache between the attacks. There was a history of hæmaturia, but no blood or pus was found in the urine examined at the hospital. A complete X-ray examination showed a small calculus in the right kidney, the kidney itself showing plainly and not enlarged, and a very large shadow in the pelvis over 1 in. long, an elongated oval in shape. I was not present at the operation, but the kidney stone was found and removed and the ureter probed. Instruments passed down could not be got into the bladder, but nothing in the nature of a stone was felt and no more was done. She was sent down to the X-ray department on her recovery, and further plates, stereoscopic and otherwise, were exposed

upon the pelvic region. These were not developed at once, but the next day, on development, it was found that the large pelvic shadow had disappeared. It was not possible to get more X-rays, as the patient had gone to her home in the country the previous afternoon. Two and a half months later she came in again, saying that she was just as bad as before the operation. Another complete X-ray examination showed nothing except a large, elongated shadow in the right kidney, exactly corresponding to the shadow previously found in the pelvic region. A few days later the surgeon telephoned me that he was going to remove this stone from the kidney that afternoon. I asked permission to X-ray her again as she was going to the operating theatre. This was done, through the pre-operation dressings applied to the kidney region, and there was no stone in the kidney. Another plate showed the stone in the lower end of the ureter (fig. 1), and the operation was modified in view of this, and the ureter opened and the stone removed. The ureter was found dilated, and this large stone could apparently move at will up and down its whole length.

The two points this case indicates are clear. One is that although a stone is found by X-rays in a kidney, and this either by plate or screen examination, or by both, it is nevertheless essential to examine the rest of the possible stone area as well. The second point is that one must bear in mind these unusual cases and remember that it is possible for stones to move up and down the whole length of a ureter, even when as large as this one. It is obvious that a small stone may move from kidney to ureter in the interval between X-ray examination and operation, but it is a much rarer occurrence to meet with a wanderer such as in this case. The inference is that no long interval ought in any case to pass between the X-ray examination and the operation, and that, should such an interval have elapsed, then one ought to confirm the previous examination before the operation is carried out.

Case II.—This case I quote to again point out the vital necessity of a complete examination. A. E., a female, aged 19, had her appendix removed in 1904; it was normal. Pain continuing, she had an abdominal section performed in 1905, and during this the surgeon felt the right kidney and suspected the presence of a stone. On her recovery the right kidney was X-rayed and a small stone shown, which was a little later on removed. This did not relieve her symptoms, and she came back eleven months later, when the whole area was examined—as it ought to have been at first—and three calculi were apparent, low in the right ureter (fig. 2). On the removal of these her symptoms disappeared.



FIG. 1.

Case I (E. A.).—Large stone in right ureter.

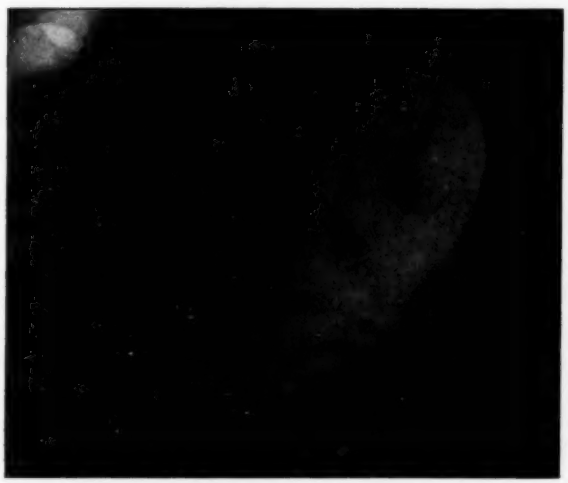


FIG. 2.

Case II (A. E.).—Three stones in right ureter.

A chequered career indeed, and one, I venture to think, creditable neither to surgery nor radiography.

I would again strongly emphasize here the necessity of complete examination in all cases, and if mistakes are to be avoided it must be in all. The presence of ureteral calculi in addition to kidney calculi is not uncommon. I find that I have met with this condition in eleven cases, and it is quite possible that I have missed some in the early days when the whole area was not always done. Further, it is not at all uncommon to meet with other cases where stones are also present in the urinary bladder, and I have met with this condition several times. Whilst on this subject, though not bearing directly on ureteral calculi, it may not be altogether out of place to refer to two other facts—well known, of course, but not always to be illustrated so forcibly as in the two following cases, of which I show the radiographs.

Case III.—That of a man, E. T. B., aged 33. This was a man whose symptoms were some hæmaturia and for two years pain of an aching character in his right back. It was a case in my own practice in which the doctor in charge of the case told me that his patient was a poor man, but was anxious to pay as far as his means allowed him, and would I make the examination for a much reduced fee? So convinced was the doctor that the stone, if present, was on the right side, that as an additional inducement to examine the case it was pointed out that all that was wanted was one plate of the right kidney. Having learned by experience, at the time this case came along, to do the whole area always, I found in this man no stone on the right side, but the large multiple calcareous deposit in the left kidney which this radiograph shows (fig. 3). An operative feature of this X-ray is that, notwithstanding the size of this stone, and its shape, and the evident fact that one end points into the ureter, the kidney itself was not disorganized at all, and the complete removal of the stones was easy.

This case is a most striking illustration of the fact that the symptom may point entirely to one side whilst the stone is in the other.

Case IV.—A male, W. W., aged 48, came to me to be radiographed, with the following history: He had no medical man, as he had never had one in his life. He told me he had never been ill, had never had any pain, had never been to a doctor until a few months previously. Then, not feeling quite up to the mark, he took his summer holiday at Llandrindod, and, before taking the waters there, went to consult Dr. Murray. The urine was examined, pus found in it, and Dr. Murray advised him to have an X-ray examination when he returned to Liverpool.

I show you the radiographs of his right and left kidneys (figs. 4 and 5), and I think it is of some interest to note that such a condition of things can exist without any symptom, without any pain, and that merely the chance discovery of pus in the urine led to this discovery. So far, ever since this discovery, this patient has declined operation, and, really, I am not at all sure that he is not right. It is now sixteen months since the X-ray examination. I heard from him the other day, and he told me he was in splendid health, without an ache or pain.



FIG. 3.

Case III (E. T. B.).—One large stone and several smaller ones in the left kidney.

The point in this case is, of course, the absence of symptoms, especially pain, and the fact that there are huge stones in both kidneys; and it also emphasizes the necessity of a complete examination.

Case V.—A male, R. S., aged 19, who for four years, without pain, had had persistent hæmaturia. The surgeon in charge was very certain it was due to a ureteral calculus; a complete X-ray examination showed nothing. The symptoms continued, and ten months later he was again in hospital and again X-rayed; this time there was found this shadow in the line of the left ureter (fig. 6). "The surgeon was right, there was a stone in the ureter;" "I told you so ten months ago, and I shall

operate at once." I did not feel satisfied; the previous plates were turned up and found to be good ones. This shadow was very clear—too clear, I thought. He was again given a thorough preparation, radiographed the next day, and the shadow had disappeared. An operation on his bladder showed a typical tubercular ulcer close to the right ureter, and this caused his symptoms.

Now this was a case where an error was almost asked for. I cannot, of course, tell you what caused the shadow; but probably, indeed almost certainly, it was something in the rectum. The two facts, (1) that he had been radiographed ten months before negatively, and (2) the very clearness of this shadow, raised my suspicions and saved a mistake. The point I would insist upon, from this case, is that one examination ought not to be relied upon when any operation is contemplated. Almost without exception, certainly in all those cases in which I am allowed to or where it is possible, I confirm a shadow or shadows shown by X-rays before an operation is performed; and with ureteral, or suspected ureteral, calculi I am quite sure that this ought to be a routine practice. Not doing this certainly led me to make the only mistake I have ever been proved to have made amongst my private cases.

Case VI.—This man, H. H., aged 27, was sent to me with a history of eight years previously having passed a stone from the left side, of having had for five months aching pain in the left side, plus one attack of acute pain followed by hæmaturia. A complete examination showed a shadow suspiciously like stone on the left side. A report was sent to this effect, but I had it in my mind that the man would come into hospital, and that another examination could be made before operation. Unfortunately, he was forgotten in the rush of work, and the next I heard was that he had been operated upon, and that no stone had been found. I have had an opportunity of radiographing the man since, and from the change in position of the shadow I think it is probable that it is due to a calcareous gland. Probably this mistake would not have occurred had he been radiographed at least twice before operation, as then the probable alteration in position of the shadow would have raised suspicion. Therefore I say again that all cases, and especially those of suspected ureteral calculus, ought to be examined radiographically at least twice before operation.

These are the cases I bring to your notice to-night to illustrate the points I wish to emphasize and to serve me with materials for a few other remarks. I have brought other radiographs with me to illustrate these and other points, and these are on the table for those of you, who are at all interested in the subject, to look at.



FIG. 4.
Case IV (W. W.).—Stone in right kidney.

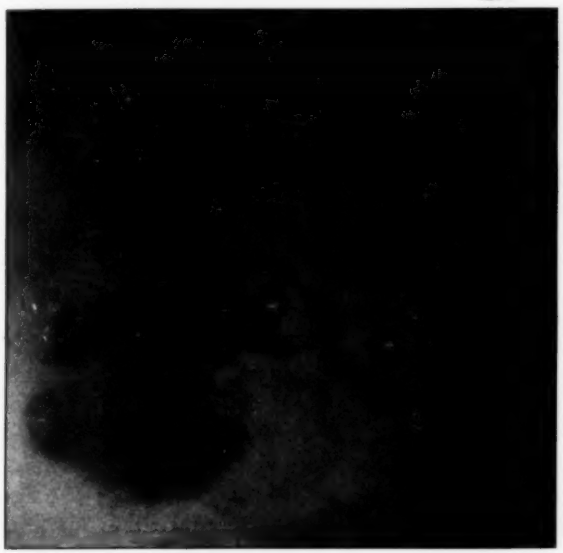


FIG. 5.
Case IV (W. W.).—Stone in left kidney.

I do not intend to enter into the discussion this evening of the differential diagnosis of ureteral stones from gland shadows, calcareous patches on the arteries, phleboliths, and so-called phleboliths. All these cause the greatest difficulties at times, and indeed I think we must admit that it is sometimes impossible for us to make an absolutely certain diagnosis from X-rays alone. Pelvic shadows are so common that they are apt to become a nuisance, and I have plenty of plates to show shadows which, from position and appearance, it is impossible to differentiate from shadows in other cases known to be ureteral calculi. In 750 cases I have recently tabulated, out of 329 males pelvic shadows were present in 98, and out of 189 females in 48; not a very marked difference in percentage, but what difference there is pointing to their greater frequency in males. Adding these together, out of 518 cases pelvic shadows were present in 156, or in 4 out of each 13.

The frequency of ureteral calculi is another interesting point. In the reports of the Liverpool Royal Infirmary for the eight years from 1897 to 1904 there is mention amongst the statistics of only 2 cases of ureteral calculi, and this includes the surgical cases and the post-mortems. During this time only 33 attempts at radiography of stone at all were made, and I believe only one successful result—that of a very large stone—was obtained. From the end of May, 1905, to the end of 1909—that is, four years and seven months—the time since I was appointed to the electrical department, 436 have been examined for stone, and of these 14 have been operated on for ureteral calculi, 5 other cases have certainly had stone in a ureter, whilst 2 may be looked upon as very suspicious. That is, taking them altogether and counting them all, there have been 21 cases of ureteral calculi.

In the *British Medical Journal* of January 1, 1910, in a paper by Mr. Sinclair White, of Sheffield, on ureteral calculi, appears the following paragraph: "The X-rays have demonstrated that, contrary to the belief formerly held, calculi are found in the ureters *more frequently* than in the kidneys. Leonard, from a careful analysis of a series of cases, estimated the ratio as being two ureteral to one renal stone." This is quite contrary to what has happened in my own practice, and I feel sure it must be contrary to what has happened with every one of us who has had anything like a large number of cases.

Of course, statistics are very misleading. As proof of this one has only to study statistically one's hospital and private work; the figures will be found to work out quite differently. Then hospital statistics of different men can scarcely be compared, especially as regards the question of the

percentage number of cases in which stones anywhere in the tract are found. The presence or not of a large out-patient department will vary such statistics at once. And the method of the hospital as regards sending cases to the X-ray department will have a great influence, some out-patient departments sending up any case of chronic backache without any further clinical examination; others only sending up the cases selected after a most careful clinical examination. However, my statistics, hospital cases only, give 21 ureteral calculi, counting those operated

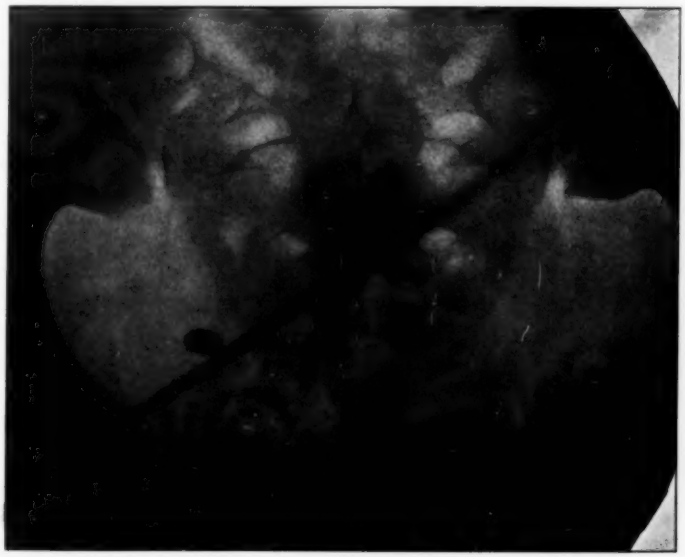


FIG. 6.

Case V (R. S.).—Shadow simulating that of a left ureteral calculus.

upon, those passed, and those suspicious but not proved, to 62 cases of kidney stone removed by operation, and not counting a considerable number of others in which, for some reason or other, no operation was done. This percentage is quite different from that of Leonard, being nearly three to one in favour of kidney stones, instead of two to one in favour of ureteral; whilst if the unoperated-upon kidney stones were counted in, the percentage would be at least four to one. I cannot help thinking that Leonard's statistics, published in 1905, are entirely

misleading, and that they can only be explained by certain special circumstances, and that previously to 1905 stone statistics were not so reliable as at the present time.

Before finishing I should like to say a few words on the general technique of X-ray examination for stone. I will preface these remarks with this: A gentleman wrote to me a short time ago, saying "that he entirely relied upon the screen examination for making a diagnosis, and that it was possible to see stones on the screen which could not be shown on a plate." This appears to me to be at any rate a photographic, if not an X-ray, absurdity. I do not wish to decry screen examinations at all; in many cases they are of the utmost value, and no doubt fewer mistakes would be made if screening were done as a routine in addition to plate exposures. But I am quite sure that with proper technique one can always show on a plate anything in the nature of a calculus shadow which can be seen on a screen. And that, if one method alone is going to be used, properly conducted plate exposures are the most reliable. Good screening is better and more reliable than a great deal of the plate work which is being done: that I am quite prepared to admit. But given the best possible plate work, then the very best screen work is quite unable to compete.

In the very large majority of stone cases there is no difficulty whatever in demonstrating by either screen or plate that a shadow is present. But in the small minority, those where a mistake may occur, the difficulties are very great. Certain I am that the screen alone cannot be relied upon for making a negative diagnosis of calculus, and it is in these cases that we are not quite so likely to be found out.

My hospital operation statistics of stone cases are as follows: Since June 1905, 436 cases examined and 107 of these operated upon. No proved mistake at all unless we accept one case where a kidney was opened *in situ* and a stone not found, a case in which, having again X-rayed the man since the operation, I am quite sure that a stone is present. That is, counting this as a mistake, my percentage of correct diagnoses comes out at over 99 per cent. The most recent statistics of one relying upon screen examination alone, as published in the *British Medical Journal* (and from kidney cases only, not a mixture of kidney and ureter cases), give a percentage of over 95 per cent. only of correct diagnoses as proved by operation on 92 cases.

In conclusion, there can be no doubt but that we must all make occasional mistakes, some of which will never be found out, and so will not count against us. Those mistakes will most often happen to those of

us who are too cocksure and too satisfied with one particular method of examination. Be always dissatisfied with your X-ray work, always distrust your screen vision, always look with doubting eyes upon your very best negatives. To reduce errors to a minimum no one method of examination must be entirely relied upon in all cases. Of course to a certain extent one must have a routine in the examination of kidney cases, but this routine ought to be constantly undergoing modification to suit the requirements of any individual case under examination. It is on these lines, and these lines only, that one can reduce X-ray mistakes to the smallest possible percentage.

The Static Wave Current.

By F. Howard Humphris, M.D.

Perhaps some apology is needed for bringing before your notice any matter pertaining to static electricity, since in this country this form of electricity is much neglected. Discovered many years before any other form, it fell into disuse owing to the lack of knowledge as to how to construct a reliable static machine and the comparative ease with which the galvanic and faradaic batteries can be made and kept in order. To-day it is different, and reliable static machines are easily obtainable. Static currents are replaceable with no other form of electricity, and, with a competent knowledge of their uses, therapeutic effects can be obtained from them which appear marvellous to any one using them for the first time. To get these effects consistently, however, two factors are essential :—

(1) A proficient knowledge of static electricity and of the nature of the disease to be treated ; and

(2) A static machine of efficient power.

I think in this country the medical profession in general, and the specialist in electro-therapeutics more particularly, have not given this subject that same careful consideration which has yielded them success in other branches of therapeutics.

Some of the advantages of static electricity are that the applications are for the most part not unpleasant, that they are perfectly safe, and that results obtained are apparent from the beginning of the treatment.

On the evolution of the static machine, of its many uses and the various currents which can be derived from it, I cannot dilate now. To-night I propose to describe the technique of the static wave current and the class of case in which it is indicated. Perhaps it is the most generally used and the most useful of static currents. It is also known as the "Morton wave current," as it was first introduced by Dr. Morton, of New York.

The technique of this current is as follows: The electrode may consist of either metal, clay, or metallic cloth, applied to the skin or mucous membrane of the patient, who is seated on a platform which is insulated with glass legs some 12 in. long. This electrode is then connected with the positive sliding terminal by means of a copper wire; the negative sliding terminal is similarly attached to a good earth connexion (fig. 1). The terminal balls (brightly polished) being closed, the static machine is started, and the operator, standing on the grounded side of the machine, very slowly draws out the sliding rod, thus separating the terminals. Sparks will flow between the terminals, forming a spark-gap, which should be gradually increased up to the tolerance of the patient. The length of the spark gap is governed by the size of the electrode and the sensitiveness of the patient, and also by the relative humidity of the atmosphere. The electrode should be kept in close apposition to the part treated; this may be done with a bandage, a cushion, or, what I have often found very useful, an ordinary hot-water bag. Any clothes or linen between the electrode and the patient cause a burning sensation, due to the current passing in the form of short sparks through the obstacle. Even if the skin be very dry, it is well to moisten it or the electrode with a little soap and water. After the current has passed a short while the skin becomes moist and allows a larger volume to pass through. It is advisable for the patient to remove articles containing metal, such as hat pins, hats with metallic foundations, and even bodices if they have metallic braid or gauze upon them, because a current is induced in these metallic points which materially interferes with the comfort of the patient. Summing up the points of technique:—

(1) An efficient static machine.
(2) A sufficiently insulated platform.
(3) A suitable electrode in close apposition.
(4) Removal of metallic clothing.
(5) A suitable grounding attached to the negative side.
(6) The patient connected to the positive side.
(7) The gradual opening of the spark-gap.

The average time for the application of the current is twenty minutes. In some cases during the last five minutes, instead of keeping its spark-gap at its maximum, the rods may be gradually opened and closed so as to produce a surging effect. The current then, starting from zero, gradually swells to a maximum and returns to zero in the same way, thus contracting and relaxing the tissues beneath.

Turning now to the indications of this current. When properly applied, there is a local vibratory effect, and muscular contraction takes place; local congestion is relieved, secretion is increased, and local pain is modified, either wholly or in part.

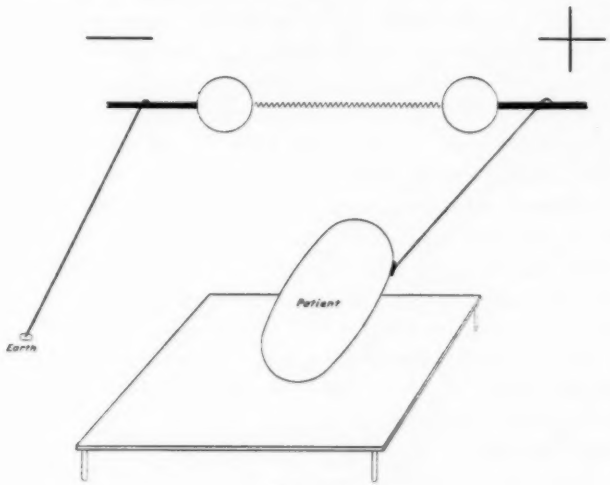


FIG. 1.

It is indicated in almost every case of inflammation, either acute or chronic; the presence of pus, however, distinctly contra-indicates its use. Inflammation is accompanied by congestion or stasis, and it is this stasis which is relieved by the passage through it of static electricity. Hence it is not difficult to understand how wide a range of influence is possessed by these currents. Of the diseases in which they are frequently used, let us take for an example neuritis, such as sciatica. Here we have an increased vascularity, a multiplication of tissue elements, and the presence of migrated leucocytes within the nerve-sheath pressing on nerve-tubules, giving rise to pain. With the current properly applied, tissue contraction takes place and the leucocytes are

squeezed out, the congestion is relieved; hence relief is obtained from the pain. A typical case is as follows: Dr. E. W., aged about 50, had a history of frequent attacks of pain radiating down the length of the sciatic nerve. This had extended over a period of four or five years. During the last six months there had been almost constant pain in the calf of the leg which had interfered with the patient's walking, his working, and his sleep. I applied the wave current from a powerful influence machine (four-tenths of a milliampere and 750,000 volts), preceding it with a short administration of light from a high-power incandescent lamp. The metal electrode covered most of the gastrocnemii, the soleus, and the peronei. The current flowed for twenty minutes, at the end of which time patient felt his leg to be much lighter and little or no pain in it. This was on November 18; on November 20 he came again and reported that there was a very considerable diminution in the severity and duration of the pain, and that he had had two good nights and that it was a pleasure to wake up in the night to realize that he was free from pain. The treatment was repeated. On November 20 he reported continued improvement with pain only now and then, with long intervals of freedom. Treatment repeated. On November 29 there was hardly any pain, his improved walking was the subject of comment amongst his friends. I think he has had no treatment since then, and has had no return of the pain.

In myalgias, such as lumbago or stiff-neck, static electricity is almost a specific, many cases being cleared up entirely in one, two, or three applications. A typical case is as follows: D. L. R., aged about 30, came to me on October 27, his doctor saying he had had typical lumbago for four days. He moved with a good deal of pain, and stepping on to the insulated platform caused him to cry out with pain. At the end of twenty minutes patient stepped down saying that he had no pain whatever. The patient came on the following day with a feeling in his back which he described as more of a discomfort than a pain. The treatment was repeated, complete relief from pain was obtained, which has been maintained up to the present. It would be easy to extend the recital of similar cases.

Dysmenorrhœa is another disease in which the wave current is most useful. Here it may be administered by means of an electrode having a metal bulb placed upon an insulated rubber handle. The patient should be instructed that the bowels should be freely opened before commencing treatment. The patient, being placed in Sim's position on the reclining static chair with her back to the machine, the electrode is lubricated and

passed up the rectum, following the sacral curve, and pressed against the uterus. The handle may be held by the patient or maintained in its position by means of an X-ray tube-holder, or the current may be administered by means of a piece of block tin, or Crook's composition, about 22 gauge, measuring 4 in. by 6 in., and placed over the lumbo-sacral region. In either case the electrode is, of course, attached to the positive pole, the negative being grounded. The spark-gap is gradually increased to the tolerance of the patient, but not producing any pain, a painless pulsating sensation being all that is experienced. The duration of the treatment should be about twenty minutes, and in very obstinate cases should be given daily for the first month, three times a week during the second month, gradually decreasing the treatment with the increase of comfort at the time of menstruation. If after fair trial of this method the relief is not satisfactory, it is certain there exists some pathological condition beside congestion, and the case has become one for the surgeon. A typical case is as follows: Miss D. P., aged 26, complained of having painful periods, each lasting a week at a time. The pain began with the flow, which was scanty, and lasted over a week, the patient having to go to bed for most of the time, the greater part of most days. Here dysmenorrhœa dated from childhood. All sorts of medicine and treatment had been tried without avail; the only thing that had given temporary relief had been leeches applied just before the period. Treatment given as already described. Period came on a day before the usual time, with much less pain, and patient did not stay in bed after the second day. The flow was much freer and unaccompanied by headache. Flow ceased on the fifth day and patient felt quite well, instead of feeling slack for several days. There were nine treatments during the next intermenstrual period, which, when it arrived, was less painful than last time. In five months' time patient had painless menstruation. That was several years ago, and patient has remained well ever since.

I could quote many more cases, but they all bear a great similarity one to another. Given a case of dysmenorrhœa where there is nothing abnormal on examination (except, possibly, a uterus abnormally anteverted and not fully developed), the pain should always be relieved by this agent; the pathology in these cases being that the endometrium has become congested and hyperæsthetic, causing a spasm of the uterine muscles, particularly at the internal os. At each monthly period the added physiological congestion increases the pressure and spasm, and hence the pain. With the contraction caused by the current passing

through the electrode pressed against the uterus, the congestion is dispersed and the spasm relaxed. The mechanism is similar in prostatic hypertrophy, and the electrode similar, only that the bulb is hollowed out to accommodate the enlarged prostate. This method is only, of course, of use in the adenomatous variety, which, however, forms over two-thirds of the total number.

In the fibroid and fibromyomatous variety this treatment is not satisfactory, for, however well the alternate contraction and relaxation may squeeze out the secretion from the acini, remove the passive congestion, and stimulate the muscles, it cannot be expected to remove fibrous tissue. Relief will occur within the first six treatments, often earlier, and patients will frequently remark that they have lost the dragging-down sensation at the end of ten minutes of the first treatment.

Another condition in which this current and the rectal electrode is of striking service is chronic constipation in which there exists—either as a cause or a concomitant—a weakness, atony, or paresis of the muscular coats of the intestine. Our President bore striking testimony in regard to this in his opening address at the beginning of this session. I think he reported 100 per cent. benefited, and remarked that the treatment was a revelation to some of the patients, in two of whom constipation was absolute. My own experience has been most satisfactory. In 1904 I read a paper on static electricity before the medical society with which I was then connected, and mentioned in it the results I was then having. At the close of the meeting Dr. N. B. E., aged about 70, came to me asking me to undertake his cure, though he was very sceptical. Here, as in Dr. Sloan's cases, mechanical means had to be employed to empty the rectum, and medicines and diet had proved of no avail; constipation had existed for fifty years. I cannot recall the number of treatments he received, but he noticed benefit within seven days, and at the next annual meeting bore testimony to the efficacy of the treatment, saying that he was getting no treatment at that date, taking no medicine, but getting a daily evacuation of the bowels. This improved condition had lasted up to December, 1908, when I last saw him.

The rectal electrode for constipation which I use was devised by Dr. Snow, of New York, and, as you see, varies somewhat from those used for prostatitis and dysmenorrhœa.

In only very obstinate cases have I had to resort to the static-induced current. In sprains the relief afforded by this current is exceedingly striking. Relief from pain and power of movement are obtained within half an hour. Mr. S., a dental surgeon, sprained his ankle nine days

before coming to me. He complained of much pain in walking, especially along the tendo-Achillis, and that he could not stand at his work owing to the pain; after two treatments he telephoned me that he could walk for four or five hours without pain. Colonel K., leaving me after a treatment for another trouble (synovitis with effusion), sprained his wrist getting out of a motor-bus. His hand was useless on arrival, he had been unable to sleep on account of the pain, he could not shake hands or button or unbutton his coat, the hand was swollen and exquisitely painful. After twenty minutes of the wave current he could use his hand to button his clothes, and the pain was no longer acute. The next day he reported a good night, the pain was then limited to an area the size of half-a-crown, and the patient could use his hand with only a certain amount of tenderness. He shook hands on coming in and easily lifted a heavy candlestick.

In locomotor ataxia the application of the wave current has accomplished results when nothing else seemed of any avail. Pain is relieved, the ataxic gait is improved, the progress of the disease is arrested, and the general condition of the patient is altered for the better. The current, however, makes no difference to the Argyll-Robertson pupil nor to the knee-jerks. In June of last year I saw Mr. B., a stockbroker, aged 46. His history briefly was this: About three years ago, while washing his face, he noticed he could not stand. Soon after he began to have lightning pains, and was told he had locomotor ataxia. This was confirmed by several eminent specialists. When I saw him the case was typical: he had the significant gait, the Argyll-Robertson pupil, his knee-jerks absent, and Romberg's sign was very distinct. He complained chiefly of the pain, of which he gets attacks utterly incapacitating him, and lasting seven or eight days. From the beginning of the treatment the pain decreased and his general condition improved. On August 19 he had had twenty-three treatments in all. He had not been one day without attending his business, but had some very heavy work in front of him and wished to suspend the treatment to see how he could get on without it. He returned on October 4 saying he had been one of two doing the work of five men, but now had some circumflex neuritis; this was cured in two or three days. I saw him with his doctor the other day. His doctor remarked how very much better he was walking, and the patient said he had not been laid up since he came, and was very busy, but would come back to me if ever the pain returned.

I could quote many and varied cases in which I have obtained results with the aid of this current—cases of enlarged spleen reduced to

one-third of the size by this treatment, cicatrices rendered less adherent, the pain from pleuretic adhesions relieved, other cases of such varying nature as rheumatoid arthritis, infantile paralysis, dyspepsia with dilatation of the stomach—in all of which I have proved the utility of the wave current, but already the time allotted to me has expired. The fact that I have been unable to cover the ground of the indications for this current is my excuse for not having mentioned any other forms of static electricity, as in the title of this paper.

I thank you for the courteous attention which you have accorded me.

DISCUSSION.

The PRESIDENT (Dr. Sloan) said the paper was so interesting that one could have wished the author had gone on to speak of several other kinds of current. As he had remarked, his (Dr. Sloan's) experience had been more with the static induced. He was surprised, and yet not much surprised, that he made no mention of the static bath. He assumed, from the lack of reference to it, that it had fallen into disuse. He (the speaker) recently made some physiological experiments with the static bath, and he found that, whether the patient was normal or abnormal—in one case the patient was perfectly normal, whilst in another the blood-pressure was reduced, through temporary cardiac failure, from 200 to 170—no change was made in the pulse-rate, the blood-pressure, or the mouth temperature by these static baths, either positive or negative. His own experience of the wave current had not been large; it had been more a matter of experiment. But he intended to use it for the kind of case which Dr. Humphris had mentioned, and he hoped he might find as good results as the author had. He was surprised that the current Dr. Humphris got was four-tenths of a milliampere, as all that he (the speaker) could get out of his own was 0·05 to 0·1 of a milliampere. He would like to know what the author's primary sparking distance was. His own was 8 in. to 10 in., reduced to about 2 in. under the wave current, because as soon as the one pole was negatived, the sparking distance was made much closer, as only one of the prime conductors then had a high potential, the other being zero. Perhaps more members would use the current after having heard the paper, and he would be glad to hear from members some scientific data which might give greater confidence in the use of static currents.

Dr. HORACE MANDERS asked Dr. Humphris what particular current he had been describing in his paper. The diagram shown was not what he had always understood the Morton current to be. He had regarded it as a current taken from the outer armatures of two condensers fed from a static machine; on one side the electrode to the patient was taken from the positive, and the

negative was grounded. On that diagram there was a unidirectional continuous current of very high potential and very small milliamperage. Dr. Humphris had obtained a larger quantity than most people were able to do, and the reason seemed to be that he used very big plates indeed.

With the current described, certainly very much could be done with sciatica, because the *one* treatment of sciatica was by the continuous current, though not necessarily of so high a potential. He believed that treatment of sciatica by the continuous current was first introduced by Sydenham J. Knott, though he might have originally got it from de Watteville. Mr. Knott brought it in about the year 1876, and he had a run of very successful cases in St. Mary's Hospital. He (the speaker) had never known a case of sciatica which did not yield to the continuous current. If an acute case of sciatica were treated with the faradaic current, it was thereby made much worse. He believed the good results from the static current in sprains and effusions were due to the fact that a unidirectional current promotes absorption. And he did not see why, with a current of that intensity, cases of pleurisy should not receive very great benefit—i.e., cases which would not otherwise clear up, and where there was a thickening of the pleura. Lumbago certainly yielded to the Morton static induced current, but chiefly to the alternating current, such as the faradaic or the more rapidly alternating current of high frequency. He thought the static treatment should do great good in the paralyses of children, especially those after acute anterior poliomyelitis. He had had some very interesting results in the prevention of talipes by the use of the continuous current. But he thought that if there were used an exceedingly penetrative power with small quantity of current there would be better results in the case of little children, because little children did not stand the continuous current well; even with a fraction of a milliampere they would say "it burns." On the other hand, when they had palsy from acute anterior poliomyelitis, they would stand any amount of faradaic current without complaint. He would like to have a static machine to try the effect. He asked also why there should not be ionization with the continuous current of that intensity. Many people objected to ionization because it was so painful, but then as much amperage as the patient could stand is generally used. The true Morton current, taken from the outer armatures of two condensers, is a peculiar current; there is no other like it. He had personally tried the effect of the current from condensers with the ordinary high-frequency arrangement, but without the solenoid, as he showed before the old Society five or six years ago, and he did not like it at all. But taken off a static machine, the result is not unpleasant. It produces good muscular contraction with a minimum of pain, and for certain diseases it is a first-class remedy. It is essentially a displacement current swinging between the two armatures, but it has to pass through the body, and the body has an enormous resistance (something like 30,000 ohms); consequently the discharge having got through such a resistance cannot get back, and the condenser discharges itself in nearly one go: so there is produced an alternating current with

rapid discharge. It is not a truly oscillatory current, but an alternating current with considerable rapidity and of high potential. The condensers cannot charge up very rapidly, because the amperage is so small in static machinery that it takes a comparatively long time to fill the condensers.

Dr. E. WAGSTAFF (Leighton Buzzard) said he could offer a most practical experience of the use of the static current, as Dr. Humphris had completely and absolutely cured him in a short time after a very long period of suffering. He had suffered severely from pure lumbago, and had had muscular spasms extending to the heels for ten or twelve days at a time, so badly as to confine him to bed. An attack in June last year left a deep-seated lumbar pain, and as he had suffered from nephritis in his time he thought it must be due to that. The pain settled down in the left leg, and he was often acutely conscious of the whole distribution of the sciatic nerve. He was unable even to cough when in bed without sitting up. Dr. Humphris had understated his (Dr. Wagstaff's) case. The first time he came he walked only with the greatest difficulty, and he had not walked a quarter of a mile with any comfort for six months. But when he came out of Dr. Humphris's room he walked straight back to Euston. Since that time the whole pain he had endured had not amounted to an hour, and he had been sleeping well and doing his work in comfort.

Dr. CURTIS WEBB said he had had six years' experience of work with the static current. He quite agreed with Dr. Humphris's definition, and also with the definition of Dr. Titus, the late President of the American Electro-Therapeutical Society, as to the meaning of the Morton wave current. That which Dr. Humphris had explained on the board was what he himself always understood as the Morton wave current, as the antithesis of the Morton static induced current. The static induced current was an alternating current; and might be compared to, but was not identical with, the high frequency. The one described was a pulsating, continuous current, and he had obtained with it very similar results to those of the author. His practice was a general one, and he treated by the static method those cases which he regarded as suitable for it. For neuritis, sciatica, and myalgia, there was no treatment to compare with it. One case was that of a practitioner who had been house-physician at one of the large teaching hospitals. When he went through the hospital course he developed sciatica. He was treated by stretching by the closed method twice under an anæsthetic, and he had the advantage of the electro-therapeutics of the hospital, but did not derive any particular benefit. He spent two years at spas, and after that came into his (Dr. Webb's) hands. After three weeks' treatment of the wave current he was completely cured of his pain. That was about four years ago, during which time he had held an important post as senior resident officer at another hospital. He had a relapse about two years after the treatment, and came for treatment the following day, and after two applications he was all right again. He did not regard him as an hysterical man; he was a very healthy specimen. His own machine was a twenty-four-plate machine—twelve revolving, and twelve

stationary—of 36 in. diameter; he got three-tenths of a milliampere out of it with fair regularity, and he could run up to 500,000 to 600,000 volts, measured by an instrument which Dr. Humphris lent him, which was brought over from New York by Dr. Titus. There was a variety of cases for which static electricity could do an enormous amount of good. Some were acute cases, and he had had several of them. One patient was in Edinburgh, where he had an accident, apparently dislocating his shoulder; the late Professor Annandale was called in, and the description given was the classical one of reduction of the dislocation. When he (the speaker) took the bandage off, two days after the accident, he moved the arm from the side, and after getting about 3 in. from the side it caused the patient fairly acute pain. He applied the static wave current over the shoulder for about twenty-five minutes, and at the end of that time he could move his arm just beyond a right angle without pain. In ten days he was doing his work; he could write and dress himself, but could not button his collar at the back; he was not able to do that for three weeks. The mobility of the shoulder was quite restored. Another case was that of chronic synovitis in a well-known athlete who was a good golfer; at the end of six treatments by the current he was able to take part in a golfing tour at Easter. Another curious case was that of vomiting of pregnancy, and it might possibly be said that such cases were those of pure suggestion. But it was so bad that a consultant was called in, and it was a question whether labour should be induced. As a last resort, he thought he would try the treatment under discussion. He applied the wave current over the epigastrium; after three treatments vomiting, except in the mornings, ceased, and she continued well. She had a child at full term, who was now aged 5. Another case was one of prolapse of the rectum, of which he had had two cases, unassociated with piles, and complete cures had been effected by the static wave current, with a similar electrode to that shown by Dr. Humphris that evening. There were two other interesting cases, both in unmarried women, who suffered from chronic backache. In neither instance was it associated with dysmenorrhœa, and there was no history of uterine trouble. One was treated seven and half years ago, and there had been no return of the symptoms. Another case was that of a gentleman who had spent many years in the East, and who suffered from liver trouble for four years after his return, and also had constipation. The wave current applied over the liver region removed the symptoms. Since then he saw him twice a year for a few treatments, but he had not treated him for a year, and he knew that he had kept well. One could not be positive whether the improvement was due to the patient having left the East or to the application of the static current, but as the sluggishness of the liver had persisted in spite of ordinary medical treatment for those four years, and it was relieved after a course of static wave applications, it seemed a reasonable presumption that the latter had played a not unimportant part in the cure.

The PRESIDENT remarked that the Morton static induced was a bipolar, whereas the Morton wave was a unipolar current.

Dr. HUMPHRIS, in reply, said that when the milliampere meter gave a reading of four-tenths of a milliampere, the spark-gap was 8 in. or 9 in. long. He had made great use of the static bath or static insulation, negative to platform and positive grounded. He quite agreed with Dr. Sloan that it was difficult to see how this current could act on the body, and was aware that experiments had been made which gave negative results as to any demonstrable physiological action; but clinically he had seen results suggesting the hypothesis that this powerful current surrounding and flowing over the surface of the body might stimulate certain nerve endings in the skin, and produce a sedative effect on the internal economy. Some misapprehension appeared to exist as to the Morton wave current. Fig. 1 is the diagram of the Morton static wave current, and fig. 2 of the Morton induced current. The static induced current, in which Leyden jars are necessary, was introduced by Dr. Morton, of New York, in 1881. Here a rapid flow of noisy sparks passing between the discharging

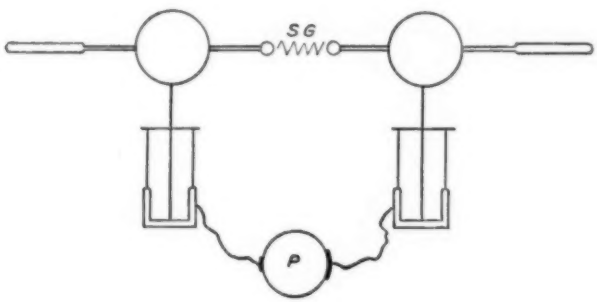


FIG. 2.

rods, the wires connected to the external armatures, are the seat of current phenomena. As to ionization by means of static electricity, he was not convinced that it was practicable; it was preferable to use galvanism when we wished to introduce ions into the system. The only time that he thought he had introduced any drugs into the system by means of static electricity was in a case of tubercular laryngitis. Here creasote was applied to the outside of the throat by means of a specially devised electrode. The patient on several occasions stated that he could taste the drug, and also one could apparently detect the odour of it in his breath. But here there may have been a fallacy.

Electro-Therapeutical Section.

March 18, 1910.

Dr. A. D. Reid, Vice-President of the Section, in the Chair.

Examination of the Intestines by Means of Röntgen Rays, both under Normal and Pathological Conditions.

By Franz M. Groedel, M.D.

It is well known to you that only those organs can be examined by means of Röntgen rays which possess a greater or less power of absorption than their surroundings. Where no such natural difference exists we must create an artificial difference, unless we prefer to renounce the use of the Röntgen rays altogether. Frequent use of this artificial means of investigation is made when examining the intestines. The intestines cannot be distinguished on the screen, either when full or empty, the only exception being such parts as are filled with gas temporarily or permanently. This is especially the case with the two flexures of the large intestine and the upper part of the stomach, the so-called "Magenblase," which is usually full of gas. This fact led to the idea of filling the whole intestine with gas; in other words, to the idea of making the organs which are to be examined visible in an artificial way by reducing their density. This method, however, proved to be of little real utility, as the form and position of the organs were thus rendered abnormal. Experiments were then made to produce an opposite effect by introducing into the alimentary canal materials which increased their density—that is, their power of absorbing Röntgen rays. Of these materials I will only mention ordinary sounds or sounds filled with shot or quicksilver, sounds made of plumbic rubber, metallic salts, such as iron, thorinic acid, oxide of zinconium, and, last of all, bismuth—the latter diluted with water or milk, given in capsules, wafers, biscuits, or mixed with the food.

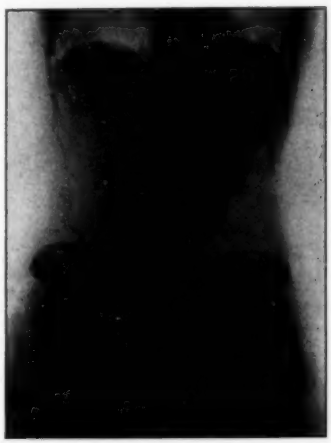
The method which is used most extensively, and is sufficient for nearly all purposes, is the Rieder-bismuth meal. The Rieder meal should be of the consistency of porridge. It may consist of boiled rice, barley broth, potato purée, spinach, or minced meat, according to the fancy of the patient. Forty to fifty grammes of bismuth carbonate must be well mixed into a smooth paste with water or milk, and the broth should be added, little by little, to the bismuth suspension. The amount of the meal for an adult should weigh 400 grm. The taste may be disguised by the addition of the juice of raspberries or other fruit. Here I may discuss the advisability of introducing so large a dose of bismuth into the digestive tract. Rieder was the first man who had the courage to give such an enormous quantity of bismuth. But a number of observations have shown that bismuth subnitrate, the salt first employed, occasionally gives rise to severe poisoning. There have even been several cases reported in which death has occurred after the administration of bismuth subnitrate.

I was the first to employ the carbonate in my investigations; numerous institutes have since followed my example. In Germany this salt is now used almost exclusively, and ample experience has shown it to be evidently quite harmless. Of the bismuth oxychloride, recommended by Dr. Arthur Hertz, I have no personal experience.

I will not now go into the technical details of the examination, but only mention that stomach and bowels are always examined standing. The most important part is the Röntgenoscopic—the observation of the picture on the screen. For fixing the exact form of the stomach, orthodiagraphy can be especially recommended. Röntgenography—i.e., the taking of a photograph—is only necessary for insuring a correct diagnosis in doubtful cases. It is very interesting to watch the changes that take place in the stomach during a meal. The empty stomach appears like a flabby, folded tube, and therefore cannot be reproduced by the Röntgen rays. We can only see the "Magenblase"—that accumulation of gas that is mostly to be observed just under the left diaphragm at the highest point of the stomach. The first morsels of food appear like dark shadows at the bottom of this "Magenblase," which is seen on the screen as a bright disk. Under normal circumstances they only remain there a short time. Then we see the morsels gliding slowly downwards. The following morsels move more quickly, and the food collects at the bottom of the stomach. Soon, however, the whole stomach fills up and becomes visible in all its parts. Should more food be taken, the different parts of the stomach expand at the sides. As

I have already mentioned, a normal stomach will be completely and uniformly filled up to the "Magenblase" by a normal bismuth meal (400 grm.)

Of late years there has been much discussion as to the normal form of a stomach filled with a Rieder meal. Rieder himself said that the stomach had the form of a fish-hook. Holzknecht has maintained that the more rare but *only* normal form of the stomach is that of a bull's horn. I myself, after having examined a number of persons with healthy stomachs, have found the Rieder form to be the normal one, and from its function have termed it the syphon form. It is this syphon form which permits most easily the letting off at will of the properly prepared food in the intestines.



SKIAGRAM 1.

Normal stomach.

Skiagram 1: When observing the picture of a normal stomach, taken in a standing position and filled with food, we can distinguish the following points: Beneath the diaphragm we see the "Magenblase." It is clearly marked by a horizontal line against the shadow of the bismuth food. We can distinctly follow the course of the great and small curvatures. In its entirety the stomach has the form of a hook, almost uniform in breadth; we see a long descending part and a shorter ascending part; both are connected by means of the "Magensack." Towards the duodenum the stomach is bounded by a sharp, clear line—the

pylorus. Beneath the pylorus we see, from time to time, a contraction of the shadow which is caused by the sphincter antri. Besides this very interesting but not yet sufficiently studied peristalsis of the antrum, we can also distinctly observe the peristaltic waves.

Skiagram 2: Here we have a more schematic picture. It is taken from a stomach-orthodiagram.

Skiagram 3: I have already mentioned that for a short time after the stomach begins to take food it only expands at the sides; its length, or rather height, does not increase. The reason of this is that the stomach hangs loosely between the cardiac end and the pylorus, and is bedded on the small intestines. The form of the stomach is the result of the concentrated pressure of the surrounding organs and the gravitation of the food. If the stomach were a loose sack, the ingesta would collect in a mass at the bottom; but, as we have seen, the bismuth meal fills the stomach in a uniform way to the highest point. This is accomplished by the tone of the stomach muscle—the peristole. Thus we see in the next picture that even after a double bismuth meal—that is, when the stomach is charged with 800 grm.—it has indeed become very much wider, but not longer (*see* fig.).

Skiagram 4: Very interesting pictures of the stomach are also obtained by transverse illumination (e.g., from right to left). In this picture, which is technically very difficult to take, you see that the stomach extends from high up at the back to low down in front. The "Magenblase" lies behind the heart. Here, too, the stomach is more extended in the upper than the lower parts, owing to the peristole. The shape of the stomach changes, of course, with every position taken by the body, as the stomach hangs loose. The factor upon which the form is chiefly dependent is the force of gravitation. We find a slight variation in the form of the stomach in almost every individual, but it is always a syphon form. The numerous individual varieties of shape are caused by the different space which exists in the abdomen of every person. Unfortunately, I cannot now enter more fully into this most interesting subject.

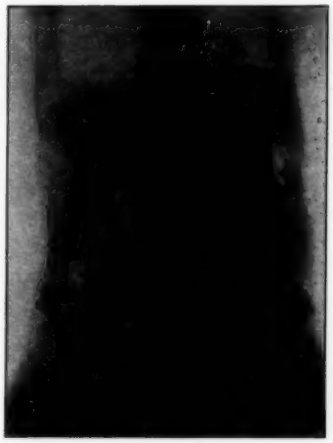
Skiagram 5: As one example only, I will show you a picture of a contracted stomach. The lady had a curvature of the spine and had therefore been lacing tight. You can distinctly see the curvature on the screen. The pylorus is to the left of the spine. The lacing-in of the waist corresponds to the contraction of the stomach.

Skiagram 6: Of the numerous kinds of changes that are produced in the shape of the stomach by outward circumstances I will only illustrate

the best known, the so-called "hour-glass" stomach. In such pictures you can distinctly see the division of the stomach into two parts caused by a scar or by perigastritic cords.

Skiagram 6*a*: Here is another case of an hour-glass stomach.

Of the affections of the stomach, gastroptosis is one of the most frequent. As I have already stated, there is no sinking of the whole stomach, but only of the pylorus. The pyloroptosis can, as I have found, very easily be detected by means of Röntgen rays. If we let a patient with a depressed pylorus draw in his abdomen, the pylorus is moved upwards. We also see, if we press in the lower part of the abdomen, that the pylorus is pushed high up. (Skiagram 7): This picture of a



SKIAGRAM 3.

Normal stomach after a double bismuth meal.

patient with pyloroptosis shows you the pylorus far below the umbilicus. (Skiagram 8): In the next picture, of the same patient, you see the pylorus high above the umbilicus, because the lower part of the abdomen has been pressed in.

A deep caudal pole of the stomach is not necessarily a symptom of gastroptosis. It occurs as frequently in cases of gastrectasia.

Skiagram 9: I distinguish three kinds of ectasia, according to their three distinct and characteristic Röntgen pictures. The first class I call "Belastungsektasie," which means ectasia caused by overloading the stomach. In these cases the stomach is extended lengthways, because

its support, the bed of the small intestine, has sunk down. But in this picture, as you see, the stomach is quite full and in all its parts visible; therefore the peristolic function is intact.

Skiagram 10: The next slide shows a very different state of affairs. The stomach is indeed a little elongated, but, above all, it is expanded. The meal fills up the lower part of the stomach in particular. The upper section is expanded in a very abnormal manner by gas.

Skiagram 11: The same characteristic features are to be seen in the following picture. In such cases the influence of the peristole is entirely missing. The stomach can no longer bring its sides in contact with the meal everywhere. I have therefore called this form of disease the atonic ectasia. The caudal pole is here often in its normal place.



SKIAGRAM 16.

Circular carcinoma of the stomach.

Skiagram 12: If you give such a patient a double bismuth meal, it does not change the form of the stomach; the stomach always seems to be insufficiently filled. Here you see a picture of such a case.

The third form of ectasia I shall have occasion to speak of later on. Of course other important symptoms for the fixing of the diagnosis of ectasia are to be found in the filling of the stomach, the change of position, the compression of the abdomen, and so on. It would carry me too far, however, if I were to speak of them all here. I will only call attention to one fact, and that is that we cannot prove the atony of

the stomach by means of Röntgen rays. Perhaps atonic ectasia was formerly considered to be a simple gastric atony. If the position of the diaphragm is abnormally high, it is not, in my opinion, caused by gastric atony, but by an affection of the diaphragm. I will here demonstrate two cases of especially aggravated affections of the diaphragm—of eventratio diaphragmatica. (Skiagrams 13 and 14.) Coming to the question of the Röntgen diagnosis of gastric tumours, or rather of gastric carcinomas, I will not detain you by enumerating the many Röntgen symptoms. The most important of these symptoms is the defect in the shadow of the stomach when full. I will now show you the most frequent forms of this stomach disease.



SKIAGRAM 21.

Carcinoma of the pylorus.

Skiagram 15: If the carcinoma is seated in the fundus of the stomach, it is quite possible that it will not be reproduced. We often detect it in the irregular invagination of the great curvature.

Skiagram 16: The circular carcinoma often causes an early complete division of the shadow of the stomach, as will be seen in the next picture (*see* fig.). Carcinomas of the great curvature often cause a great diminution in the lumen of the stomach. (Skiagram 17.) At other times they make distinct vaginations of the shadow of the stomach. (Skiagrams 18 and 19.)

Skiagram 20: Carcinomas in the region of the pylorus produce an extremely great variety of pictures. If the whole regio pylorica is attacked by circular carcinomas, the stomach has at the end the appearance of a jagged or notched line.

Skiagram 21: Very often we find (*see* fig., p. 115) that the pylorus is attached to the liver by the carcinoma. The pylorus is then mostly destroyed and we see the bismuth shadow of the stomach passing over into that of the duodenum without any boundary line.

Skiagram 22: The picture of the pyloric carcinoma with stenosis is particularly striking. The stenosis produces a highly motoric insufficiency. The permanently full stomach becomes dilated and the muscle becomes hypertrophic. This third form of ectasia I call megalogastria. In these cases the stomach is indeed like a flaccid sack. A normal meal only fills a small part of the stomach, and appears with a horizontal upper boundary-line. The carcinoma is to be recognized by the indistinct lateral boundary-line of the shadow.

Skiagram 23: In especially bad cases the narrow horizontal shadow of the bismuth appears just over the symphysis.

We are also able to investigate the function of the stomach by means of Röntgen rays, in the simplest way, by fixing the time of evacuation. A normal stomach becomes empty, as a rule, after from two to six hours.

I will now proceed to the examination of the intestines. Here there are two ways at our service. We can either follow the bismuth meal, by means of repeated examinations, on its way through the intestines, or we fill the large intestine with a bismuth injection. The first method serves especially for the important investigation of the motility of the intestines, and also for ascertaining the situation of the small intestine. The bismuth injection, on the other hand, can only reveal the situation of the large intestine, or the seat of a stricture. First of all with a few slides I will demonstrate the position of the normal intestine.

Skiagram 24: Almost directly after taking the meal we see bismuth shadows in the pars horizontalis superior duodeni. They are separated from the shadow of the stomach by the contracted pylorus.

Skiagram 25: The next slide shows you a rare case, in which the pars horizontalis superior and the pars descendens duodeni become distinctly visible (*see* fig.). The small intestine is seldom visible to any great extent; the chyme passes quickly through it. As a rule, we only see a few cylindrical shadows below the stomach.

Skiagrams 26, 27 and 28: The next three slides belong to a rather rare case, with sluggish motility of the small intestine. You see more and more of the curves filling and the shadow of the small intestine becoming denser and denser. If we watch the bismuth meal long enough we find the colon full. The striking prominent segmentation—the "Haustren," as we call it—is the most characteristic mark of this (*see* fig., p. 118).

Skiagrams 29 and 30: A bismuth injection gives us much more quickly a correct knowledge of the position of the colon. We give an injection, in a lying or kneeling position, of about 80 gr. of bismuth carbonate and 300 gr. of bolus alba to 1 litre of water. As pictures of this kind show, the large intestine is completely filled and we can examine each of its sections.



SKIAGRAM 25.

Rare case in which the duodenum became visible.

The next series of slides will illustrate the motility of the intestines.

Skiagram 31: Here you see the stomach of a girl, aged 16, taken directly after the meal. Skiagram 32: After a quarter of an hour some of the windings of the small intestine are filled and visible as cylindrical shadows. Skiagram 33: After half an hour the filling of the small intestine has increased. Skiagram 34: After the lapse of no more than one hour we can see bismuth shadows in the cæcum. Skiagram 35: At the end of the second hour the ascending colon is almost completely full. Skiagram 36: After six hours the ascending colon is crowded; the whole

bismuth meal has collected in it. Skiagram 37: Eight hours after the meal only the upper part of the ascending colon is full. The first half of the transverse colon is now visible. Skiagram 38: After exactly twenty-four hours the entire transverse colon is full. Skiagram 39: Forty-eight hours after the meal the chyme has accumulated in the descending colon and the pelvic colon. By the defæcation, which now follows, everything is evacuated.

The most frequent disturbance in the motility of the intestines—constipation—is usually accompanied by a change in the form or position of the colon. Dr. Hertz has, in his well-known book,[1] given a detailed description of this disease, and therefore I will give but a brief summary.



SKIAGRAM 27.

Stomach and small intestine.

Skiagram 40: Here you see a very ordinary case of atonic constipation, caused by a coloptosis. The transverse colon has assumed the form of a letter **Y**. We can clearly see that it is too long.

Skiagram 41: In other cases the coloptosis and the constipation are to be attributed to a sinking of the flexures. This slide shows a case of aggravated sinking of the right flexure.

Skiagram 42: In cases of spastic constipation you often see a contraction of the transverse colon, as in this picture.

[1] "Constipation and Allied Intestinal Disorders," 1909.

Skiagram 43: More typical, however, are the cases in which the contents of the intestines are often retained for weeks in the region of the hepatic flexure. Then we can see the scybala wandering separately and slowly through the colon.

Skiagram 44: In conclusion, I will show you a case of the most frequent **W**-shaped coloptosis, in consequence of a too elongated transverse colon. In this case a bismuth-bolus injection was used (*see* fig.).

I have now arrived at the conclusion of my paper. I have only been able to give you a short summary, illustrated by a very small number of my slides, of all the possibilities of Röntgen rays in the examination of the intestines, and the formation of a correct diagnosis. I have had to



SKIAGRAM 44.

Coloptosis (bismuth-bolus injection).

refrain from mentioning many an important fact, and no less important symptom, that I might not trespass too much on your time. I hope, however, that I have convinced you of the great importance of this new method of investigation for the physiology and pathology of the intestines.

Some of the pictures shown have been made with the high-tension transformer, and some with spark coils of Reiniger Gebbert and Schall, and a 3-anode Wehnelt break of the type described by the author. The exposures were between one and two seconds while the older intensifying screens were used, and a quarter to one-third of a second when the new type of intensifying screens of Reiniger Gebbert and Schall was used.

DISCUSSION.

Dr. HERSCHELL asked as to Dr. Groedel's experience of the time which it took a bismuth meal to reach the different parts of the intestine. In describing the slide which was said to represent a normal person, it was stated that the bismuth meal reached the cæcum in two hours after the meal was taken. Dr. Hertz, in his book on constipation, said that the normal time for a bismuth meal to reach the cæcum was four hours after the meal: two further hours to reach the hepatic flexure, and two hours more to get to the left iliac region. How did he account for the discrepancy?

Dr. HERTZ thanked the author for his very interesting demonstration. Like Dr. Herschell, he wished to ask whether the author could explain the considerable discrepancy between the rate at which the cæcum was reached in the case which he had described and that which he (Dr. Hertz) had found was normal. In his experience the bismuth never reached the cæcum after such a short interval as two hours; the average being about four hours, and it might be four and a half. Had this particular patient any diarrhœa? Possibly the shortened time might be due to his having been given the bismuth in spinach, which was stimulating to the intestine and passed through it more rapidly than an ordinary mixed meal, and quicker than the bread-and-milk or porridge with which he (Dr. Hertz) gave the bismuth. He asked whether the lecturer examined his patients in the horizontal as well as in the vertical position. He believed that one could see the peristalsis in the neighbourhood of the pylorus and the bismuth passing into the duodenum more easily when the patient was lying down; and by the double examination one could make interesting comparisons between the position of the stomach in the horizontal and the vertical positions. A stomach might be in the normal situation when the patient was lying down, so that it appeared to be normal when examined by ordinary methods of physical examination, such as percussion after insufflation; but as soon as the patient stood up there might be an extreme degree of gastroptosis. In other cases of gastroptosis the stomach was already abnormally low when the patient was lying down. Dr. Groedel had spoken of spastic constipation; he (Dr. Hertz) had only seen one well-marked case with the X-rays, and that showed a very different appearance from that demonstrated by Dr. Groedel; the ascending and transverse colon were normal in size, while the whole of the descending and iliac colon were seen as a very fine line, like the shadow of a wire. This spasm, extending over a considerable length of intestine, corresponded better with what was generally regarded as spastic constipation than the condition shown by Dr. Groedel.

Mr. THURSTAN HOLLAND agreed with the last speaker in most of his remarks. It had occurred to him that the time stated for the passage of the meal through the intestines was much too short; the case mentioned by Dr. Groedel must have been exceptional. He would have liked Dr. Groedel

to enter more into detail as to the various pathological conditions of the stomach and the appearances which they gave rise to. Beautiful radiograms were shown of what was said to be malignant disease of the stomach, and certain appearances were pointed out as evidences of that disease. But he would say, from his experience, that not one of those appearances was really pathognomic of serious stomach mischief—i.e., malignant disease. He thought all the shapes which had been shown in that connexion might arise from gastric ulcer, and he would like to hear whether it was possible by X-ray examination to differentiate between malignant disease of the stomach and old gastric ulcer, or even recent gastric ulcer. The other day he took a radiogram from a case which was sent from one of the medical wards, and he did not think he had seen a more beautiful example. It was taken in the upright position. It was apparently an hour-glass contraction of the stomach, with a very narrow band of food joining the two portions. He thought it was probably some serious disease, such as malignant disease, but operation shortly afterwards revealed only a small gastric ulcer the size of a sixpence on the lesser curvature, with much inflammatory thickening around it. It was somewhat hour-glass in shape, but there was no difficulty in pushing the fingers folded up through the opening between the two chambers. He would be very glad to know if there was any means of knowing, by means of the radiographic examination, whether one was looking at cancer or gastric ulcer. He expressed his cordial thanks to Dr. Groedel for his interesting paper.

Dr. PIRIE asked whether Dr. Groedel had seen the contents of the large intestine being quickly shot along in a way comparable to the quick passage seen down the œsophagus. Professor Holzknecht had described such an appearance.

Dr. HARRISON ORTON asked whether Dr. Groedel considered the stomach which he showed in the first skiagram was normal. He thought it was rather large in size. Many times he had seen a stomach, such as he had not seen represented that evening, in which, after a bismuth meal, the organ took a vertical position with parallel sides, and the ordinary stomach curves were absent. It was to the left of the umbilicus. He had sometimes noticed that when patients were standing up the stomach outline was normal, but on examining in the recumbent position the bismuth seemed to divide it into two parts, giving an hour-glass appearance. On reverting to the standing posture the original appearance was again seen. A point in favour of examining patients in two positions was its value in diagnosing between gastroptosis and dilated stomach. If it was merely gastroptosis, when the patient was standing up the bismuth would be seen low down in the pelvis; but after the patient was put on his back the whole stomach dropped back, and the lower border was well above the level of the umbilicus. Another point he had noticed was that in very dilated stomachs the bismuth sank to the bottom; and he had frequently seen a stomach in which there was a bubble of gas above, then a layer of fluid, and at the bottom of the stomach the shadow due to the bismuth. He agreed with Mr. Holland that shapes such as had been exhibited on the screen could

be seen where there was no carcinoma present. He would not like to say, from looking at these skiagrams, whether carcinoma was present or not. He also thought that the time Dr. Groedel mentioned for the passage of the bismuth meal was too short; his experience agreed with that of Dr. Hertz. He would be glad to hear from Dr. Groedel whether in giving the bismuth he used spinach, and whether in some he gave porridge, because the composition of the meal would play a great part in the time taken. With regard to the use of oxychloride of bismuth, that salt was used because it was thought that the carbonate, by neutralizing the gastric juice, might cause some delay in the passage of the food which would not be normally present. Dr. Hertz pointed out some time ago that oxychloride of bismuth and carbonate of bismuth passed through in approximately the same time, and therefore the carbonate was used, as it was more easily obtained.

Dr. HERTZ said that it was true that he had found that there was no difference in the rate at which the bismuth reached the cæcum, whether the carbonate or oxychloride was used; but he had recently made comparative observations with the two salts, and found that with the oxychloride the activity of peristalsis in the stomach was greater. He supposed that with both the chyme began to leave the stomach at the same time, so that the arrival in the cæcum occurred after the same interval. The greater activity of gastric peristalsis was what one would expect, considering that hydrochloric acid was known to be one of the chief stimulants of gastric peristalsis, and that a large dose of the carbonate completely neutralized it.

Dr. CLAUDE WILSON desired further information about the cases, of which two slides were shown, where the diaphragm was highly distended upwards. He recently had a case, which puzzled him very much, in which the symptoms were purely cardiac, with almost fatal angina: nothing cardiac was found, but for a few days after the attack there was an area of abdominal resonance as high as the second rib, and extending widely over the front of the chest. The heart was displaced and could be heard only distantly, accompanied by a curious clicking sound, doubtless extra-pericardial. He had seen nothing like it, nor could he find anything in the books about it; he had thought it might possibly be due to a diaphragmatic hernia. Seeing the two cases projected on the screen, in which there was evidently distension from the abdomen as high as the second rib, he wondered whether they threw any light on his own case; in the latter there were no gastric symptoms, nor was there any feeling of relief after eructations of gas.

Dr. WILLIAM EWART commented upon the frequency, in his experience, of the condition to which Dr. Wilson had referred, although he had never had so extreme a case to treat. The nature of the ordinary cases was commonly misunderstood, because the rise of the stomach and diaphragm backwards was usually overlooked. In 1899 he had demonstrated the condition before the Clinical Society,[1] the title of his paper being "Gastric Dilatation Upwards and

[1] *Trans. Clin. Soc. Lond.*, 1900, xxxiii, p. 202.

Backwards: its Relation to the Heart and Respiration, and its Treatment." Since that date he had seen many cases, in some of which the upward and backward dilatation was part of a general dilatation of the stomach.

Dr. BARCLAY thanked Dr. Groedel for his very interesting demonstration; the radiograms of the stomach were the most beautiful he had ever seen. In his own practice it was rather the exception than otherwise to find such stomachs as Dr. Groedel had shown. Like Dr. Orton, he had found that the food went to the lowest part of the stomach, and that it often stayed there. Whether it did so or not depended on the tone of the stomach, a point which the author did not sufficiently bring out. There were not many toneless stomachs shown, such as he so often saw in both private and hospital practice. In his own practice he had come across cases in which the air in the upper part of the stomach was increased—a condition called aerophagy, which the French observers had called attention to. The author did not mention the speed at which food left the stomach, or whether he drew any particular inferences from it. He also had frequently seen the segmentation which several speakers had referred to, and he would like to know whether the author agreed that it was common to find that putting more food into the stomach caused it to lose that segmentation. He had come across one case in which the stomach appeared normal when the patient stood up, yet when he lay down it was segmented into two parts. The man died, and the post mortem showed malignant disease of the pancreas; the stomach contents were simply divided across the tumour when the patient lay down.

Dr. C. J. MORTON desired to refer to a physiological question—namely, that of the filling of the normal stomach with food, as shown by a bismuth meal. In Dr. Groedel's skiagrams the upper level of the bismuth in the cardiac portion appeared to be always the same, however much food had been taken, and he (the speaker) understood him to say that this was due to the tonic contraction of the organ. Most of the papers also which had been written on the subject, showed the bismuth first as a narrow line, and, if more were taken, then as a broader line, but always reaching to the same height in the cardiac end. That had not been his own experience; in a normal stomach the bismuth always dropped at once to the lowest part, the pyloric portion was first filled, and when that was full the food gradually rose in the cardiac portion as more was swallowed. Most of the cases examined were those of gastric disease, and even in normal cases he had not been able to get the patients to take enough of the bismuth food to distend the cardiac part to the extent shown in the skiagrams. In reference to the diagnosis of tumours and of pyloric obstruction, he asked whether peristalsis of the pyloric portion was any guide. He had not found it so. In his cases of obstruction the peristalsis was not more than that seen in normal stomachs. Sometimes there was a division of the cardiac end into two parts, and in some cases the bismuth could be seen trickling down through the constriction from the upper to the lower, indicating adhesions from old gastric ulcer or neoplasm. He had found this useful in diagnosis, but the division must be definite. In one case there was an apparent separation, and

he had no hesitation in diagnosing gastric ulcer; but on operation the stomach was found to be of normal shape and size, and the symptoms due to gastrostaxis.

Dr. FINZI said he would like to ask about bismuth and tragacanth and bismuth and water, because the results were probably different from those of bismuth and porridge. With the former meal, in his experience, it passed out of the stomach in five or ten minutes. How did that compare with the Rieder meal? With the bismuth and water he often found the stomach-shadow divided into two parts, a funnel-shaped upper portion and flat lower portion, connected by thin streaks. He thought that this might be due to the difference in composition of the test meal.

Dr. GROEDEL, in reply, said he would answer as well as possible in English the various questions which had been put to him; but to deal with them properly would require much more time than the meeting would be likely to accord to him. With regard to the motility of the colon, there was found to be very great variety. In all cases a bismuth meal passed quickly through the small intestines. He believed that by looking carefully one would find the first part of the meal in the cæcum in two hours, though he admitted that in many cases the time was nearer four hours. Much more experience was required on the point, and he hoped his hearers would join in providing that experience. He seldom took such pictures for diagnosis; he examined with the screen many times, and employed also the other means of diagnosis. In taking the pictures, it was necessary to employ quite definite pressure against the abdominal wall. In regard to the posture, it must be remembered that man was a standing animal; and it was notoriously bad to retire to bed immediately after a meal. X-rays alone would not enable one to diagnose stricture from gastric ulcer. He had not found food shoot quickly along the large intestine, as mentioned by Dr. Pirie. If the stomach was dilated, the intestine lay on the spine, and became divided into two parts. It was not possible to judge exactly, but if the stomach appeared to be in two parts, that particular stomach was not normal. The bismuth meal should be carefully prepared. He used the juice of raspberries and added bismuth carbonate, and it was important that the meal should be taken quite warm. Of course the patient should not know that anything had been put into the meal. When that was done, segmentation was not noticed. At first, three or four morsels would accumulate at the bottom of the stomach, and then the bismuth meal would ascend higher into the stomach, the height depending on its motility. With regard to angina pectoris, patients with heart disease often complained of stomach symptoms. In mild myocarditis especially one found the diaphragm extending high and pressed against the heart, and air bubbles could be seen against the chest. In the case of a diseased heart, the circulation in the stomach and diaphragm might be found to be bad. A patient with angina pectoris felt much better when he could get rid of the wind. Often it was impossible to say whether the stomach disease or the heart disease came first. But in many cases the stomach could be seen to be deficient in tone, with bad circulation.

Electro-Therapeutical Section.

April 15, 1910.

Dr. Samuel Sloan, President of the Section, in the Chair.

The Disappearance of Enlarged Glands in Lymphadenoma under Treatment by X-rays.

By A. Howard Pirie, M.D.

Lymphadenoma, or Hodgkin's disease, is characterized by progressive overgrowth of lymphatic glands, anæmia, and general weakness. It occurs in young persons, mostly males. During the past two years I have had nine well-marked cases to treat by means of X-rays, and the following observations are based on the results obtained in these cases. In the cases which improved quickly under X-ray treatment the enlarged glands were discrete, soft, elastic, movable, practically painless, and not attached to the skin. Cases in which the glands were hard or painful were not so amenable to X-ray treatment. The size of the swelling is of little importance. Large swellings go down comparatively quicker than do small ones. In two cases the swellings were about the size of a fist. In the other cases they were much smaller. In no case was there any tendency to suppuration. In every case the cervical glands have been the most enlarged. In some cases there was also enlargement in the axillary glands and in the mediastinal glands. The chief histological change in lymphadenoma is an increase in the cells. In the softer glands the reticulum supporting the cells is reduced in quantity. In the harder glands the reticulum is abundant. This histological difference explains to my mind why the softer glands yield to X-ray treatment better than the harder glands. In the soft glands there are large quantities of quickly growing cells, and X-rays exert their greatest effect on quickly growing cells, whereas they have little or no action, in comparison, on connective tissue.

Symptoms.

In every case the first symptom noted was enlargement of the cervical glands. Anæmia and general weakness soon followed. In two cases pressure upon deep veins caused enlargement of veins of the skin covering the neck and chest. In one case swelling of lower limbs occurred with albuminuria. In the cases in which the blood was examined the red blood corpuscles were diminished. In one case slight poikilocytosis was shown. No marked increase of leucocytes occurred, but the proportion of large mononuclear leucocytes was slightly above normal. Shortness of breath was present in the advanced cases.

Diagnosis.

Diagnosis has to be made from tuberculous glands. In early cases this is impossible without microscopic examination. In two early cases in which a gland was removed and examined by the microscope, the diagnosis of lymphadenoma was made, and X-rav treatment begun at once. These two cases have not advanced, and the patients are able to keep at work while continuing treatment by X-rays. Tuberculous glands are commoner in the submaxillary group of glands, whereas lymph-adenoma is commoner in the anterior and posterior triangles. Tuberculous glands form a matted bunch, whereas in lymphadenoma the individual glands can be felt from the first. (This discrete condition comes on in tuberculous glands when the first signs of improvement occur during X-ray treatment.) A case of lympho-sarcoma which I treated by X-rays for months, thinking it was lymphadenoma, eventually died, and the post-mortem examination showed the correct diagnosis. This case differed from lymphadenoma by having hard, tender swellings instead of soft, painless ones, and by difference in the blood—viz., no anæmia, and a comparative increase in lymphocytes.

Treatment.

Lymphadenoma has until lately been looked upon as an incurable disease. Professor Osler stated before the introduction of X-rays, "No instance of recovery has come under my notice in the cases of which I have had notes." Arsenic and X-ray treatment combined have been used in most of my cases, but I do not consider that arsenic is essential in the treatment, because in one case under arsenic and X-ray treatment,

the glands treated by X-rays disappeared, while those untreated remained. Another case treated by X-rays and no arsenic is doing very well, as I shall show you later on. The regular decrease in the size of the glands which follows large doses of filtered X-rays is quite a different thing from the spontaneous fluctuation in their size which is a recognized characteristic of these glands. My technique is one pastille dose once a week with a large Bauer tube, of hardness 4 to 5 Benoist scale. The rays are filtered through aluminium (gauge 30) and are thus reduced to about half their quantity, and the softer rays are removed. The hardness of X-rays is also increased one degree in Benoist's scale by the aluminium filter. The patient is 24 cm. from the source of X-rays and the pastille is 12 cm. from the source of X-rays. The skin of the patient grows brown, but there should be no redness produced. I have arrived at the above effects of the filter by the following experiments. A Benoist aluminium scale was exposed over a photographic plate for ten seconds. On the same plate, but on another part which had been protected by lead during the previous exposure, another X-ray photograph was made of the same scale, but this time it was covered by aluminium of gauge 30. A similar exposure was next made with aluminium 1 mm. thick. The plate was developed, and the three skiagraphs of the aluminium scale were compared. In the first the hardness of the X-rays was three and a half, in the next four, and in the next case five. To estimate the total quantity of X-rays cut off by the aluminium it is only necessary to expose two pastilles at the same time, one of which is covered by aluminium of the same thickness as the filter, and the other is left naked. When the naked pastille colours the B tint it will be found, with rays of hardness 3 on Benoist's scale, that the filtered rays have coloured the shielded pastille only to half the B tint. When one uses no filter one frequently hears a patient say that after a full pastille dose the part became red the same evening, remained red next day, and then the redness faded away. I have not noticed this primary redness while using aluminium filter, even after giving two pastille doses at once, and I would like to hear if others have noticed the same. Radium has been used by Dominici for the treatment of lymphadenoma with good results. At the Paris Congress at Easter I heard him say, however, that the use of radium for lymphadenoma was less useful than X-rays, especially when the glands were disseminated. Moreover, it is necessary to make an incision into the mass in order to insert the tube of radium. The use of X-rays would therefore seem to be preferable. Dominici recommends the use of 300 mgm. of pure radium sulphate for

use for lymphadenoma. On another occasion I heard him recommend 400 mgm. of pure radium sulphate. This is a quantity that few, if any, medical men can obtain in this country, but the sooner we get large quantities the better. I believe there is a great field of usefulness for large quantities of radium.

The first case I have to describe is that of a boy who came for X-ray treatment two years ago. He had a swelling in the side of his neck about the size of a fist. A short account of his history during the last ten years is as follows :—

Admitted	Discharged	Under	Complaint	Operation	Pathological Report	Notes
Feb. 27, 1899	Mar. 24, 1899	Mr. H. Marsh	Enlarged glands in neck	Erasion	Inflammatory	Mr. Marsh removed one gland and found it lymphadenomatous, and so left the rest
June 6, 1899	July 19, 1899	,,	Tuberculous glands in neck	,,	*Nil*	Mr. Marsh removed all the glands; only note is that glands are very elastic
Jan. 22, 1900	Feb. 9, 1900	,,	Enlarged glands in neck	,,	No evidence of tubercle; highly suggestive of lymphadenoma	Two large glands were removed
Sept. 1, 1902	Sept. 13, 1902		Enlarged glands in neck; lymphadenomatous	,,	Tuberculous lymphadenitis	No enlarged glands in axillæ or groins; the glands, to the naked eye, appear to be more lymphadenomatous than tuberculous

He had been operated on four times, and the glands removed, but they grew again so fast that the surgeon refused to operate any more. I show you his photograph as he appeared when the X-ray treatment was begun. After six months of X-ray treatment all the glands treated by X-rays had practically disappeared, as shown in the second photograph. During these six months I had devoted myself solely to the glands in the neck, and left untouched those elsewhere. I then began to treat those in the axilla, which had remained stationary for six months, and they grew smaller in like manner under X-ray treatment. In this case the patient was taking arsenic, but only the glands under X-ray treatment grew smaller. At the commencement of X-ray treatment I made an X-ray photograph of his chest, which I show you, and you see there is a great mass of glands closely simulating the

appearance of aneurysm. I thought it was hopeless to try X-rays on such deep-lying structures, and have not as yet treated these with X-rays. I hope, however, to begin treatment of these glands, owing to a communication made by Professor Kienböck at the Paris Congress this year. There, Professor Kienböck showed orthodiagraphic tracings of enlarged glands in the mediastinum which he treated by X-rays, and he has kindly sent these to me to show you this evening. His case received three courses of X-ray treatment, and a marked diminution of the glands is shown after each course of X-ray treatment. To return to my first case: The patient's neck and axilla now show very little of their former condition, but his mediastinum remains the same.

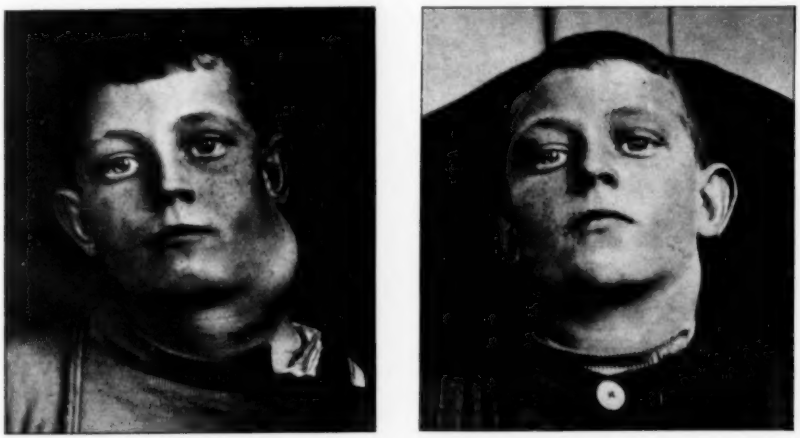


FIG. 1.

Photograph taken February 15, 1910, before beginning X-ray treatment.

Photograph taken April 7, 1910, after five X-ray treatments.

My second case is that of a small boy, whose photograph I show you (fig. 1). He came for X-ray treatment on October 6, 1909, and after one full pastille dose the swelling began to go down. He was sent to the seaside, and the swelling began to grow once more. After two months it had increased to a greater size than when he first came to me. This result is what happened to another case that was sent to the seaside, which I shall report presently. X-rays were next applied on February 15, 1910, and after five treatments the swelling was very much reduced, as shown by the photograph (taken on April 7). The patient is at present

a bright, healthy-looking boy, with only a slight fullness in the side of his neck. He has been under treatment for only two months, so I expect that in another two months the swelling will be entirely gone. No microscopic examination has been made in this case.

My third case is one in which the diagnosis was made after a gland had been removed and examined by the microscope. In this case X-ray treatment was begun early, and no further development of glands has taken place. The patient is now in perfect health.

Another case is one which was rapidly going down hill till X-ray treatment was begun. I show you his photograph when he first came

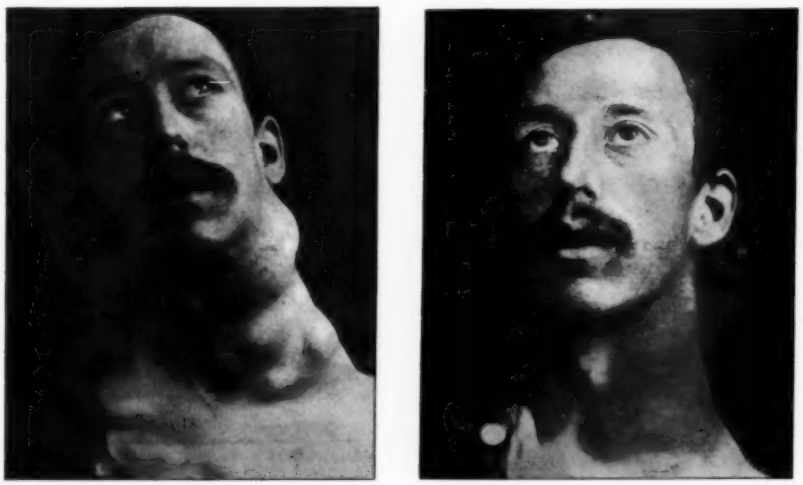


FIG. 2.

Photograph taken March 22, 1910, before beginning X-ray treatment.

Photograph taken April 14, 1910, after three X-ray treatments.

for X-ray treatment (fig. 2). He was anæmic and extremely weak. He has had only three sittings of X-ray treatment, but already he is much stronger, and the glands are markedly reduced. Cross-fire, filtered rays, maximum doses per week, and greatest practical distance between the skin and source of X-rays, is the basis of treatment in this case. In the chart of his blood you see the decrease in the red blood-corpuscles and increase in percentage of large mononuclears are the prominent features. Since beginning X-ray treatment the red blood-corpuscles

have increased, the lymphocytes have increased, and the large mononuclears have decreased.

The next case I show you was sent to me as a case of lymphadenoma, but I believe there is a mistake in the diagnosis, as the condition has existed for eleven years, and there is no response to X-ray treatment. I show you her photograph, and it looks very like a case of lymphadenoma, but I believe these swellings are really multiple fatty swellings and therefore not amenable to X-ray treatment.

The last case I have to report is a case treated in the service of Dr. Béclère by Dr. Haret, in Paris, and reported by their permission. Girl, aged 14, came for treatment on October 28, 1907. A painless swelling began at 11 years of age behind the right ear. It remained for two years, and she was then sent to the seaside for two months. It increased in size while she was at the seaside. When treatment was begun by X-rays the patient was anæmic. She had no cough. There was an enormous mass of glands from the ear to the clavicle and from the middle line to nearly the outer border of the clavicle. The swelling was as large as a fist. The skin was not adherent, and discrete masses of glands could be felt. The circumference at the base of the neck was 17¼ in.—that is, this girl, aged 14, would have required a collar of size 18. The swellings were discrete. A blood count showed red blood-corpuscles 2,300,000 ; whites, 11,200 ; polymorphonuclears 73 per cent. ; large mononuclears 14 per cent.—i.e., reduced red blood-corpuscles, and slight increase of white blood-corpuscles, with increased percentage of large mononuclears. Treatment was given once a week to the side and to the front of the neck—3 H. and 7 Benoist. On April 2 only three small hard glands could be felt. After this date treatment became irregular, but was continued for two years. Total quantity given was 115 H. to each part treated. The patient has now no enlarged glands.

Conclusions.

In lymphadenoma the enlarged glands which can be treated by X-rays are reduced to their normal size by X-ray treatment. The treatment should be by hard filtered X-rays, delivered by cross-fire. The skin should turn brown under the treatment. Maximum dose per week that the skin will stand should be given.

DISCUSSION.

The PRESIDENT (Dr. Sloan) said members would agree with him that the paper was an admirable one, and showed what an advance in X-ray therapy there had been during the last few years. There was every reason to congratulate Dr. Pirie on the work which he had done in that direction. He asked whether, coincident with the disappearance of some of the glands which were treated, there was any effect on the other enlarged glands not treated by the rays. He presumed that if Dr. Pirie were treating deeper glands he would use a thicker layer of aluminium.

Dr. FINZI said he was very glad the subject had been brought forward, especially as Dr. Pirie had been treating the cases by filtered rays. With regard to the method of measurement, he did not think it was quite satisfactory to measure a pastille dose with the pastille on one side of the aluminium and the patient on the other. It was as easy to put the pastille on the same side as the patient. That was first shown to him by Dr. Béclère, of Paris, who used the pastille behind the aluminium, and on the same side as the patient. Another factor which probably came into the question of filtered X-ray treatment was that of the intensity of the rays. He thought that the greater the intensity of the X-ray stream the more beneficial would the effect be on the tumour. He did not think people obtained the same results by treating a tumour with $\frac{1}{2}$ ma. going through the tube, as with 1 or 2 ma., especially when using filters. When no filter was used he had also noticed the redness a few hours after the application, and it lasted about one and half days. He had never seen that redness when a filter was used. It was difficult to treat such a large lesion with radium, and if the condition would respond to X-rays at all, it was better to use them. An enormous quantity of radium would be needed for such large glands, and very few people possessed such amounts. With regard to treating the mediastinum, he had seen a case of Kienböck's, in which there was lymphosarcoma of the mediastinum and the glands in the neck, as proved by microscopical sections. He saw that case five years after it had been so treated, and the patient remained perfectly well. The skiagraphic appearances were curious, because one side of the diaphragm was drawn up, and there was much fibrosis in the mediastinum; but there was no sign of the tumour. He was able to compare the present skiagram with those which Kienböck had taken both before and after the treatment.

Mr. THURSTAN HOLLAND said he did not like throwing cold water on anything, but in the cases which Dr. Pirie had just brought forward, the periods since the treatment had been altogether too short to justify their being spoken of in connexion with cure. He had had a long experience of X-ray work, and when he saw the announcement of the paper he hurriedly looked over some cases in his old case-books at the hospital. Most of the cases to which he referred were treated as long ago as 1903, 1904 and 1905, and, of course,

conclusions drawn from them were of more value than those from cases treated last year. Nearly all the patients showed a diminution of the glands after X-ray applications, quite soon after the first application; a certain number, as usual in hospital work, were heard of no more. The other patients who could be traced returned to hospital with recurrences, and eventually died. His assistant threw a sidelight on the kind of case by remarking that "he did not like them: they seemed to go the same way as the spleen cases; they thought they were going to get cured, but they came back and died." But sometimes there were good results from X-ray treatment which could not be readily explained, and thus it was justifiable to try it; but he was anxious that Dr. Pirie should not have the idea that such cases were going to be cured. If he waited a year or eighteen months it was to be feared he would have to confess that many of the patients in question had not survived. However, in the case of young people, he had seen large masses of enlarged glands disappear after a few weeks of the treatment, and, as far as could be traced, some of them had remained well for years; X-ray treatment seemed to effect more permanent results in these.

Dr. BARCLAY (Manchester) said that he did not insist on the word "cure" in connexion with these cases; he had not, so far, seen any of them go wrong. He had only seen one case which had been under treatment for more than two years, and this was in a youth, aged 17, who was still doing well. One patient whom he had been treating, with Dr. Bythell, was a young married lady, aged 25, and her case threw some light on what Dr. Pirie had said. When she came for treatment she was following the usual course—namely, going gradually down hill, the size of the glands varying from time to time. She had attacks of pyrexia and night-sweats, could hardly walk at all, and altogether was in a very bad way. Within a few weeks of starting the X-ray treatment the pyrexia had disappeared, and she became gradually well, and had remained so until the present date, though slight enlargement of the glands was still present. She had been under treatment for a year. Last October she went to the seaside, where she stayed three weeks, and during that time she came up for treatment once. But when she came back she had very bad dyspnœa, and all the glands had increased in size. Large masses of mediastinal glands were seen by means of the screen, but since the treatment of the mediastinum there had been no more dyspnœa. Menstruation had just returned, quite naturally, after an absence of eighteen months.

Dr. REID said that what had struck him most was the extraordinary rapidity with which the glands disappeared in certain cases, and the very remarkable rapidity with which they recurred. During the last three months he had had under treatment a small boy, who had such enlargement of the cervical glands that he was practically flush from the jaw to the clavicle. He had also two masses in the axillæ the size of a tangerine orange, and two enormous masses of glands in the groin. Treatment was started on only the mass in the right groin. A filter was not used. After six applications with ½ ma., the mass had almost entirely disappeared. Then he commenced on the glands on the other

side, and practically all the glands on that side disappeared within a month. In the meantime, the other glands had not altered at all. The other set of glands was then commenced on, and they also began to subside quickly. But even while the treatment was being carried out on the second side, the glands on the side first treated had commenced to reappear. That might possibly mean that an insufficient dose had been used. But the disappearance of the glands treated, and their subsequent reappearance, was very remarkable.

Dr. REGINALD MORTON desired to congratulate Dr. Pirie on the way in which he had done his work; his methods of filtration and measurement were very scientific. But during his connexion with the London Hospital he (Dr. Morton) had had several cases of the kind, and his experience of them had been very much that mentioned by other speakers. There never seemed to be any difficulty in getting the glands to subside, and the treatment was most encouraging up to that point. He had one case which was as severe as any which Dr. Pirie had shown pictures of; there was very marked enlargement, and the glands stood out in globular form. After ten weeks of treatment, the neck was practically symmetrical and normal in appearance. Then, as the patient felt out of sorts, he begged to be allowed to go to the seaside for a time. As there seemed to be no objection, he did so, and he was told he might stay a month. But he came back in a fortnight in great alarm, because the lumps were larger again. He understood the case was still being treated, but the disease was getting ahead of the X-ray treatment. He looked upon the treatment of the condition under discussion as distinctly discouraging; so much so that he was beginning to lose interest in the X-ray treatment of such cases.

Dr. HARRISON ORTON desired to thank Dr. Pirie for his excellent paper, and especially for the way in which he had described the dosage. He gathered from the remarks of previous speakers that rays were sometimes given several times a week in unknown dosages. Certainly in X-ray treatment a large kick had a very different effect from a series of small pushes. Frequently he had seen a case of rodent ulcer which had been treated by small doses of X-rays two or three times a week without much effect, but after a two-pastille dose the condition had cleared up after the one application. In England, filtration of the rays was comparatively new, and he doubted whether the cases mentioned that evening had been treated with filtration. The experiments of Bordier showed that without filtration it was impossible to get at some of the deeper regions without burning the skin. It was only recently that that authority had been able to produce an artificial menopause. One could understand that with a filtered dose and the powerful rays at one's disposal, quite a different effect could be produced on glands than with small unfiltered doses oft repeated. It was astonishing to see, even in hospitals, how many cases were now treated with unknown doses of X-rays, without any attempt at filtration or measurement. Until more work was done on the lines set forth by Dr. Pirie, he did not think one could form very definite conclusions as to what one form of treatment could do as against another form. He asked whether Dr. Pirie

had had experience of Dr. Bordier's pastilles. He (the speaker) had used them himself, but the tints were so difficult to judge in the stages in which the skin was not affected that they seemed to be almost useless for any but rather large doses, though Bordier apparently used nothing else.

Mr. McADAM ECCLES said he came as a surgeon and a colleague of Dr. Pirie, and because cases of lymphadenoma were perhaps one of the greatest trials of the surgeon. He often proceeded to remove glands in those cases at a comparatively early stage, and found microscopically that they showed all the evidences of lymphadenoma, and then it was known that the case would come back within a few months with a larger mass of glands than before the operation. And not only so, but glands would be found enlarged in other parts of the body. Up to the last few years, such cases had progressed downhill, in spite of every form of treatment. But the cases were, in their early stages, very difficult to diagnose, and the text-books showed that there was considerable confusion as to the glandular enlargement of lymphadenoma, Hodgkin's disease, lymphosarcoma, and lymphoma. In trying to work out new lines of treatment, it was very important to do everything from a truly scientific as well as experimental point of view. In these cases there should be a removal of perhaps one or more glands from each area which it was proposed to treat by X-rays, and the microscopical characters of such glands should be very carefully recorded. He knew the difficulties when cases were sent from the wards of the hospital, and were therefore not the X-ray operator's own cases, but if Dr. Pirie and other radiographers would treat cases in this critical and scientific spirit and would ask for the removal of a gland, it would be of immense advantage for future investigation. He would go further, and say that as the glands diminished under one or more treatments, it would be well once more to remove a gland, which could be done under local anæsthesia, and the gland should be very carefully microscoped, so as to show what were the exact differences as compared with the gland first removed. He had watched several such cases very closely, and when the glands had gone down so as to make the neck look practically symmetrical one could still feel small glands, about the same size as those often detected after tuberculous invasion of glands was considered cured. One of these glands again should be investigated. There was much difference of opinion as to whether the lymphadenomatous glands were really infected glands—i.e., whether they enlarged because of the irritation of some infection, probably bacterial; and he asked whether Dr. Pirie considered it possible that the X-rays prevented the organisms from producing a toxin—a toxin which was the cause of the irritation and enlargement, the supposition being that the organisms lay latent, but afterwards again took on the manufacture of a toxin. He asked whether it was possible, after treating them with the "big kick," to continue to treat them with "small pushes" without the interval which one found in hospital work, in which patients went away for a month or six weeks, not necessarily to the seaside, but often to the slums of London, after which they came back with the glands largely developed. Could such cases be kept down by small repeated doses? If that were possible the cases

should be taken in hand at the earliest stage. Many cases of supposed lymphadenoma were really tuberculous. One found that in a certain number of cases of tubercle of glands, particularly in the neck, there were not the typical tuberculous deposits, with giant cells, epithelioid cells, and small round cells, but a large area of granulomatous tissue. That class of tuberculous glands did very well under X-ray treatment, according to his experience. He congratulated Dr. Pirie, because he was most energetic in the treatment of those cases, and went to work scientifically and carefully.

Mr. DEANE BUTCHER desired to express his appreciation of the scholarly and scientific character of the paper. Evidently measurement of the dosage had been carried out most carefully; Bordier's pastilles, used under the aluminium filter, would be a distinct advance in technique. He suggested that some of the good effects produced by the X-rays might be due to a sort of auto-vaccination. With regard to Mr. Eccles's most interesting case, the sudden subsequent improvement in the general condition might have been due to the excision of a gland causing a surgical auto-vaccination. The evidence of Dr. Reid appeared rather to negative the idea of Röntgen auto-vaccination, since he had found that the irradiation of the glands on one side seemed to have no effect on the glands of the opposite side. In certain cases of lupus, however, he (Mr. Butcher) had observed that irradiation of the enlarged glands benefited the lupus. He urged that Dr. Pirie should endeavour to have the opsonic index taken in case of irradiation of glandular enlargements. There was abundant evidence that X-ray irradiation was followed by the production of antibodies and cytolytic toxins. The effect was most marked both on the serum and the leucocytes. What was wanted was some means of converting the wonderful temporary effect of the rays into a permanent one.

Dr. MCCULLOCH desired to recall a contribution he had made to the *Lancet*[1] more than three years ago, entitled "Observations on the induction of auto-vaccination in tuberculosis and other chronic glandular infections by X-ray irradiations of the lymphatic glands involved, controlled by estimations of the opsonic index." He had then formed the opinion that involvement of lymphatic glands, as a rule, arose physiologically from *extrinsic* and infectious causes. On such occasions the fibrotic processes were often in excess of the delicate chemical and physical syntheses, which went to the elaboration *in vivo, and within these glands*, of the required vaccines. The fibrolytic action of the X-rays thus regulated a physiological process of a profound kind, and allowed of the transudation of this material into the lymph- and blood-stream. The opsonic index was raised by easy stages, without any negative phase whatever, which in the localized cases never rose beyond the normal range, a slight variation only occurring during the menstrual period in females, and the corresponding period in males. These observations were carried out over a period of ten months, and with the greatest precision by two expert opsonists together, who compared and checked their calculations. They often declared he must

[1] *Lancet*, 1907, i, p. 215.

have administered *minute* doses of Tuberculin Koch, though a negative phase was never met with. But in those pathological conditions where enlargement of the lymphatic glands arose from *intrinsic* causes, as in lymphatic leukæmia, lymphatism, or in lymphadenoma, we had to deal with a pure parenchymatous and not a reactive fibrotic hypertrophy, a very different state of affairs, and more so, if in such a reactive fibrosis happened to be superimposed. He thought it was inaccurate to speak of lymphatic glands "disappearing" when they ceased to be palpable after irradiation; they were restored to their normal size, being relieved of the fibrosis, or remained as inert, dense, fibrous masses. When excised they were frequently regenerated, as was commonly seen in tonsils that had been excised in childhood. He would like to add that certain American and Italian observers had confirmed the observations he had referred to, and he ventured to suggest the importance of this line of research as tending to elucidate many of the dark problems concerning the anatomy and physiology of the lymphatic system.

Dr. PIRIE replied that in the case of the boy with very much enlarged glands in the neck, he only X-rayed that mass, and the glands in his armpit remained the same size, thus showing quite a local action. He would like to use thicker aluminium for the deeper parts, but it cut off so many rays that a much longer exposure was needed, and that, in busy hospital practice, was a very important consideration. He had put the pastille behind the filter, as Dr. Finzi suggested, but if the filter were applied directly on to the pastille, the effect was different from that caused when it was put some distance from the pastille. He agreed that the larger the dose the better the result produced. He was always glad to hear Mr. Thurstan Holland's criticisms. But that speaker used the word "cure," which he (Dr. Pirie) carefully abstained from; he referred only to the disappearance of the glands. Mr. Holland's experience in the past was very valuable, and the only hope he (Dr. Pirie) could extract for the future was that perhaps Mr. Holland did not measure the dose used: if that were done, and a big dose given, there seemed no reason to despair in regard to lymphadenoma. He agreed that residence at the seaside seemed bad for lymphadenoma. Dr. Reid had mentioned the rapidity of recurrence, but against that was the case of the boy whose enlargement went down after six months, and the improved condition had persisted for eighteen months, with only an occasional dose of the rays. Possibly other speakers also had not measured their doses. He thought Dr. Orton's simile as to a big kick and several small pushes was very apt. It was well illustrated by the growth of hair, which was stimulated by small doses of the rays, but inhibited by large ones. He had seen much the same thing in connexion with carcinoma of the breast; small doses of the rays had not prevented recurrence, in fact the growth seemed to be quicker under their stimulus; but he had seen good results in that disease from big doses. He was glad to hear Dr. Finzi say he had not seen primary redness caused when a filter was used. Soda glass kept back the ultra-violet rays, and those rays did not cause the primary redness. It was necessary to look for some other cause.

The glass hardened the rays to some extent. He had tried the Bordier pastille a little; the tints were difficult to distinguish by daylight, but if examined by the electric light they were easier. But there was the Sabouraud method, and that was good enough. Mr. Eccles's suggestion as to removal of glands was good, and scientific, and he would like to co-operate with him. He had described three cases in which the glands were cut out and examined, and those patients had done well so far. He was willing to go on and try more in the same way. In regard to Dr. McCulloch's remarks, he believed that gentleman was the only one who had done work on those lines in England. The rays might inhibit the formation of toxins, but that theory was not supported by the case of the boy whose glands in the axilla were not treated, and remained the same, although the rays were applied to another part, the glands of which disappeared. There seemed to be three categories of enlarged glands in relation to treatment by X-rays: inflammatory, which disappeared very quickly; lymphadenomatous, which took a medium time; and tuberculous, which required a long time. With regard to auto-vaccination, he would like to co-operate with anyone in taking the opsonic indices, but it entailed so much work that it would need the foundation of a scholarship, and perhaps £300 per annum would not be too much.

A Simple X-ray Stereoscope.

By A. J. H. Iles.

This stereoscope is modified from a somewhat similar appliance used in France by Professor Pigeon. It has great advantages in size, portability, and simplicity. The actual stereoscope consists only of a single flat mirror 3½ in. square. It is useful to have a stereoscope with which it is possible to take the plates straight from the fixing solution and, after having hastily washed them, place them *side by side* before a piece of obscured glass, thus doing away with the complicated lanterns which we all know. All that is necessary to convert the usual obscured glass examination screen into a stereoscope of this kind is to fix a mirror consisting of a plane glass, with a dull backing in front of it, at about 9 in. distance. The surface of this mirror faces right, and the metal back in which it is fixed is fitted with a ball-and-socket joint, by means of which focussing manipulations can be accurately carried out. I have found that a small photographic plate, exposed to light and developed, makes the best mirror, ordinary looking-glass being useless, as a double image is thrown by it.

The method of working is as follows: The plates are exposed in the usual way for stereo-radiograms, the view corresponding with the right eye is then placed with the *glass side*, and that corresponding with the left eye with the film side, towards the observer, who, by looking at the left-hand plate with his left eye, and at the reversed image of the right-hand plate reflected in the mirror with his right eye, at the same time focussing by means of the ball-and-socket joint, obtains a stereoscopic view of the structures. If it is desired to work with both films towards the observer, the plate which is to be viewed through the mirror can be exposed through the glass back, as was done in the demonstration stereo-radiograms before you. Should it not be convenient to carry the whole apparatus, or if the stereo-radiograms have to be sent by post, the mirror can be detached at the joint, and examination carried out by placing the radiograms against a window facing north, the mirror being held before the eye by hand.

Dr. REGINALD MORTON said he was much struck by the simplicity of the device. When Mr. Iles sent the description to him, he simply held two plates up before the window, and got a most perfect stereoscopic image, using the back of a quarter-plate negative, held in the hand, as a reflector. Some years ago, Mr. Clarke showed a stereoscope on much the same principle, in which he had two prints—one made in the ordinary way, and one from the back of the plate. The image given was excellent.

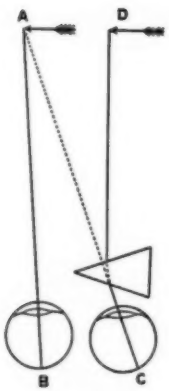
A new Pocket Stereoscope for Use with X-ray Negatives.

By A. HOWARD PIRIE, M.D.

IN order to see stereoscopically one must have perfect vision in both eyes. Those who have not perfect vision in both eyes cannot see stereoscopically. Those who have perfect vision in both eyes can see stereoscopic photographs in relief without the aid of a stereoscope by a method of squinting. This pocket stereoscope is simply an aid to this squinting method, so that a beginner can see stereoscopically at once without much practice and straining of his eyes. In this stereoscope the left eye looks directly at the left hand picture, and the right eye sees the right hand picture but thinks it is looking at the left-hand picture. That is, both

eyes think they are looking at the same object, but they really see the same object from two points of view. Thus, all the conditions are fulfilled for stereoscopic vision. A diagram shows how this is brought about. The nearer one can approach to the photographs while seeing stereoscopically, the greater is the stereoscopic effect. For large (12 by 15) plates one must stand farther away, and for small plates one must come close up.





A, B, is the direct line of vision of the left eye. **A, C,** is the line of vision the right eye thinks it is looking along. **D, C,** is the refracted ray which the right eye really receives. An ordinary prism is shown in the diagram, for simplicity; a reflecting prism is used in the stereoscope to avoid chromatic aberration.

Electro-Therapeutical Section.

June 3, 1910.

Dr. SAMUEL SLOAN, President of the Section, in the Chair.

The Relief of Symptoms of Prostatic Obstruction by Electrical Treatment.

By J. S. BOLTON, M.D.

THIS modest title is purposely chosen. A more ambitious stand might have been taken, and some mention made of reduction of the size of the prostate; but it is better to postulate too little than too much. All that is implied in the title can be demonstrated; improvement in symptoms is easy to ascertain and register. Every patient can appreciate the difference between a disturbed night and a good one; between getting up five to ten times to urinate, and sleeping the night through with little or no disturbance.

But when the size of the prostate is in question and exact measurements are required difficulties begin. The prostate is often described as being the size of a small orange. This may be perfectly correct, but it is very indefinite. Then, again, when it is sought to compare the size before and after treatment, not only is accurate observation necessary, but a very exact and retentive memory, especially if several weeks have elapsed between the observations. The desire to record diminution is apt to mislead the judgment. Even when the operator is certain that there is considerable shrinking, it may not be easy to demonstrate. But if the symptoms are cured, and the patient made comfortable, the chief end of treatment has been obtained.

The claim now put forward may be thus stated: That by a treatment which is harmless and painless, so that it can be administered to the aged and feeble, so much improvement can be brought about as to make the patient comfortable, remove the necessity for catheter interference, and restore his hope and confidence for the future.

The method of treatment is to pass an electrode into the rectum and administer high-frequency currents. The electrode is so placed that it lies against the posterior surface of the prostate. The current is taken from the top of the resonator. The local effect can be increased by the operator placing his hand flat upon the abdomen just above the pubes. In urgent cases the patient should be treated every day for a quarter of an hour, but in more chronic cases three times a week will be sufficient.

In my earlier cases I used a solid metal electrode, and I continue to use this in all cases where congestion is a marked feature and where irritable nerves require soothing. But for large, hard prostates a more stimulating treatment appears to be indicated. For this purpose I tried the ordinary condenser electrode, but found much trouble from stray sparks to the buttocks of the patient. I then tried vacuum electrodes, but here the same difficulty showed itself. If the electrode was pushed in far enough to reach the prostate the buttocks were immediately attacked. I had longer electrodes made, and after some breakages obtained serviceable instruments; but even with these much of the effect reached only as far as the sphincter ani, and I did not obtain that bombardment of the prostate which I desired. I again turned my attention to the condenser electrode, had it also lengthened, but still lost most of the effect upon the sphincter. My son, who is an electrical engineer, suggested what proved to be the remedy for this defect. Let the metal stem be very small until it reaches the position of the prostate, where it must be enlarged almost to the dimension of the glass tube. By this means practically the whole effect is produced just where it is required—in the immediate neighbourhood of the prostate. The stem is made to move up or down as required. These electrodes, which have been made for me by the Codd Arc Lamp Co., of Nottingham, in several sizes, answer their purpose perfectly as far as treatment *per rectum* is concerned. But something more than this is needed where obstruction is due to enlargement of the middle lobe. An electrode must be employed through the urethra. This I have not so far attempted; but I hope to find in the discussion which this paper is intended to call forth that others have succeeded where I have failed.

It is scarcely necessary to add that drugs and other necessary treatment were also prescribed where these appeared to be indicated.

In a former paper upon this subject I gave particulars of two or three early cases. I propose now to report the progress of these and to add the results of the treatment of others.

My first case was that of a man aged 60 who had been troubled with his prostate for three years. He suffered much from disturbed nights and retained urine, and had been obliged to have occasional recourse to the catheter. His prostate was a large one. Only by extreme care in the avoidance of damp and cold was he able to continue in his shop. This was four years ago. To-day he looks the picture of health; he can do his work in comfort; he usually rises once in the night to pass water; but in order to maintain this condition the treatment has had to be repeated on several occasions.

My next case was that of a younger man, aged only 54, and should rather be described as congestion than enlargement. He had no obstruction, but great distress from prostatic cystitis. He improved immediately under treatment, and has not, so far as I am aware, had any relapse.

My third patient had already undergone operation for removal of the prostate. The operation had been so far successful in that it had removed the obstruction, but much irritation and distress remained. This completely passed away under the electrical treatment, and he was keeping well when I last saw him.

My most successful cases have been those where congestion and irritation rather than size of the prostate have been the prominent features. A doctor, aged 59, had been waking almost every hour of the night, and rising several times to pass water. This condition had been gradually getting worse for several years, and he had tried medicinal treatment without avail. But, though the symptoms were so disturbing, the enlargement of the prostate was not great. He was treated three times a week for five or six weeks, using the solid metal electrode, and could then sleep the whole night through without disturbance. The cure was perfect and lasting.

A very similar case was that of a man, aged 60, who had to be relieved with a catheter three years ago, and had been troubled with disturbed nights ever since, rising five or six times to pass water, but was not troubled in the daytime unless he got cold or damp. After seven treatments he was able to go through the night with only one disturbance. Indeed, he improved so rapidly that he only continued the treatment for

a short time, and therefore I fear the result may not be permanent. A marked feature in this case, as in so many others, was that he had to wait for his urine to come. After a few seconds it would begin to dribble, and a little later it would come in a fair stream. This condition is amenable to high-frequency-current treatment.

Another patient of about the same age was troubled with the opposite condition. After a cup of tea or dram of whisky the desire to pass water would be so urgent that if he could not obtain immediate relief he would sometimes find himself wet to the boots. There was no marked enlargement of the prostate, and the trouble was not due to retention. His urine was clear, faintly acid, specific gravity 1022, and contained neither albumin nor sugar. The condition was greatly improved by the treatment. The patient was, however, advised to abstain from whisky in future, and to be very moderate in the use of tea and coffee.

But my patients have not been all of them of this comparatively youthful age. A preacher, who is in his seventieth year, came under my care just recovering from an acute attack, which had required the use of the catheter for eight days. He had a similar attack three years before. He usually sleeps all night without rising, and is not much troubled in the day. He has, however, some increased frequency of micturition, and is on that account occasionally nervous in the pulpit. Electrical treatment lengthened the intervals and restored his confidence.

Many patients have been made worse by the well-intentioned advice of surgeons whom they have consulted. An elderly gentleman with symptoms of enlarged prostate is warned of his danger and advised to keep a catheter and antiseptics for any emergency. He is also told never to delay when he has a desire to micturate, but carefully to avoid holding his water too long. The fear of a stoppage is always before his mind, and he urinates twice as often as he need. In time the act becomes a habit, and his bladder expects to be emptied frequently by night as well as by day. This faulty habit cannot be overcome without assistance. High-frequency treatment will remedy the cause and restore the confidence. Such a case was treated for me by Dr. Primrose Wells. The patient was aged 73. He was troubled with frequency of micturition, especially in the daytime. There was also delay in starting the stream, and sometimes dribbling after passing it. He consulted a surgeon, who drew off more than a pint of urine and advised him to have his large prostate removed. A physician reported unfavourably on the condition of his heart, and, fearing operation, he decided upon

electrical treatment. The result was eminently satisfactory. There was improvement in all the symptoms and in the health of the patient. The prostate, also, was reduced in size. As Dr. Primrose Wells is present he will probably give further particulars.

Another patient treated by me last summer was aged 76, and symptoms of enlarged prostate had been gradually getting worse for eight or ten years, but he had never had stoppage. The prostate was of moderate size, but he had much pain and discomfort in the region of the bladder and urethra, and rose eight or ten times a night to pass water. After three or four weeks' treatment the pain left him, but the nights were just as disturbed. It was only after two months' treatment that he obtained good nights. I saw him ten months after the treatment and the improvement was still maintained. He said "I am as well as ever I was."

My successes have led me to attempt some very hopeless cases, which have ended in failure. I treated two cases in which there had been no urine passed without the catheter for three or four years. The prostate as felt *per rectum* did not appear to be very large, but there must have been a central obstruction. Treatment through the urethra would be required to give relief. I also treated two cases of men who were not only old, but had very little vital reaction—cases of senile decay. They were so feeble mentally that it was not possible to rely upon their statements, or make sure whether an effect was being produced. It is not possible to cure every patient, but relief is experienced by such a large proportion as to make the treatment worthy of trial in all uncomplicated cases. The results of the treatment may be summed up in a few words: High-frequency currents have some effect upon the blood-vessels, the nerves, and the muscles. Congestion in the blood-vessels and stasis in the tissues are relieved. The nerves are soothed. The intense irritability of the bladder, which is such a marked feature of the disease, passes away. The tone of the bladder muscles is improved, and as a consequence the size of the stream is increased and also the force of propulsion. The bladder becomes able to empty itself and the urine returns to normal. The restoration of the confidence of the patient is no small help in lessening the frequency of micturition.

Is the prostate reduced in size? In seeking an answer to this question we must remember that the enlargement is not all due to overgrowth. Much of it is due to congestion. So there may be very considerable reduction in size without contending that high-frequency currents will remove fibrous overgrowth. It is important, also, to bear

in mind that the prostate is often enlarged for years before it causes any trouble. The symptoms only show themselves when the vital powers begin to flag owing to advancing age. It is only natural, therefore, that high-frequency currents, by renewing to some extent the healthy vigour, will remove the symptoms of obstruction. But besides all this, probably no one who has watched carefully the course of treatment of a successful case will fail to be convinced that there has been considerable reduction in the actual size of the prostate.

DISCUSSION.

The PRESIDENT (Dr. Sloan) expressed the thanks of the Section to Dr. Bolton for his admirable paper. While members of the Section were electro-therapeutists, in order to be successful as such they must also be clinical physicians. Before electrical treatment came into the hands of qualified medical men cures were often wrought where orthodox measures had failed. If this was so in the hands of the ignorant, surely the educated physician, with his knowledge of electric science, physical and physiological, his skill in diagnosis, and his experience in general treatment, ought to obtain vastly better results. Dr. Bolton said that in some of his cases there was not much enlargement of the prostate, yet there was bladder trouble; in others there was much enlargement of the prostate, but not much bladder trouble. Why was this? Why were old men troubled with their bladders? Evidently not simply because they had enlarged prostates. The first item in the bladder trouble of old men was atony of the bladder. Dr. A. G. Miller, a distinguished Edinburgh surgeon, had said he believed old men would be much less troubled with their bladder if they always made special attempts to void their residual urine. The bladder was weak, the patient micturated in a hurry, and thought he had emptied his bladder; but if the catheter were then used several ounces of urine would probably be withdrawn. A time came when the stagnant urine would decompose, and the bladder walls becoming affected, cystitis usually followed. There was congestion of the prostate, not inherent in itself, but due to the decomposition of residual urine. If, after waiting fifteen minutes, there was more than ½ oz. of urine passed, the subject of it was on the way to bladder trouble. A bladder which would not empty itself completely at the first attempt often would after waiting a few minutes. Urine was also liable to lodge in the part of the urethra anterior to the prostate, and often if, after micturition, the fingers were pressed behind the scrotum some urine could be squeezed out from there. It could thus be seen that damage might result to the prostate from the constant presence of such urine. He was glad Dr. Bolton did not attach very great importance to the effect of his treatment on the organ itself,

but much to the effect on the general health. A lady whom he was treating electrically for hæmorrhoids asked him whether it was intended to make her "fit." He replied that that would be the effect of it, though not the primary intention; and the same might be said of treatment of prostatic conditions. An electrode in the rectum greatly improved the tone of the bladder. He (the speaker) had had little experience of electrical treatment of the bladder in men, but in women he had no doubt of the value of the faradaic current in cystitis; he used it even in subacute cystitis. If the current was taken from a fairly large secondary coil the effect was sedative; and if after that a smaller coil was employed, the bladder was stimulated, the primary being subsequently used. He did not approve of the passage of a urethral electrode in cases of disease of the prostate. A few days ago a patient of his, aged 85, had a bougie passed into his bladder for enlargement of the prostate, and serious bleeding into the bladder followed, although the bougie had been passed by one of the most expert surgeons in Scotland. Such a man might be entrusted to pass a urethral electrode in these cases; but the danger should be remembered. Galvanic currents were not suitable here, because by applying the positive pole in the urethra constriction resulted, and by applying the negative pole hæmorrhage might be induced, both of which were undesirable.

Dr. PRIMROSE WELLS said he was glad to bear testimony to the utility of the treatment. Dr. Bolton had stated the case fairly, and given the matter attention without saying too much. The case which he treated for Dr. Bolton was a typical one, and his objection to it was that it did not go on long enough. He was a healthy man of regular habits, and soon brushed aside the inconveniences which he had; so that half the treatment which was regarded as necessary for him was sufficient. He complained that his stream of urine was so twisted that it was a great inconvenience for him to use a public urinal, as he came away with his trousers and boots wetted. After a fortnight's treatment that passed away. Where there was atony of the bladder, and a pendulous abdomen, it was good to give mechanical stimulation as well, and it seemed to dissipate the dull pain so often mentioned. He hoped someone would be able to suggest a satisfactory electrode for treating the middle lobe of the prostate. He had not succeeded in finding an instrument by means of which current could be brought exactly to the spot required. There were some cases due to obstruction in the middle lobe which could not be got at very well with present appliances, though they yielded eventually to prolonged treatment.

Dr. HOWARD HUMPHRIS bore testimony to the fact that almost all prostatic trouble could be relieved with the rectal electrode. He believed the amount of success depended mathematically on the amount of fibrous tissue which had been laid down, because the current contracted the muscular tissue and squeezed out the infiltrated matter mechanically, toning up the muscle-tissue of the prostate, and incidentally of the bladder. He thought some stasis

might return and the size of the prostate might remain; but with properly-directed treatment relief, greater or less, came immediately. He could not recall a case in ten years in which the patient did not mention that he felt "lighter" as he stepped down after having the treatment. He agreed that there should not be urethral interference, as the desired result could be obtained from rectal electrodes. It acted from such a short distance from the prostate. In gonorrhœal cases he had got better results with the vacuum electrode; where there was no history of gonorrhœa the solid electrode was better. The more recent the trouble the greater the success in treatment. In cases of acute prostatitis following gonorrhœa the results from static treatment were magical. He devised years ago an electrode to convey a spark direct to the prostate. It consisted of a small bulb heavily coated with vulcanite, and a wire inside leading to another bulb 6 in. further down. He thanked Dr. Bolton for his paper, and especially for the title. To relieve the symptoms was all that was needed to justify the more extended use of electricity in cases of enlarged prostate.

Dr. BOLTON, in reply, expressed his pleasure at the way in which his paper had been received. Some of the suggestions would be very helpful to him. He would note and try the device of putting the finger behind the scrotum. The reason he only spoke of high-frequency current was that that was the only form he used, but he was anxious to hear what current others used. He was very glad to hear what Dr. Humphris said about gonorrhœal cases; he had not yet had the opportunity of treating such cases. He hoped some time to try a urethral sound, as suggested by Dr. Manders. He knew the difficulty and danger of bleeding into the bladder, of which he had seen a good deal in the last year or two. Some cases did not seem to have any tendency to such hæmorrhage. It was in cases with hard, fibrous middle lobe that a urethral sound would be useful; and these cases did not tend to bleed.

The Relative Value of Various Types of High-tension Transformers (including Coil) Used for the Production of X-rays.

An Address Introductory to a Discussion on the Subject.

By Reginald Morton, M.D.

In the absence of a completely satisfactory method of generating a high-tension current, suitable in every way for exciting X-ray tubes, every means at our disposal must be in the nature of a compromise. This is only another way of saying that all the devices at present in use are more or less imperfect. With the recent demands of rapid radiography, new instruments have been brought forward and the older and more familiar ones have been modified to meet the new conditions. In such a matter as this, finality is not to be dreamt of, much less spoken about, for the present at least; but I am sure it will be agreed that it is a good thing occasionally to have a discussion on instrumentation in this Section, as a means of reporting progress, so to speak, and find out in what direction opinion is leading, on the part of those who have had a large experience. Before going over the various questions that may be raised, I wish to make one or two suggestions bearing on the conduct of this discussion, which I trust will receive the approval of the meeting. In the first place, I think we should avoid mentioning the names of the makers or agents of the various types of apparatus that may be referred to. Apart from the fact that our proceedings should be free from anything appearing like advertisement, it would be sure to lead to inequality in the amount of praise or otherwise that each firm received. Personally, I do not think there is very much to choose between the apparatus made by different firms, and, after all, what we want to get at is: What *type* of instrument is the best for our purpose? This, in my opinion, can be easily arranged without reference to any particular maker.

Further, I would ask that we confine ourselves to the requirements of X-ray diagnosis on this occasion, as this is the more exacting branch of our work. In comparison the needs of X-ray therapeutics are relatively simple, and what is very suitable for this class of work is not the best for radiography. As an example of this, at the X-ray department

over which I have control there is an outfit of the most elementary kind, consisting of an ordinary coil, old-fashioned dipper break, and operated from a small transformer which gives a continuous current of 50 volts. For treatment it is very satisfactory and efficient; for radiographic purposes it is a weariness to the flesh, and work up to modern requirements for the thicker parts of the body is quite out of the question.

With a view to still further narrowing down the field of discussion, I think we can afford to ignore the question of efficiency of our instruments as transformers of electrical energy. To us as radiographers it is a matter of the least importance whether the apparatus we use has an electrical efficiency of 25 per cent. or 95 per cent.; as, under any conditions, the consumption of primary current is a trifling detail, so long as we get the current we want through the X-ray tube.

The transformers at present in use are as follows:—

(1) The static machine.

(2) The high-tension transformer, with valve tubes.

(3) The induction coil.

(4) The rectifying high-tension transformer.

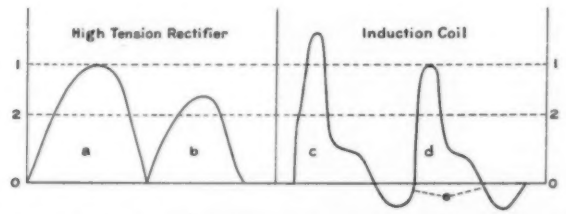
The first two of these need not delay us very long. The static machine gives out an ideal form of current for our purpose, and, provided we had one big enough, many of our troubles would disappear. The only one I know of that meets with the requirements of modern X-ray diagnosis is that used by Hulst, of Grand Rapids, Michigan. It is of the Toepler-Holtz type, but the shaft is arranged vertically. There are fifty revolving and the same number of stationary plates. Through a "Walter 6" tube it passes a current of 12 ma., the plates revolving at 1,800 per minute. He finds two seconds enough for an average kidney case; a mere flash is ample for a chest; and the remarkable thing is that a water-cooled tube worked in the same way for even thirty seconds. No mark is made on the polished surface of the antikathode. In the above exposures no screens were used. This absence of marking of the antikathode is peculiar to the static machine, and is one of the many advantages it possesses. Though the disadvantages of the static machine, in this climate at least, are so serious that in all probability it will never come into general use, it must be admitted that it gives the ideal current for our purpose, and provides the standard towards which our efforts should be directed. In its present form it is costly, bulky, and troublesome to keep in order.

With regard to the high-tension transformer with valve tubes to suppress or divert the negative wave, I am not aware that instruments

of this type have found favour among those who make radiography their chief consideration. They are admirable for high-frequency work, and are thus more frequently found in the hands of those practitioners whose work lies largely in this direction. By means of valve tubes the more ordinary requirements of X-ray diagnosis can be met. I think it hardly necessary to refer to this class at any length, because they represent, in my opinion, a step in the development of the high-tension rectifier which has attracted so much attention recently, and with which we shall deal presently. The elimination of the first two types of apparatus from our list thus narrows down our discussion to the relative merits of the induction coil and the high-tension rectifier. A year ago I thought the latter instrument quite the most desirable thing in the world. After using one for a few months I was not quite so sure; and now, with the great improvements that have been made in induction coils, I have very grave doubts about it. I hope we shall have the evidence of those who have had experience of the rectifier, because I am sure many of us are in doubt as to their real value and as to the advisability of investing in one. To get thoroughly unbiased evidence in a case of this kind is of course difficult. The man who has got thoroughly used to his coil and multi-anode electrolytic break is none too ready to admit the superiority of the rectifier; similarly he who has invested £150 or so in a rectifier is naturally unwilling to admit the possibility that he has not materially improved his position thereby. But I ask those of you here present who intend to take part in this discussion to leave personal feelings out of your consideration. The matter is an important one, and as such we should do our best to get at the truth.

An X-ray tube, according to the vacuum, has a certain degree of inertia; that is to say, a certain electromotive force has to be applied to it before the electrons are set in motion and the tube begins to work. So far as my observations go, the electromotive force necessary to start the tube is higher than what is sufficient to keep it working afterwards; and if we had a continuous current from a dynamo, adjustable from 0 to 100,000 volts or more, and applied this to our tube, we should find that after turning on a sufficiently high voltage to start the tube we should have to reduce this very considerably if the tube was not to become overheated. Further, if the vacuum rises, as it is certain to do while running on the small current, the tube will suddenly stop working; or possibly it may go on fitfully for a time before stopping altogether. It will then be necessary to increase the voltage to a considerable extent before the tube starts working again. Of course I have not had an

opportunity of putting these statements to the practical test, as, so far, electrical engineers have not succeeded in making machines that are capable of generating continuous currents of so high a voltage; but I am certain that we should find matters essentially as I have described. The nearest current we have to this is the high-tension transformer, with a closed magnetic circuit. The current is the same for the purposes of the argument, whether it is combined with a rotating rectifier using both waves, or when valve tubes are employed to suppress or divert one of them. A tracing of the current shows a broad sweeping curve of sine formation, and standing more or less midway between the straight line of the continuous current and the high, sharp peak of the current from the induction coil. Anyone who has used a closed magnetic circuit machine for any length of time must have noticed this peculiar behaviour of his X-ray tubes, and have found how difficult it was to get them to run smoothly when using the small currents required for much of one's daily work. When I was using a machine of this type I found it extremely difficult to keep in touch, so to speak, with my tubes, owing to this erratic behaviour. In contrast to this let us consider the character of the current we get from an induction coil. Here we have a curve (if, indeed, it may be called as such) rising abruptly to a great height, but of very short duration. If we draw a chart illustrating these various points it would be something like this:—



(0) represents the line of no pressure; (1) that of the voltage required to start tube in action, and (2) the voltage that will keep the tube running once it has been set in action; (a) the curve of the rectifier current at the moment the tube lights up, but which will overheat the tube if left on for any length of time; (b) the curve of the current from the same machine, upon which the tube will run more or less regularly, and without heating unduly; (c) (d) represent induction coil curves having a mean current value corresponding to (a) and (b).

When the tube is running on a current such as (b), it will be seen that there is a comparatively long period of rest, and there is a liability of the electrons falling into that same state that occurs when it has been at rest for some time, and requires an impulse as at (a) to set it

going again. This is, to my mind, what happens when we use any closed magnetic circuit transformer for passing small currents through an X-ray tube. With an induction coil, on the other hand, owing to the high initial impulse, even with a low mean current value, there is never any chance for the electrons refusing to come into action, and the tube will work steadily with any current required in practical work. This I consider an important advantage of the coil over the rectifier.

Now let us consider what were the disadvantages of the coil that led to the introduction of the rectifier. They appear to be mainly the low secondary output, and the presence of an inconvenient amount of inverse current; we might also add the presence of the interrupter, which has its inherent defects.

The high-tension rectifier is free from all these defects, inasmuch as it gives a current entirely free from inverse waves, and, in a quantity greater than any tube we know of at present, will withstand for more than a very few seconds. Moreover, it is self-contained and complete. These are great advantages, and if there were no other considerations to take into account, they would decide the choice very quickly. The matter, however, is not so simple as this. Most of us cannot afford the space or the money (or perhaps both) to have a coil as well as a rectifier, and we must select the one that best suits our requirements, our space, and our pockets. However great the advantages of the rectifier are, we have to take into consideration its disadvantages. We find that it is very bulky, it costs from two to three times that of a coil outfit, it is inconveniently noisy, even when enclosed, and the tubes work irregularly on small currents, such as we have to use for much of our work. Further, if we try to get rid of the noise by placing the machine in an adjoining room, the irregularity of the working of the tubes is greatly increased.

We have also to consider if, though the advantages over the coil were such as I have stated a year or two ago, the most modern and up-to-date coil outfit possesses the same disadvantages as its forerunners. I think I can convince you that it does not. I had an opportunity of testing a new coil recently, which gave some noteworthy results. Across an 8-in. air-gap, and without a valve of any kind, it passed a current of 52 ma., and this with less than 10 amp. in the primary coil. Through a tube of medium hardness it easily passed 12 ma. In both cases a mercury break was used. With such currents very rapid exposures are possible, and if we further employ one of the new intensifying screens which give so little "grain" as to be almost

quite unobjectionable, good work on the thickest parts of the body can be done in as short exposures as are likely to be needed for all ordinary purposes. Working under these conditions, Mr. Thurstan Holland has shown me an excellent radiograph of a renal calculus after an exposure of three-quarters of a second!

Only to-day, in doing an X-ray examination of the urinary system in a lady of large physique all my plates were over-exposed, the longest exposure of the series being twenty seconds, without a screen of any kind. This latter is, I know, nothing phenomenal, but it does show that the modern induction coil is a very different thing from what we were accustomed to a few years, or even months, ago. It should be noted that I am speaking of the use of a mercury, and not an electrolytic break. With regard to the latter form of break, I may say that I have given up the use of it almost entirely. With the high primary voltage we now have to work with, the inverse current is too high for comfortable working as a matter of routine. Tubes vary rapidly and have a short life. This would not signify very much if it were always possible to get a highly satisfactory tube at a moment's notice. This, however, is seldom the case, and our most satisfactory tubes are those we have had in careful use for a considerable time and have become "seasoned," so to speak, until they are capable of safely carrying the currents required for heavy work.

These remarks apply all the more to the multi-anode type with the aid of which huge currents are possible in the primary and successful radiographs secured with very short exposures. I know that some workers are using this arrangement and getting what they describe as the most excellent and satisfactory results. At the same time, so far as I can learn, this arrangement has not been gaining ground to any great extent, and there is a strong tendency to hark back to the mercury interrupter in spite of all its defects. It would seem that our faithfulness to our "first love" is in a fair way to get its due reward. The improvements that have been made in induction coils and mercury breaks are very great indeed; so great are they, that the high-tension rectifier no longer holds the commanding position it did in regard to heavy and rapid work.

There is much more that I might say on this important and interesting subject, but I think I have said enough for the patience of this meeting, as well as enough to raise some points for discussion. If these rather disjointed remarks of mine achieve this result, their purpose will have been attained.

DISCUSSION.

Dr. BARCLAY (Manchester): The experience of six months' work with an American-made "Snook" as compared with coils such as Newton's Instanta and Watson's Intensified, leads one to state the case thus: The "Snook" current differs greatly from that of a coil. It tends to leakage by brush discharge, and this brush discharge, unlike the brush from a coil, tends to char and burn even good insulators. The heating effect on the anticathode is far greater than equal quantities of current from the coil, therefore light anode tubes are useless, and this was our primary mistake that led to the burial of several old and trusty friends in the first week of misfortune. Having found tubes that will stand the current—and we now have no difficulty in this respect—I will state my impressions of the "Snook" as used for ordinary purposes just as one has used a coil for the last three years, making kidney exposures in from five to thirty seconds—i.e., in rapid radiography, screen work and treatment, and will later speak of instantaneous work.

The screen image is beautifully steady, but the lighting of the screen is poor and very deficient in contrast. To get even a fair view of a stomach one has to put about 3 ma. to 4 ma. through a medium hard tube, whereas 2 ma. or less would suffice with the coil. The whole screen effect is grey and gives the impression that the tube is too hard, yet if the tube is softened the rays cease to give any image. Now it is a curious thing that although the screen effect is so poor, yet a photographic plate is affected far more than one would expect from the picture: a radiograph can be taken of the screen picture in half the time that a similar picture from a coil would take to impress itself on the plate; in other words (other factors being equal), I find that the "Snook X-rays," although they produce less fluorescence of the screen, have quite as great a chemical action on the plate. In treatment the "Snook" is possibly preferable to the coil. The tubes, when once seasoned, stand the work well, and we find that our Sabouraud dose is given in two-thirds the time we used to require with the coil.

To sum up: The "Snook" is inferior to the coil for screening, but is preferable for treatment; while for ordinary radiography it is better than the coil. I would like to qualify this last statement. I do not think the best "Snook" plates are better than "coil" plates; I do not think there is anything to choose between the two; but the "Snook" gives more even negatives on the whole and there is this feature always present—plentiful detail in the soft parts. Another point in these plates is that they are evenly exposed all over; consequently in a kidney radiograph, for instance, one does not find the lower part of the plate over-developed as compared with the upper part.

So far I have been speaking of only about one-third of the output of the "Snook" as a maximum, and this is the way in which Dr. Bythell and I have chiefly used our machine. Our early experience of using heavy currents with difficult cases was disastrous, but this was due in part at least to the tubes being quite inadequate to such work as the "Snook" throws on them. Given

a tube such as the "moment" when it is seasoned it is quite easy to take radiographs of children, &c., in fractions of a second, using only two-thirds of the power—i.e., it gives results with two-thirds its power that are on a par with my experience of the 3-pole Wehnelt break and a good coil, and so far the wear on tubes is not noticeable. But beyond this we have seldom gone except in one or two thin kidney patients where two or three flashes have sufficed: it is purely a question of waiting till such time as we can trust our tubes to withstand the terrific bombardment required to radiograph difficult cases in a fraction of a second. Am I going to get any gain by effecting my exposure in a fraction of a second that will in any way pay me for my possible broken tube and frightened patient? The answer is plainly "No," except in very exceptional cases, and when these turn up I will try it. While the work needs that my radiographs should be taken during a pause of respiration, I will take full advantage of that cessation of movement; but when the call comes to catch a picture of the heart in its very action, I feel that the "Snook" will supply the power, and I know that I shall succeed; but at present I am content to develop along the lines of rapid exposures (there is plenty to learn yet), and when I know more of the rapid method with the "Snook" I shall be ready for the instantaneous method, which is at present unnecessary, except sometimes for children.

As to Protection.—It is extremely difficult to protect oneself effectually from the rays when doing rapid radiography, and practically impossible in instantaneous work unless one so cumbers the room with protective devices that real work is impossible. It may be my fancy, but I think the rays from the Snook-operated tube penetrate my protective boxes, which are all lined by Müller rubber, more than the ordinary rays do; and, while speaking of this matter, I might mention that, although one is said to be safe behind the anticathode of the tube, it is a fact that whether with the "Snook," which has no reverse current, or with the coil, I can plainly see the bones of my hand within two feet of the back of the tube, and can see the outline of my hand on the screen at the farthest limit of the room, whether in front of or behind the anticathode.

For all-round work the "Snook" is, in my opinion, preferable to the coil; its failings are that it is noisy and that the screen effect is comparatively poor, both of which failings can be overcome, I am certain, as also can a great deal of the heating effect on the tube. Its advantages are that it affords as much power as we are likely to want at any time—its present limitation is that the power is too great for the tubes.

Since writing the above we have tried an experiment which has proved extremely successful—i.e., placing a spark-gap of $\frac{3}{4}$ in. in the circuit on each terminal. This has the effect of cutting out the lower voltage current—i.e., until the voltage has risen sufficiently high to overcome the spark-gaps no current passes through the tube, which results in a period of rest for the tube, followed by a sharp blow as the current jumps the spark-gaps, and the tube is not bombarded by a continuous series of waves, but by an intermittent series of blows. Practically this very simple device makes all

the difference for screening purposes. We have now perfect illumination of the screen, and whereas before this change we required 5 ma. or 6 ma. to get even a very poor stomach examination, a few days back we obtained a perfect view with a current of 1 ma. to 1½ ma. in a man of average size. The next change is the result on the tube. Instead of finding the glass bowl almost too hot to touch after heavy work, we find it is almost cool—not as hot as after use with a coil. It seems to have no effect on the radiographic exposures or on the length of time taken to obtain the Sabouraud dose, and this slight modification of this extremely powerful apparatus makes it, to my mind, far superior to the coil for all purposes. The noise can be overcome by boxing in, and now that we have done this we have absolutely no fault to find with the machine.



(*a*) Snook current; (*b*) as modified by spark-gap.

Dr. BYTHELL (Manchester): During the six months we have been using the "Snook" high-tension transformer we have taken some 400 radiograms, and the general impression I have formed is that for *radiography* it is a very distinct improvement upon the induction coil. In the first place, I consider that the plates are clearer, and contain greater contrast, thus showing less evidence of secondary rays. The majority of our exposures have been taken, I may say, with only about a quarter to a third of the full strength of the current, about 10 ma. passing through the tube. This gives us kidney and bladder radiographs in from 10 to 30 seconds. In others we have obtained beautifully exposed plates of these regions in 3 to 5 seconds by using more current. In both cases I am of opinion that the results are more certain than with the coil, and that the percentage of failures is less. We have made little of the machine as a means of obtaining instantaneous radiographs, but such as I have tried have been satisfactory. Thus I recently obtained a particularly good plate of a vesical calculus in a well-built boy of 13 with a snap-shot exposure. As regards the *effect upon the tubes*, our first experience—like that of most people—was unfortunate. There is no doubt, however, that this was our own fault, owing to attempting to pass enormous currents through tubes which were either unsuitably constructed for the purpose or were not at the time in good working order. For the last three or four months, on the other hand, we have done practically all our work with a couple of large Moment tubes, and have had no tube mortality whatever that could be attributed in any way to the apparatus. I find, moreover, that with such exposures as I have detailed, the vacuum of the tubes does not alter to any appreciable extent, a fact which I think must be explained by the absence of inverse current. One point upon which I am not yet quite convinced is the relative suitability of the

"Snook" for *screen examinations*. The perfect steadiness of the illumination is certainly a very great advantage, but up to the present I have found it necessary to use a heavier current than would be required with a coil in order to obtain an equal degree of illumination. It is possible, however, that this may be due to the fact that the tubes I have been using for the purpose have been on the soft side, and I do not think it safe to draw conclusions from such a short experience. Lastly, I may say that we have done a certain amount of treatment with the "Snook," and that we find we can give a full Sabouraud dose in about ten minutes without affecting the vacuum of the tube. The conclusions I have come to are: (1) The "Snook" is quite as good as, if not better than, the coil for comparatively slow exposures or for treatment; (2) It is, in addition, better suited for instantaneous work; (3) If suitable tubes are used, and these are carefully "educated" to the heavy currents, they stand the work extremely well; (4) The simplicity of the machine and the absence of interrupters are great advantages, there being fewer factors to go wrong; (5) It is scientifically more sound in theory.

Mr. A. D. REID said he gathered that some of Dr. Morton's remarks might have reference to his (Mr. Reid's) experience. He believed they were in the transition state, and that it was difficult to advise anybody which was the better: both had their defects. He would recommend anybody not to get the first of any kind which had been made. He had done that, and, perhaps owing to his desire to get the machine quickly, it was made to wrong specifications. He had decided to try the effect of getting rid of the noise by putting the machine in a cellar beneath his room. The secondary wires were brought up through the floor running in metal cylinders through vulcanite rods. The leads were then taken to a spreader and carried across the room to a convenient situation. That gave him 5 yards or 6 yards of secondary wiring. The result was disastrous, and he destroyed many tubes in the early stage. There were high-frequency oscillations which had to be disposed of, and it was not until water resistances were inserted that that was got rid of. Still the breakage of tubes continued. He cut down the metal tubes to a minimum and shortened the wires to 3 yards. He had had much better results since, and very little trouble. If three or four stereos were wanted in one case, the transformer increased the pace at which one could work, and there was much less trouble from slight movements of the patient. He agreed with Dr. Barclay about the screenwork; the illumination from that was very disappointing, and he was glad to hear of the spark-gap, which he had not tried, but would do so. His basement was very damp, and he had only been able to overcome that by covering the machine and the wires with vaseline. It was now a delightful instrument to work with, and the tube would last for months. From his experience of the transformer, his advice would be not to try very rapid exposures at first. At first his plates were hopelessly over exposed; one should have as short a secondary as possible. A transformer to work in the consulting room was hopeless unless it was enclosed; if a noisy machine were started when examining a nervous child there would be great difficulty in keeping the

child quiet. The rapidity was so great that one could do everything necessary in this case with an absolute snap. If he were to lose his present apparatus he believed he would take a week to make up his mind what kind to have next. He would like to see some work with the newer coils. He agreed with Dr. Morton that the electrolytic current was unsatisfactory, because there was so much trouble with the inverse current. He had given up that work for this reason. With a three-pole interrupter he never could get efficient radiography, and he discarded that for the machine which he had at present.

Dr. FOWLER (Bournemouth) said he had had experience of all the forms of producers which Dr. Morton mentioned, and he thought much depended on what current there was in the main. He who had a 240-volt direct current was much better off than the man who had an alternating current to deal with. In Bournemouth there was an alternating current of a periodicity of 100. To anyone who was going to have only one instrument he would not recommend a high-tension rotary transformer. The noise of a transformer in a small room was prohibitive. He was using a hard tube and filters for most cases; a high-tension rotary transformer was not as good on a hard tube as was a coil. He understood that Dr. Barclay put in a $\frac{3}{4}$-in. spark-gap at each end of the tube. There was already a $\frac{1}{2}$-in. spark-gap in the rotary rectifier, so an increase in the spark-gap of the rectifier should have the same effect; a spark-gap at each end of the tube was likely to perforate the tube.

Mr. S. GILBERT SCOTT said that in France he found no high-tension transformers of Dr. Snook's type in use at any of the hospitals he visited. In Germany, in Berlin and Cologne he found one hospital in each place using this transformer for all radiographic work. These were the only hospitals he found abroad doing regular work with it, and although a number of them had it installed they preferred to use their coils. One well-known radiographer in Hamburg had in his private room no fewer than three Snook transformers (one small one for treatment) and three coils, but he preferred to use his coil and long exposures for general work, only using his transformer for chest and bismuth cases with exposure of one-fifth of a second with intensifying screen.

Dr. PIRIE said that before deciding whether to get a "Snook" or a coil one should remember the screens, which reduced the exposure to one-tenth and gave little graining. The graining depended largely on the length of exposure. In order to X-ray the foot or hand of a child, the exposure could be done as fast as one could switch on and off, when using a screen. The need for anæsthetics was thus done away with. He had thus X-rayed restless children with the old-fashioned coil and the old-fashioned brake.

Dr. MORTON, in reply, said he was very pleased that the subject had attracted so much attention. It was as important as it was fascinating. Regarding the remarks by Dr. Barclay and Dr. Bythell, he thought he was right in saying that they had not had experience of the new induction coils he had referred to, and which had been brought out only during the last few

months. These, used in conjunction with the new intensifying screens, enabled one to do the fastest work with the smallest amount of strain on the X-ray tube. Personally, he found from three to five seconds a sufficiently long exposure for a renal case, the current through the tube being less than 10 ma. The tube he now used was of American make, and easily took this current for thirty seconds and very likely longer, but he did not care to risk damaging a good tube for the sake of curiosity. The great feature of the new coils was not only their large output of direct current, but also the almost entire absence of inverse current. The new screens reduced the exposure to one-tenth, with a remarkable effect on the life of the tubes, the short exposure apparently affecting them but very slightly. In a prolonged exposure it was the latter part of it that told so heavily against the tube, and the bombardment became very severe after the tube got overheated. With regard to results, all he could say was that the improvement in his X-ray negatives since using the new coil was very marked, and he attributed this to the absence of inverse current, and also to the peculiar characteristics of the induction-coil current; he considered the latter more suitable for exciting X-ray tubes than the long, sweeping voltage curve of the high-tension rectifying transformer with a closed magnetic circuit. The scheme suggested and found satisfactory by Dr. Barclay for correcting some of the faults of the high-tension rectifier was very interesting, because in his opinion it had the effect of making the current more like that of the induction coil. He believed that the makers of the rectifiers would be well advised to make the collecting blades much shorter, so as to use only a small part of the current curve at its highest point. If an oscilloscope were placed in circuit with an X-ray tube it would be found that no matter what current was passed through it, the individual impulses were very short—so short that, however fast the mirror was rotated, there was little or no broadening of the reflected glow. There would thus seem to be no advantage in supplying the tube with other than very short electrical impulses, but these might be of any desired intensity and frequency. He was quite aware that some radiographers had done good work with a coil and electrolytic brake, but with the high-voltage current now supplied it was noticeable that it was not so popular. The inverse current was excessive with so high a primary voltage, and the effect on the tubes was very deleterious, often making it unduly hard after a single exposure. With a single-pole Wehnelt brake it was bad enough, but with a three-pole arrangement it was at least three times worse. With regard to the instrument mentioned by Dr. Fowler, he believed the result alluded to would be obtained if the manufacturers would use shorter collecting blades, as already referred to.

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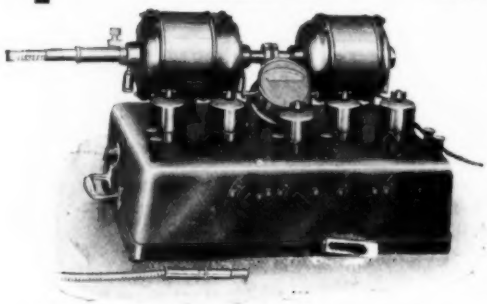
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